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CEREBRAL DETERMINANTS OF AUDITORY LOCALIZATION

By EDWARD GIRDEN, Brooklyn College

With the increasing interest manifested in the analysis of the cerebral determinants of psychological processes, considerable progress has been made in the study of the visual mechanisms. Other systems, as for example the auditory, have been relatively neglected; with but few exceptions, the majority of the studies in this field have had as their objective the analysis of the peripheral (cochlear) functions. This report represents the results of the first of a series of investigations of the relationship between cerebral mechanisms and hearing.

Having completely transected the corpus callosum in a dog, Bikov and Speransky then conditioned the animal to salivate to the sound of a whistle of 1500 ~ presented from the dog's right side.[1] They were unable, however, subsequently to train the animal not to respond to the same stimulus sounded from its left side. That is, the dog salivated to this whistle regardless of its locus; it could not discriminate between left and right. Upon the basis of this experiment, Pavlov concluded that "a differentiation of the direction of a sound required a united activity of both hemispheres."[2]

When a sound is presented from the left or right side, some inequality is produced in the peripheral acoustical mechanism (cochleas) which is transmitted to the cortical system. Since the corpus callosum functions primarily in uniting the cortices of the two cerebral hemispheres, it would

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[1] K. M. Bikov and A. S. Speransky, Observations of dogs after section of the corpus callosum, quoted from I. P. Pavlov, *Conditioned Reflexes*, 1927, 150.

[2] I. P. Pavlov, *Conditioned Reflexes*, 1927, 150.

seem logical to conclude, as did Pavlov, that the united action of the two hemispheres is required in the normal discrimination between left and right. This is consistent with the belief that hearing is crossed: all impulses from one cochlea (left or right) terminate in the opposite cortex (right or left). Since both ears are normally involved in the correct dis-

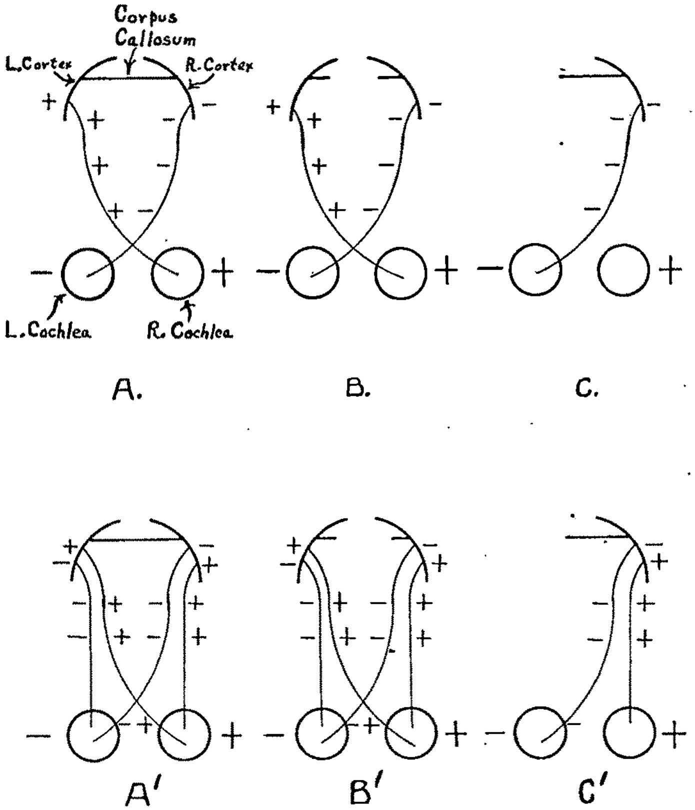

FIG. 1. DIAGRAMS OF THE AUDITORY SYSTEM

In the upper figures, A, B and C, hearing is represented as a completely crossed system, whereas in the lower diagrams, A′, B′ and C′, uncrossed as well as crossed components are indicated. The intervening synaptic levels between the peripheral (cochlear) and central (cortical) levels are omitted. The normal situation is posited in A and A′, transection of the corpus callosum in B and B′, and left hemi-decortication in C and C′. The sound is represented as coming from the animal's right side. The plus (+) and minus (—) signs represent the differential effect produced at the cochleas in terms of the physical qualities of the stimulus: phase, time, and intensity differences. Thus, the sound reaches the right ear first (as represented here), is more intense at that ear, etc., than at the left ear. See text for discussion.

crimination of laterally produced sounds, transection of the corpus callosum would prevent the integration at the cortical level of the inequality produced at the cochleas. It would also seem to follow that a similar inability would occur with the extirpation of one cerebral cortex, as is produced by the destruction of one cochlea. This is indicated in Fig. 1, diagrams A, B, and C.

There is reason to believe, however, that the auditory system is not so simple. According to Kreidl,[3] hearing involves uncrossed as well as crossed neural components. Recent experimental evidence substantiates this view, indicating that in the dog[4] and the cat[5] the crossed and uncrossed components are approximately equal in acoustical value. Therefore, any acoustical inequality produced at the cochleas should have a representation on each cortex. From this logic, it would seem that the (left or right) hemidecorticate animal should be capable of localizing laterally produced sounds. Unless other clinical symptoms are produced by the operation, it should be possible to develop this discrimination in an animal in which the corpus callosum has been severed. This is illustrated in Fig. 1, diagrams A′, B′, and C′. As in the previous case, destruction of one cochlea would eliminate left-right discrimination.

A possible explanation of this contradiction may lie in the unreliability of the salivary (food motivation) conditioned response (CR) in studies involving the extirpation technique. Zeliony,[6] using this method, could not procure conditioned reflexes in a totally decorticate dog. Using the motor CR technique, Culler[7] produced evidence of both visual and auditory conditioning in a completely decorticate dog. These results were confirmed in the study of a second such animal[8] in which auditory, tactile, and thermal CR were developed as well as a discrimination between two auditory stimuli. In the light of these facts, the nature of the cerebral determinants of the L-R habit was studied by the motor conditioning method.

Method. A description of the technique of establishing motor conditioned responses in dogs has been reported elsewhere,[9] and the modifications introduced in the study of the L-R habit have recently been described.[10] Usually the animal is first trained to flex forepaw to an auditory stimulus sounded from in front of the apparatus or from a position on the animal's right side. The L-R habit is successively developed for buzzers and then pure tones, so that the dog always flexes forepaw when the auditory stimulus is presented from its right side, but not when sounded from its left side. The sound-sources were placed a few inches from, and on line with, the left and right ears respectively. The buzzers were 6-volt bells with the hammers removed, and the receivers were Western Electric 705-A instruments. The pure tones, emitted through these receivers, were produced by means of a Western Electric 6-A audiometer having a frequency range from 128 to 9747 ~.

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[3] A. Kreidl, Zur Frage der sekundären Hörbahnen, *Monatsch. f. Ohrenhk.* 48, 1914, 1-14.

[4] F. A. Mettler, G. Finch, E. Girden and E. Culler, Acoustic value of the several components of the auditory pathway, *Brain,* 57, 1934, 475-483.

[5] W. J. Brogden, E. Girden, F. A. Mettler and E. Culler, Acoustic value of the several components of the auditory system in cats, *Amer. J. Physiol.,* 116, 1936, 252-261.

[6] G. Zeliony, Effets de l'ablation des hémisphéres cérébraux, *Rev. de méd.,* 46, 1929, 191-214.

[7] E. A. Culler and F. A. Mettler, Conditioned behavior in a decorticate dog, *J. Comp. Psychol.,* 18, 1934, 291-303.

[8] E. Girden, F. A. Mettler, G. Finch and E. Culler, Conditioned responses in a decorticate dog to acoustic, thermal and tactile stimulation, *ibid.,* 21, 1936, 367-385.

During critical tests of the L-R habit, the dog's head was held in a relatively fixed position by wooden neck-pieces.[11] Since the two buzzers were not equivalent, either in terms of tonal quality or intensity, they were switched (the left placed on the right side, and vice versa) not only on successive test-days, but at least once during each critical test. An analogous procedure was followed with the use of pure tones. Since the receivers were a matched pair, there was no need to switch them during the tests. On each critical test, however, the intensity was varied in chance order: sometimes the sound produced at the left side (LS) was louder, while on other trials the sound produced at the right side (RS) was louder. Successive presentations of the RS and LS stimuli, whether with buzzers or pure tones, were arranged in chance order.

Problems. The following experimental situations were studied. (1) Effect upon the L-R habit of complete transection of the corpus callosum (*Dog C*). (2) Development of the L-R habit after complete left hemidecortication (*Dog 2*). (3) Effect upon the L-R habit of extirpation of the temporal lobe and considerable adjacent parietal and occipital cortex of the right hemisphere (*Dog D*). (4) Effect upon the L-R habit of (a) bilateral temporal lobectomy on one operation (*Dog 4*); and (b) removal of the left and right temporal lobes in successive operations (*Dog 6*). After the critical tests were completed, the animals were dispatched and the brains fixed in formalin. Macroscopic examinations[12] were made to determine the extent of the lesions, which are represented here diagrammatically or by actual photographs. When necessary, microscopic (histological) examinations were initiated, and are now being completed.

Results. The results of the experiments on the different dogs may be summarized briefly as follows.

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[9] E. Culler, G. Finch, E. Girden and W. Brogden, Measurements of acuity by the conditioned response technique, *J. Gen. Psychol.*, 12, 1935, 223-227.

[10] E. Girden, Conditioning and problem solving behavior, this JOURNAL, 51, 1938.

[11] Cf. *Motor Conditioning in Dogs* (Film), C. H. Stoelting Co., Chicago.

[12] Dr. F. A. Mettler of the University of Georgia Medical School was kind enough to assist in the operation and make the macroscopic postmortem examination upon Dog C.

Dog C. The L-R habit for buzzers was established, with Dog C, June 24–July 12, 1935 (9 test-periods). After the operation on July 14, the animal showed severe behavioral disturbances. There would be periods of extreme excitement accompanied by spasmodic contractions of the muscles, particularly of the face. These attacks could be produced by tactile or intense auditory stimuli. Erect posture was maintained only momentarily. Some improvement occurred by the following day, and testing was initiated on July 16. By the end of the third test, the animal's condition had become progressively worse, and he was then dispatched. The postmortem examination showed that considerable blood had clotted along the medial surface of the brain, the pressure of which probably contributed as a cause of the clinical picture. The corpus callosum had been completely transected. In performing this operation, the approach had been made by retraction of the right hemisphere which produced some damage to the parietal cortex, as shown in Fig. 2. This destruction involved none of the primary auditory cortex, the sylvian gyrus (areas 78 and 78b[13]).

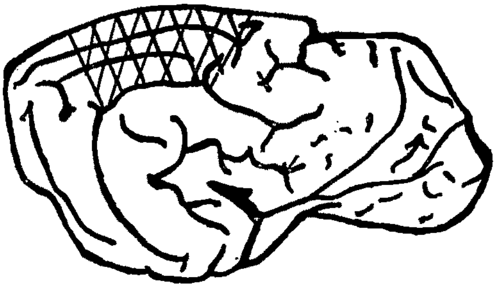

FIG. 2. DIAGRAM OF THE BRAIN OF DOG C SHOWING EXTENT OF INJURY
The crossed area represents the cortex injured on the right hemisphere in the process of transectiong the corpus callosum.

TABLE I

EFFECT OF TRANSECTION OF THE CORPUS CALLOSUM OF DOG C UPON ITS LEFT-RIGHT DISCRIMINATION

The numerators of the fractions represent the number of CR; the denominators, the tota number of trials. Responses to LS are errors, and the total scores consist of the number of CR to RS plus the number of times no response was made to LS. The successive presentations of the stimuli on RS and LS were made in chance order and include at least one shift of the buzzers on every test. When pure tones were used (see Tables III–V) the intensity of the successive presentations was also varied in chance order. All of the pre-operative data of this animal have been reported in our earlier study.

Period	Date	RS	LS	Total Score
Pre-operative	7/ 9/35	8/8	2/18	24/26
	12	21/21	2/25	45/46
Post-operative	7/16/35	0/10	0/10	0/20
	17	7/18	0/6	7/24
	18	17/21	10/37	44/58

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[13] Cf. J. W. Papez (*Comparative Neurology,* 1929, 1-518) for the procedure used here in identifying the cortical areas.

As indicated in Table I, the third and final post-operative test was the most satisfactory: 4 errors were made with RS, and 10 errors made with LS (since correct score to LS was no response). In other words, 81% correct responses were made to RS and 27% errors made to LS (July 18th). The difference, 54% (81% — 27%), is not attributable to chance. Statistically,[14] the ratio of this difference to its standard error, ± .112, is more than 4.5, and therefore quite significant. This analysis is more than substantiated by the observations of the animal made in the test-situation. It might be argued that the CR and the L-R habit were actually being relearned. In the light of experience with many animals in this situation, this is but a possibility. No animal in the first stages of conditioning develops the L-R habit to the extent of making satisfactory scores with a chance order presentation of RS and LS stimuli, combined with an interchanging of the sound sources (see the discussion of Dog 4 below). There were also other criteria indicating the presence of the pre-operative organized L-R habit, such as appropriate incipient eye movements to the stimuli,[15] which only the well trained animal possesses. Some of the errors with LS are definitely accounted for in terms of periodic excitement in the test-situation, in part due to the animal's general heightened responsiveness to auditory and tactual stimulation. On those trials during which the animal was quiet, appropriate localization was made. If the animal was excited during the presentation of the LS stimulus (which could not always be anticipated) there was a tendency to flex forepaw.

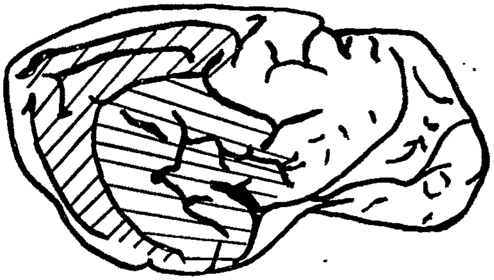

FIG. 3. DIAGRAM OF THE BRAIN OF DOG D SHOWING EXTENT OF EXTIRPATED AND NON-FUNCTIONAL AREAS

Diagonals from left to right (\\\\) indicate the cortical areas which were extirpated, while the diagonals from right to left (////) indicate those areas which were rendered non-functional by being undercut.

Dog D. The L-R habit for buzzers was established, for Dog D, June 17–July 19, 1936 (25 test-periods). The animal was operated on July 20, and dispatched on July 23, 4 P.M. The postmortem examination showed that the following gyri of the right hemisphere had been extirpated: ectosylvian (68, 68b and 38b), sylvian (78 and 78b), polar (74) and insula (64). The following gyri were undercut and therefore non-functional: middle and posterior suprasylvanian gyri (48, 50 and 70), ectolateral (52), and part of the lateral (42). A macroscopic diagram of these lesions is presented in Fig. 3. The last pre-operative and the post-operative tests are summarized in Table II. A little uncertainty was noticed on the first post-operative test, and a little general excitement appeared on the third test. The second test, on July 22nd, was completely satisfactory: 100% with the RS buzzer and 0% response whenever the LS buzzer was sounded.

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[14] Cf. H. L. Rietz, *Handbook of Mathematical Statistics,* 1924, 77.

[15] Cf. Girden, *op. cit.*

TABLE II
EFFECT OF RIGHT TEMPORAL LOBECTOMY OF DOG D UPON THE L-R HABIT
(For an explanation see Table I and the text.)

Period	Date	RS	LS	Total Score
Pre-operative	7/17/36	10/10	0/10	20/20 (100%)
	19	10/10	0/10	20/20 (100%)
Post-operative	7/21/36	20/22	5/22	37/44 (82%)
	22	10/10	0/10	20/20 (100%)
	23	12/13	2/15	25/28 (89%)

Dog 2. Dog 2 was operated at Urbana, Ill., Sept. 11, 1937, and recovered satisfactorily. (As can be seen in Fig. 4, the cortex of the left cerebral hemisphere had been extirpated almost completely, a slight bit of the medial-frontal cortex remaining intact.) Testing was initiated in Brooklyn, N.Y., Nov. 6, 1937, the animal first serving in some preliminary experiments not related to the present report. Training for this problem was begun on Nov. 24, and 90% level of left forepaw CR to buzzer sounded on dog's right side, was attained in two days (50 trials). Training with LS and RS stimuli was continued from Dec. 13–29 (14 test-periods). The first critical tests of the L-R habit were then tried, and the animal made close to perfect scores; that is, always responding to RS but inhibiting paw flexion to LS. Texts were continued until Jan. 6, 1938, at which time pure tones were introduced. A good score was made on this first test with three frequencies, but some difficulty occurred on the following day. After two subsequent tests with the buzzers, the pure tones were once again used. During these next few tests, the dog behaved in a fashion which can only be best described as neurotic. Whether this was due to the poor health of the animal at this time, or the use of test frequencies above 2048 ∼, is not clear. For example, the dog would reach towards and attempt to *eat* (attack?) the RS receiver. During some trials with this stimulus, spasmodic flexions of hind limbs or twitchings of the head appeared; again, these responses could be modified and at times completely eliminated by the immediate cessation of RS and the introduction of the LS tone. The LS receiver was *never attacked*. In order to complete the tests with the lower pitches, the frequencies above 2048 ∼ were then eliminated.

TABLE III
DEVELOPMENT OF L-R HABIT AFTER COMPLETE LEFT HEMI-DECORTICATION OF DOG 2
(For an explanation see Table I and the text.)

Date	RS stimuli				LS stimuli			
	256∼	512∼	1024∼	2048∼	256∼	512∼	1024∼	0248∼
2/22/38	8/10	4/4	4/4	4/4	0/4	0/6	0/3	0/4
24	6/7	6/6	3/3	3/3	0/3	0/4	0/3	0/4
25	5/5	3/3	3/3	5/5	0/6	0/2	0/4	0/5
26	5/5	5/5	7/7	5/5	0/4	0/4	0/5	0/5

After a few days the animal improved considerably, physically and in the elimination of neurotic symptoms; concomitantly, the tests with the lower frequencies became more reliable. The animal had no difficulty in correctly localizing the four test-frequencies used: 256, 512, 1024 and 2048 ~ respectively. The scores of the last four tests are given in Table III. These scores, as in the other tables, represent the totals for each frequency, and do not indicate the order of presentation or the variations of intensity used. For example, on Jan. 25 (Table III), the test was initiated with a pitch of 2048 ~, and proceeded as follows: (1) RS (80 db.) 2 trials; (2)

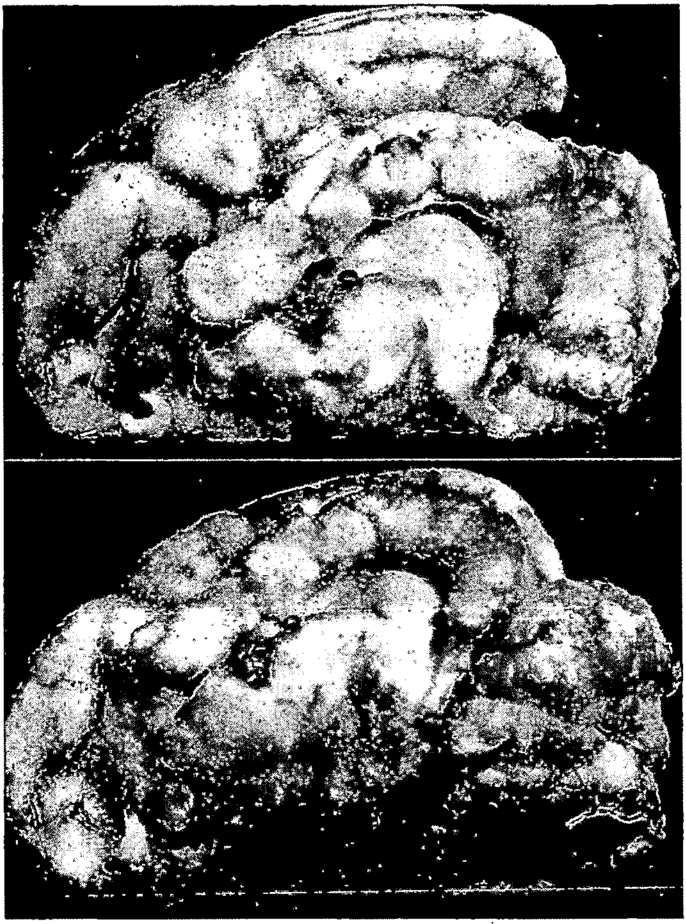

FIG. 4. LATERAL VIEWS OF THE LEFT HEMISPHERE OF DOG 2
These photographs show that all the cortex, except a slight piece of the frontal-medial surface, was ablated.

LS (85 db.) 1 trial; (75 db.) 2 trials; (3) RS (75 db.) 3 trials; (4) LS (80 db.) 2 trials. The next frequency tried was 1024 ~ (LS), and the test proceeded in a similarly treated chance-order fashion. The animal was dispatched Jan. 28, 1938.

Dog 4. Training of Dog 4 was started Nov. 6, 1937. Tests with RS and LS buzzers were started Nov. 19, and pure tones were introduced Dec. 25. The CR was established very quickly. Due to a tendency to overactivity, which resulted in erratic performances, the training with the higher frequencies progressed slowly. Satisfactory tests were finally procured in February 1938. The animal was operated upon Feb. 19th, and the auditory areas of both the left and right hemispheres were removed in a single stage. The animal was depressed for several days afterwards, requiring artificial feeding. By Feb. 22, the dog had recovered sufficiently enough

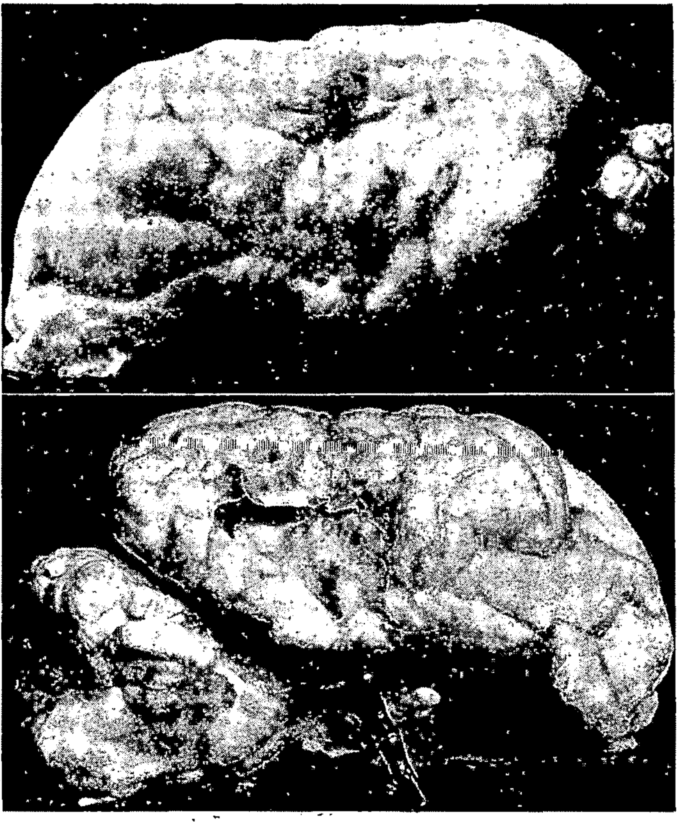

FIG. 5. VIEWS OF THE CORTEX OF DOG 4
These photographs show the areas extirpated from both temporal lobes in a single operation.

to eliminate the force-feeding, and was able to walk around. Cortical blindness and deafness were present. Pupillary response to strong light was weak and sluggish. Food was located by muzzle-sensibility, and objects were not avoided. The normal behavior to usual sounds (feeding of other animals by the attendant, approach of the experimenter to its cage), was absent. Testing was initiated Feb. 25, and the animal was dispatched March 24, 1938.

As can be seen from an examination of Fig. 5, the extirpation upon the left hemisphere was limited to the cortex, but on the right hemisphere the ablation was somewhat deeper, the exact extent of which cannot be determined until the histological examination is completed. The changes produced in localization habit cannot be attributed to relatively deeper lesions produced in right hemisphere. These lesions are far from being as deep or as extensive as those produced in Dog 2 (cf. Fig. 4).

TABLE IV

EFFECT OF SIMULTANEOUS EXTIRPATION OF LEFT AND RIGHT TEMPORAL LOBES OF DOG 4 UPON THE L-R HABIT

(For an explanation see Table I and the text.)

Period	Date	Side	Stimul								
			Buzzer	128~	256~	512~	1024~	2048~	4096~	8192~	9747~
Pre-operative	2/14/38	RS	—	2/2	1/1	2/2	2/2	4/4	1/1	3/3	3/3
	17		—	3/3	2/2	3/3	2/2	5/5	3/3	2/2	3/3
	2/14/38	LS	—	0/2	0/2	0/5	0/3	0/5	2/5	0/3	2/11
	17		—	0/3	0/2	0/2	0/2	1/10	0/6	0/3	0/3
Post-operative	2/25/38		4/10	—	0/2	0/3	0/3	0/3	0/3	0/1	—
	26	RS	11/18	—	—	0/3	0/5	0/2	3/4	3/6	—
	3/ 1/38		1/1	—	0/1	0/3	1/7	6/9	4/9	3/6	—
	2/25/38										
	26	LS	5/6	—	0/3	—	—	—	1/1	—	—
	3/ 1/38		—	—	—	—	—	3/4	2/3	2/8	—

As indicated in Table IV, the CR as well as the L-R habit was disrupted by the operation. Subsequent tests show incipient signs of the reappearance of a conditioned response, more rapidly with the buzzer than the pure tones. At the conclusion of the test on Mar. 24 (the last of four tests given after those shown in Table IV) the CR was beginning to appear regularly. The L-R habit was still completely absent, the dog responding as often to the LS as to the RS stimulus. It seems clear that this is evidence for relearning of the CR. If the latter had been simply depressed because of post-operative shock, its reappearance would have been more sudden and rapid (cf. Dogs C and D in contrast to these results).

Dog 6. Left forepaw CR to RS buzzer was started with Dog 6 on Nov. 15, 1937. Regular tests with RS and LS buzzers were begun on Dec. 23, and the animal readily developed the L-R habit. The extirpation upon the left hemisphere was made on Feb. 9, 1938. The auditory area of the right hemisphere was extirpated in two stages: Mar. 3, and Mar. 17 respectively. The amount of cortex ablated from the right hemisphere was less than the total removed in the single operation performed upon the left hemisphere (cf. Fig. 6).

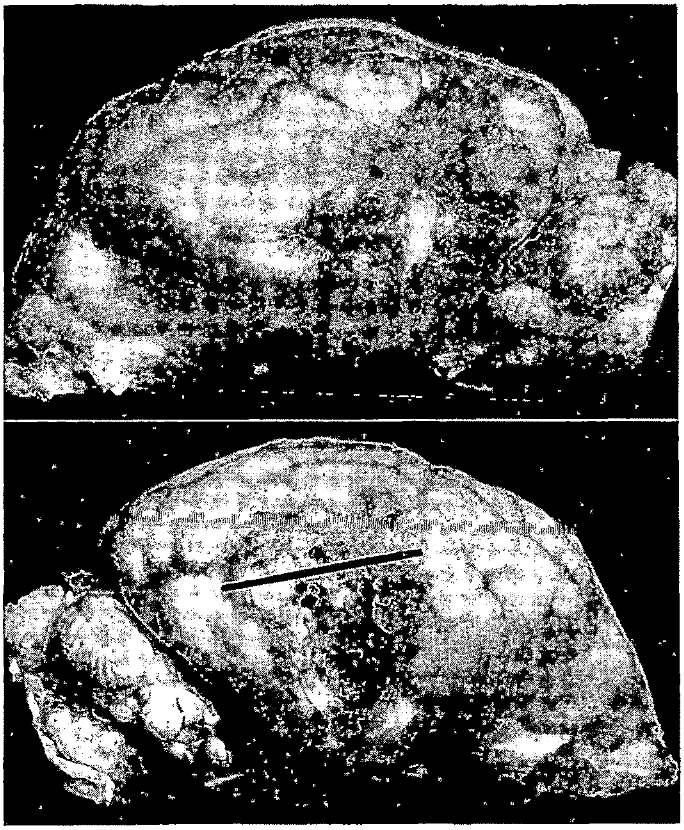

Fig. 6. Views of the Cortex of Dog 6

The cortex of the left hemisphere was extirpated in a single operation. The lesions produced on the right hemisphere were then made in two subsequent operations. The cortex above the black line extirpated first, followed by the removal of the areas shown below the black line.

The effect of the successive extirpations are summarized in Table V. During the pre-operative training all octave frequencies were used at one time or another. Some difficulty was encountered with 2048 and 4896 ~. This resulted in part from an attempt to set up tonal discriminations so that the response was inhibited to 2048 ~ whether it was the RS or LS, and at the same time make 4096 ~ positive so that it was responded to, whether as LS or RS. Immediate results indicated that this procedure would require extensive training, time for which was not available then. Since this procedure tended to disrupt the L-R habit for the other frequencies, it was halted. As a consequence, the L-R habit for 2048 ~ was not on par with the other pitches, and 4096 ~ as a test-frequency was never satisfactory.

The removal of the left auditory cortex (first operation) had the same effect as the removal of the right side in Dog D: the L-R habit persisted undisturbed. The restricted lesions on the right side (second operation) again produced no significant changes. As shown in Table IV, somewhat less than perfect scores were made with 128 and 256 ~ on Mar. 5, but the animal was entirely satisfactory on the other test-frequencies. On the second test (Mar. 14) the score was uniformly perfect. It is safe to conclude, therefore, that this area of the cortex on the right side had no functional significance for the L-R habit. The second entry on the right side (third

TABLE V

EFFECT OF SUCCESSIVE EXTIRPATION OF LEFT AND RIGHT TEMPORAL LOBES OF DOG 6 UPON THE L-R HABIT

(For an explanation see Table I and the text.)

Date	Side stimulated	Stimuli						
		128 ~	256 ~	512 ~	1024 ~	2048 ~	8192 ~	9747 ~
2/6/38	RS	1/1	3/3	3/3	4/4	5/5	3/3	—
	LS	0/2	0/2	0/3	0/5	0/5	0/4	—
2/7	RS	—	5/5	4/4	1/1	3/3	3/3	—
	LS	—	0/7	0/2	0/3	0/5	0/5	—
2/8	RS	6/6	3/3	3/3	4/4	4/4	4/4	—
	LS	0/4	0/4	0/3	0/5	0/4	0/5	—
2/9	First entry: left side							
2/10	RS	3/3	5/5	7/7	8/12	9/11	5/5	—
	LS	0/3	0/5	0/5	0/6	5/14	0/8	—
2/11	RS	3/3	5/5	5/5	4/4	10/12	4/4	4/4
	LS	0/5	0/6	0/4	0/4	0/8	0/5	0/3
2/12	RS	—	5/5	5/5	3/3	5/5	3/3	3/3
	LS	—	0/4	0/6	1/7	0/6	0/5	0/4
2/14	RS	2/2	2/2	2/2	1/1	2/2	3/3	2/2
	LS	0/1	0/3	0/3	0/3	0/3	0/2	0/3
3/4	RS	1/1	2/2	1/1	3/3	3/3	2/2	2/2
	LS	0/1	0/1	0/3	0/2	0/4	0/3	0/4
3/4	First entry: right side							
3/5	RS	3/4	6/8	2/2	3/3	4/4	3/3	2/2
	LS	0/2	0/2	0/3	0/2	0/3	0/4	0/2
3/14	RS	1/1	2/2	2/2	1/1	3/3	4/5	2/2
	LS	0/3	0/3	0/3	0/3	0/3	0/5	0/3
3/17	Second entry: right side							
3/18	RS	0/3	0/3	0/4	0/4	0/6	0/4	0/3
	LS	—	—	—	—	—	—	—
3/19	RS	5/6	2/6	6/10	8/13	8/15	4/4	2/2
	LS	7/10	2/2	5/10	5/6	2/2	2/2	2/2
3/21	RS	0/2	6/6	3/3	2/2	1/1	2/2	2/4
	LS	—	3/3	6/7	2/2	2/2	1/1	2/2

operation) not only completely eliminated the L-R habit but temporarily depressed the CR as well for all test-frequencies. In spite of 11 shock-reënforced trials out of a total of 27 trials on the first post-operative test (Mar. 18th), there was no evidence of the conditioned response. The test was made but 23 hr. after the initiation of the third operation, and it is likely that this test-result was a consequence of operative shock. On the following day, there was almost complete recovery of the CR. There was, however, no sign of the L-R habit, then or later, the dog continuing to respond indiscriminately to both RS and LS stimuli.

Discussion. In a study of perceptual processes in animals, it is evident that the validity of the investigation depends in part upon the technique that is used. On occasion, (negative) results procured by one method have been supplanted by (positive) data gathered by means of an entirely different technique. As an example, experiments in which the electrical method was used to determine the function of the round window in hearing,[16] have been negated by studies in which the motor technique was employed.[17] Ruckmick's suggestion, that results procured by an electrical approach should be checked by a concomitant behavioral study, is pertinent.[18] Furthermore, negative results procured with one behavioral approach need not be conclusive. Definite proof of pattern vision in the rat was not established until the introduction of Lashley's "jumping" apparatus. The contradictory results procured in the study of conditioned reflexes in totally decorticate dogs by the salivary and motor methods have already been discussed.

A similar discrepancy occurs between Bikov's negative findings and the present data. When tested by the motor CR technique, the localization of sounds persists following transection of the corpus callosum. One can only conclude that the salivary technique is not as reliable as the motor conditioning method in extirpation studies. The L-R discrimination likewise is shown to persist following extensive lesions in the cortex of either the left or right cerebral hemisphere. These findings, as well as the development of the L-R habit in the completely hemi-decorticate animal, were shown to be more consistent with the present views concerning the organization of the acoustic pathways.

The normal functioning of the L-R habit requires the establishment of some differential acoustic effect at the cochleas: time differences, intensity differences, and phase differences. The cortical mechanism involved in this discrimination is not indiscriminately spread through the cortex as a whole, but is limited to a specific area, probably within the limits of the sylvian gyrus. The present data would seem to indicate that not only is there a bilateral auditory representation (which conclusion has already been made by previous investigators), but that there is also a *bilateral cortical representation for both cochleas;* impulses from both cochlea terminate on each of the cerebral cortices.

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[16] W. Hughson and S. J. Crowe, Function of the round window: An experimental study, *J. Amer. Med. Assn.*, 96, 1931, 2027-2028.

[17] E. Culler, G. Finch and E. Girden, Function of the round window in hearing, *Amer. J. Physiol.*, 111, 1935, 416-425.

[18] C. A. Ruckmick, A critical review of the field of audition, *Psychol. Bull.*, 33, 1936, 407-431.

The following interesting question is immediately suggested: Do particular parts of the critical area function in the perception of specific auditory frequencies, or is there equipotentiality of function for the L-R habit? Pennington's interesting studies[19] have been directed to the solution of this problem. The present data are in agreement with his findings in the white rat, that there is a critical cortical area functioning in this discrimination. Likewise, the L-R habit is interfered with, only if there is a bilateral cortical extirpation. He reports, however, that the auditory localization habit depends upon the amount of functional cortex within the critical area: the amount of interference is in proportion to the amount of cortex extirpated.

In his experiments, Pennington used buzzers as stimuli, and the validity of his interpretation is questionable because of this fact. Let us suppose for the moment that specific parts of the critical area function in the perception of specific frequencies. If noises are used as the stimuli, the extirpation of particular parts of this area would eliminate the perception of particular frequencies. Since auditory localization is more difficult for pure tones than for complex buzzer-noises, the more cortex removed the more frequencies that cannot be perceived; hence the L-R habit becomes progressively disrupted. A similar effect could be produced by extirpating progressively more and more frequencies from the acoustic stimulus. That is, by the use of proper sound-analyzers, one could present a number of auditory stimuli varying in complexity in wave-form from noises to single frequencies. One would under these circumstances, get a varying amount of accuracy in the L-R discrimination habit.

A conclusive test of this question can be made only if pure tones are used as stimuli. If the L-R habit is disrupted equally for all frequencies regardless the size or locus of the lesions (within the critical area), one can safely conclude in favor of a theory of mass action. If on the other hand, specific lesions effect the loss of the L-R habit for particular tones, one can conclude that particular parts of the cortex function in the perception of particular frequencies, *in so far as the L-R habit is concerned.* While the present data include tests with pure tones, the results do not bear directly upon this question. One might argue that since the animal responds to the particular frequencies, such a mechanism exists. An unequivocal test of the question is, therefore, being made, by studying the effects of localized lesions upon the L-R habit for pure tones. Until such results are procured, further speculation at this time would be fruitless.

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[19] L. A. Pennington, The function of the brain in auditory localisation: IV. Method of training and control experiments, *J. Comp. Psychol.,* 25, 1938, 195-211.

Conclusions. In contradiction to the evidence procured by the salivary CR method, it is found that the L-R auditory localization habit persists following complete transection of the corpus callosum. This discrimination can also be developed in the completely hemi-decorticate animal. The relationship of these findings to auditory theory and the significance of the difference between the two methods is discussed.

The present data, corroborate previous findings reported in the white rat; the L-R habit is a function of a restricted area of the cortex within the temporal lobe. The two areas of the left and right hemispheres can act in unison or separately, as the case may require: the L-R habit persists following the removal of the auditory cortex of either the left or right cerebral hemispheres. The application of the present technique in the further exploration of cortical functions in hearing is indicated

THE PROBLEM OF SET

By G. L. FREEMAN, Northwestern University

The experimental psychologist exercises only a relative control over the phenomena which he investigates. He can set up and manipulate certain of the determining conditions, hoping that those conditions of which he knows little or nothing will operate in a constant direction or else will cancel each other's effects over a long series of observations. If the experimentalist is lucky, he can state a relationship between the phenomenon under investigation and the conditions which he has varied. If he is unlucky, he must assume the operation of some other conditions over which he exercised little or no control. Probably the most hackneyed of these alibi conditions is 'set.' For example, it is very convenient to explain a capricious relationship between the span of apprehension and stimulus intensity as due to shifts in the subject's attitude or set. This may be a shrewd guess in the right direction, but when dragged in as an afterthought, the suggestion is not very illuminating. One cannot make such an ill-defined concept the scapegoat for his failure at experimental control by the mere saying of words. The truth is that most researchers give little attention to the problem of set, having only the vaguest notion of the conditions referred to the term and being very naïve as to their experimental check and control. In the zeal to connect 'stimulus' with 'response,' this important determinant of performance is frequently ignored.

By common agreement the term 'set' refers to those vaguely defined organic states, conditions or processes which are antecedent to some overt reaction-pattern and which run along with and so exercise a selective and determining influence upon that reaction. Introduced into psychology to explain aberrations in behavior not easily associated with the external stimulus, it has had a confused history. This is indicated by the variety of terms which have been used in similar contexts—*Einstellung, Aufgabe, Bewusstseinslage, determining tendency, predisposition, posture, readiness,* and *anticipatory reaction.*

The importance of set was first recognized in experiments on reaction-time, where it was found that preliminary attunement aroused by the ready-signal increased the speed of subsequent response. This was followed by the distinction between the 'sensory' and 'motor' reaction-sets and the study

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of antecedent organic adjustments in the judgment of lifted weights. *Aufgabe* and the importance of the *foreperiod* appeared with experiments upon the action-consciousness; Külpe emphasized the importance of the 'task set' in attentive preparation, and in their analyses both Watt and Ach noted wide differences in formulated tasks. Messer went further to include nonconscious predispositions under the term. While Müller regarded the determining tendency of Ach as 'voluntaristic' and 'mysterious,' he substituted something very much like it in his 'goal idea,' and showed that in the learning of nonsense syllables one item came to carry a 'reference' to the next in the series. Introspective studies of thinking gave brief glimpses of the progress of determining tendencies; Mayer and Orth applied the term *Bewusstseinslage* to the 'flash' reports which informed the thinker of his position on the matter under consideration. In the argument which turned upon the question of their attributive status, the functional significance of these conscious attitudes and determining tendencies was all but lost. While their importance as governors of performance appears again in Köhler's 'insight,' Lewin's 'tensions' and Tolman's 'means-end-readiness,' in such contexts the determinations partake too much of the nature of psychical agents and powers to be generally accepted.

Work on the concomitants of response in directed activity has secured the interest of students of behavior in the problem of set. Dodge's 'anticipatory reaction,' Woodworth's 'prepared act,' and Dashiell's 'postural response' are attempts to conceive these important organic conditions in muscular terms. Experiments on the preparatory muscular adjustments of various acts give body to the behavioral point of view without adding much by way of clarification. Regarded systematically, all that is exhibited is the tendency for certain antecedents of activity to express themselves in terms of a muscular *'Einstellung.'* Bills' 'quantitative set,' Young's 'organic' dispositions, and the various work preparations found in the researches by Jersild, Poffenberger and Robinson are not to be reduced so simply. While some assume muscular concomitants not only for residues of preceding acts but also in more remote preparations of a task to be performed, psychologists are generally inclined to regard the problem of set as fundamentally neural; Titchener's 'brain habit,' Von Kries' 'connective disposition,' and Thorndike's 'readiness to respond' are typical examples. But the problem is not to be shifted thus easily to the backs of the physiologists (who are surprisingly ignorant of its existence): it compels the further attention of psychologists themselves. Useful discussions are rare, but where these appear, considerable has been done in bringing a descriptive order to the field and in pertinent theorizing.

Discussing sets as 'antecedents of function,' Bentley mentions two ways in which they operate to limit and define the activities of the organism.[1] The *predisposition* is "a general readiness, such as comes from long study or employment, or from devotion to an art. We are always 'tuned' for some functional performance, although the tuning often passes unnoticed for want of an occasion to touch it off." The *specified task* exists "where an anticipative interest is present. . . . It is more than a mere tendency to function. The organism . . . formulates an obligation or at the least an interest. When the formulation lapses, as it does regularly under repetition, there commonly remains the . . . predisposition." Bentley gives a helpful account of the various conditions and forms of preparation which the psychologist has been able to describe (including task, formal, occasional and self instruction, fore-period and determination) and adds "what we especially need is a physiology of the mechanisms by which a functional inclination in the central nervous system is made up and sustained while the action progresses on its way to movement. In the brain there is doubtless a specific harboring of energy which is integrated on the one hand with the central processes underlying apprehension and, on the other, with the orderly and systematic use of the mechanisms underlying movement. . . . Since we cannot accept a special governing center of automatic and arbitrary powers or a more imperious will, we must look for highly patterned processes within the brain which sustain and guide the nascent determination toward its ultimate outcome."

In Dashiell's treatment,[2] sets are classified as postural responses. After distinguishing between phasic and static forms of reaction, a strong case is made for bodily postures and sets as static responses which operate to support, reënforce, inhibit or otherwise affect the more short-lived (phasic) movements. Attention is a "form of posturing" which favors overt response to some particular stimulus. Many emotional reactions include postural alterations which 'set' the organism for ensuing response. The effect of previous sensory reactions is to establish a 'prejudicial set.' Here and elsewhere are suggestions of a dual response-mechanism, phasic and static, each part contributing to the behavior flux and each influenced by the other. But while recognizing that phasic and static elements exist in all response, Dashiell is not prepared to set up a hard and fast dichotomy and questions where in the total pattern of muscular adjustments one type of action begins and the other leaves off. It is unfortunate that Dashiell does not develop this theory more fully, especially since its difficulties become more apparent when considered in detail. Implicit in his writings is the assumption that the effect and influence of set is registered and produced in the efferent part of the reaction-arc. We are told that "behavior manifests some narrowing down of motor output to certain particular channels." Postural differentiation apparently holds for certain forms of prepared action—as in sensory and motor reaction sets; but can the 'set to add' and the 'set to subtract' be so differentiated? On the contrary, it would seem that both sets utilize the same motor channels, and that their differentiation must be central.

While Woodworth agrees with Dashiell in seeking a response interpretation of set, his analysis leads him in another direction.[3] Instead of relating sets to a particular class of responses, Woodworth, by implication, makes them a tentative activation of the response which they anticipate. We are told that "the preparatory set for an act consists in going as far as possible towards the execution of the act, without actually releasing it. To adapt our S-O-R formula to prepared reactions we have to remember that the preliminary signal, P, arouses a readiness to make a certain response and then comes the stimulus, S, and releases that response. Preparation consists in making a start toward R; S finds . . . activity R already in progress but suspended and gives O the necessary nudge to release the complete R." We shall not pause with the equivocation involved in "suspending" activities which are "in progress," but turn at once to the core of Woodworth's argument. This seems to be that the awaited stimulus acts as a trigger releasing to full potency a pattern already activated. By implication the stimulus does not change or contribute anything toward the character of the reacion. It merely increases the strength of this pattern so that what was implicit and covert becomes explicit and overt. The preparatory signal is only a part of the larger stimulus-pattern which conditions the response. Accordingly the response must exist in a tentative state until the rest of the stimulus-pattern appears, only then by the summation of energies is the threshold of overt reaction reached. This notion is especially attractive in explaining sets for reactions which are distinctly motor. If similar analysis could be successfully advanced for all types of performance, there would be little to condemn this logical extension of Woodworth's theory. But when similar muscular patterns are found to support the higher psychological functions, it is hard to accept the principle that set is a tentative manifestation of an about-to-be-overt motor response. The notion of a specific sensorimotor arc maintained during the pre-stimulus period by slight contractions in the muscles which form the terminus of that circuit is attractive in its simplicity. But the neuromuscular mechanism is something more than a mere congeries of isolated reaction-arcs, and we should approach with great soberness any conception of set which leads implicitly in this direction.

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[1] M. Bentley, *The Field of Psychology,* 1927, 388; *The New Field of Psychology,* 1934, 229.

[2] J. F. Dashiell, *Fundamentals of Objective Psychology,* 1928, 275; *Fundamentals of General Psychology,* 1937, 339.

[3] R. S. Woodworth, *Psychology,* 1934, 327; Situation-and-goal set, this JOURNAL, 50, 1938, 130.

Hoisington's analysis of set adds measurably to the descriptive side but reaches no concrete theoretical formulation.[4] He points out that 'sets' are a type of reflex integration functionally distinct from the "specific reactions" made to sensory stimuli. The responses which are the determining tendency "are general in character, muscular tensions rather than overt movements. They start with and grow through the exercise of more specific task or problem sets. They arise out of the performance of the several (specific) tasks which they come to dominate." The relational effects of specific activities are apparently registered in certain integrating centers, for we are told that "it is not unusual for the product of integration to come to dominate further functional interaction . . . mathematical determination arises with the doing of mathematics problems, but once developed, it determines overt ability in, and methods of attacking, this kind of problem." Hoisington escapes the natural tendency to assign specific neuromuscular tensions to all his determinations, implying that channalization may be central. Determining tendencies or sets may have varying degrees of specificity with regards to the activities which they anticipate and control. There may thus be 'sets' within sets, a possibility which prompts the question "was there one original task-set out of which all other sets and determinations arose, or do they all finally reconverge in some master determination; or does some one tendency spread over the total organization so that it becomes characteristic of all (more specific) determinations?"

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[4] L. B. Hoisington, *Psychology: An Elementary Text,* 1935, 133.

It can be seen that in describing the phenomena of set each account calls attention only to certain aspects of the problem; Bentley and Hoisington distinguish between 'sets' of various kinds; Dashiell mentions the difference between 'sets' and the overt acts which they sustain; Woodworth points out their similarity. All this is very confusing to the critical student. Instead of working hypotheses capable of being tested, he is faced with vague descriptive generalizations or theories quite inaccessible to experimental attack and criticism. This state of affairs is largely responsible for the relative neglect of the phenomena of 'set' both in laboratory procedures and in the systematic setting of related problems.

The present report attempts to construct a working hypothesis of 'set' in terms of postural responses and central nervous function. This hypothesis is developed in a series of six propositions, on each of which information is either already available or obtainable by direct experimentation. We consider the six propositions in order.

(1) Reaction to stimulation involves both phasic and tonic response-segments. Study of the neuromuscular mechanism shows that it is capable of reacting in two ways, phasically and tonically. This fact is first met in the distinction between tonic and phasic muscular contraction.[5] Its importance in integrated behavior is also appreciated. Beginning with Sherrington's 'reflex figure' and continuing to Walshe's 'associated reflexes,' physiologists have realized that response includes not only activity directed specifically toward the source of stimulation, but also widespread supporting postural adjustments.[6] From another direction, evidence accumulates that muscular tension supplies a major share of cortical excitation, and is responsible for lowered sensory thresholds during waking activity.[7]

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[5] For Sherrington, the same motor connections are involved in both cases, the muscle contracting tonically in response to the stretch of its proprioceptors and phasically in response to exteroceptor stimulation (C. S. Sherrington, Postural activity of muscle and nerve, *Brain,* 38, 1915, 191-234). Hunter assumes that different fibers, red and white, are active in each case (J. Hunter, The postural influence of the sympathetic nervous system, *ibid.,* 1924, 47, 261-274); while Hunt distinguishes the part of the muscle-tissue stimulated, with the sarcostyles giving phasic and the sarcolemma giving tonic movements (J. R. Hunt, The dual nature of the efferent nervous system, *Arch. Neur. & Psychiat,* 10, 1923, 37-82). Hunt also suggests that the two types of activity have differing control centers in the pyramidal and extrapyramidal systems.

[6] Cf. C. S. Sherrington, *Integrative Action of the Nervous System,* 1906, 1-200; L. M. Walshe, *Med. Sci. Abst. Rev.,* 7, 1923, 109-118.

[7] Cf. Edmund Jacobson, Further experiments on the inhibition of sensations, this JOURNAL, 23, 1912, 345-369; and L. H. Cohen and S. Lindley, The relationship

The phasic segment of response is of short duration and usually represents a temporary adjustment to some momentary and fleeting stimulus. The tonic response-segment represents a more enduring type of adjustment, acting to maintain a certain continuity in the organism's conduct. Phasic acts of voluntary or reflex nature occur only when the muscles are in a state of slight tension. Special types of response cannot be made unless the pattern of tonic contraction expresses itself in a given posture. Except for such postural adjustments, the inception of a voluntary act would find the part to be moved in an unsuitable position, as is the case in locomotor ataxia. Phasic intraneural sequences seem equally dependent for their maintenance upon a steady backflow of afferent impulses from the tonic processes Just as the taximeter translates distance into cents, so the final product of postural change is measured and new phasic orientation related thereto. Regardless of the particular type of neuromuscular action under investigation, the mutual contributions of the tonic or postural segment and the phasic or kinetic segment are always apparent.

(2) Tonic response-segments may come to anticipate the associated phasic response in the process of repeated stimulation and 'conditioning.' While the concurrence of tonic and phasic response-segments is generally accepted, the principle that the tonic aspects of the pattern may precede as well as outlive the phasic aspects requires demonstration. Unaccustomed to thinking of the total response-pattern as involving two elements, layers, or divisions of activity, experimenters have given their major attention to the phasic aspects of response. These are the most obvious features of neuromuscular action, but not necessarily the most important. It is probable that explanation of response variability in the face of apparently constant stimulus-conditions rests upon the fact that these aberrations are not strictly phasic in character but are built on a *postural substrate*—a neuromuscular background of tonic origin which operates to facilitate, inhibit, and otherwise to modify phasic reactions. Very little is known of the time-relations between the tonic and phasic processes, although there is some evidence that the postural response outlasts its phasic concomitant.[8] If the tonic

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of muscle tonus changes to vibratory sensibility, *Psychol. Monog.*, 47, 1936 (no. 212), 83-93. The relation of tonic contraction to consciousness is discussed by the writer (G. L. Freeman, Mental activity and the muscular processes, *Psychol. Rev.*, 38, 1931, 428-449). For experimental studies from this laboratory cf. R. M. Simpson, The effect of experimentally induced muscular tension upon sensory thresholds, (in press).

[8] Without the postural substrate, man's behavior would undoubtedly resemble that of a complicated jumping jack. The patterning of this substrate exhibits considerable inertia to change, forming a frame of reference which anticipates the next response and thereby renders consistency to the more fleeting aspects of reaction. (Cf. Dashiell, *op. cit.*, 317-321.)

segment continues beyond a given phasic response-segment, it would thereby precede the next phasic movement. In many performances, such as playing tennis, the postural adjustment (stance) may remain relatively unchanged during several similar phasic acts (returning the ball). While it is difficult to say just where in the total pattern of muscular adjustments one segment begins and the other ends, it can be seen that those segments which are antecedent to a given phasic reaction are theoretically able to exercise a directive influence over it. To assert that this principle holds generally, however, raises the question of how one part of a reaction-pattern comes to anticipate and govern the remainder. We know very little of the actual mechanics involved here, but we can set up an hypothesis capable of experimental check.

Let us suppose that a stimulus configuration (S_1s_2) evokes a response pattern (R_1r_2) which has both tonic and phasic elements. In the resulting complex it is reasonable to assume that tonic and phasic factors may be captured by different parts of the stimulus-configuration. The phasic element will be associated with the most dominant part of the stimulus-configuration and its character will be determined by connections of low innate 'resistance.' This is the 'unconditioned response' of Pavlovian terminology. What, however, of the tonic response-elements so generally ignored by this school? Here we had best deal with an actual situation, since a rethinking of conditoning theory is essential to our understanding. When the dog is first placed in a harness there is aroused an attitude of apprehension which expresses itself in a generalized postural adjustment. The dog does not 'know' from what quarter to expect punishment or reward. He is prepared for a wide range of possibilities. This tonic posture is active when the combination of bell (s_1) and shock (S_2) is first applied and it becomes r_1 in the resulting reaction-pattern r_1R_2. Since lifting the foot (R_2) is already connected with S_2 (unconditioned shock stimulus) there is a tendency for the postural readiness or apprehension (r_1) to be associated with the remaining stimulus s_1 (bell). But this s_1-r_1 connection does not remain unmodified by the S_2-R_2 connection, for under repetitive stimulation the posture of generalized apprehension is early changed into specialized adjustment in one part of the body. The pattern of anticipatory tension is now localized in the leg to be stimulated, and the dog turns his head in that direction. This qualitative and quantitative shift in r_1 may be accomplished without special reference to s_1. What probably happens is that r_1 persists longer in the part of the body making R_2 and this differential posture carries over to the application of the next conditioned stimulus.[9] Since the tonic factors outlast the phasic factors, it will at first be the only reaction to be connected with the stimulus-complex when S_2 (shock) is withdrawn. But since r_1 has been built up by training to be anticipatory of R_2 and since s_1 can now activate r_1 prior to the occurrence of S_2, something like R_2 may follow the s_1-r_1 connection. Thus while s_1 (the bell) was at first only connected with the apprehensive posture or set, it now carries to R_2 (conditioned foot withdrawal). By hypothesis, this connection is not direct, as indicated conventionally in Diagram 1, but indirect, as indicated in Diagram 2. In this scheme, the first effect of the postural adjustment (r_1) is to lower the threshold for R_2 (the phasic response) via proprioceptive afferents to the motoneurone field of R_1.

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[9] One of the strongest arguments in favor of this 'set' analysis is the fact that 'conditioning' occurs most easily when conditioned precedes, rather than coincides with, unconditioned stimulus.

Diagram 1

Conventional C-R formula

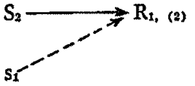
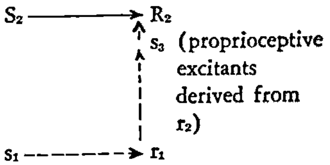


Diagram 2

Role of tonic response (r_2) in conditioning



Later, if r_1 is sufficiently intense and if the S_2 it anticipates is absent, r_1 can itself supply a full substitute stimulus (s_3) through its proprioceptive afferents so that a weak approximation of R_2 may occur.

This unorthodoxy is not without support. Conditioned responses frequently possess a configuration quite different from the unconditioned response on which they are based.[10] Tolman[11] has indicated that many facts deriving from conditioned-reflex experiments cannot be adequately described in terms of the simple Pavlovian doctrine of stimulus substitution, Hilgard[12] writes similarly that "the dynamics of the conditioning process require either the abandonment of the simple formula, or its supplementation with other concepts;" and Guthrie[13] has mentioned the necessity of adding the animal's attitude or set as an organizing principle in conditioning experiments. Various investigators[14] have noted postural accompaniments of conditioning, and there is some evidence that these may precede the conditioned (phasic) response. The fact that a trained animal frequently begins to salivate when placed in the harness seems to indicate that a postural set may itself arouse a conditioned response. More crucial information would be provided by an experiment in which an attempt is made to shift the phasic response by shifting the postural adjustment or set.

It should be emphasized that we have recast the conventional C-R formula only as a means to an end. Our major interest has been to show the possible mechanism by which the tonic aspects of a patterned reaction may be sufficiently disassociated from the phasic aspects to become antecedent to, and anticipatory of, it. Whether this analysis can be extended to the more subtle varieties of prepared response is discussed in the next section.

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[10] E. C. Weaver, The upper limit of hearing in the cat, *J. Comp. Psychol.,* 10, 1930, 221.

[11] C. C. Tolman, *Theories of Learning in Comparative Psychology,* 1934, 317-408.

[12] E. R. Hilgard, The nature of the conditioned response: The case for and against stimulus-substitution, *J. Exper. Psychol.,* 20, 1936, 366-385.

[13] E. R. Guthrie, *The Psychology of Learning,* 1935, 1-258.

[14] Cf. in this connection, M. A. Wenger, A criticism of Pavlov's concept of internal inhibition, *Psychol. Rev.,* 44, 1937, 297-312.

(3) Set is essentially a central expression of the limiting and directing effect of tonic upon phasic response-aspects of the total reaction-pattern. In ascribing to the tonic response-factor a governing effect in all types of patterned reaction, our explanation of set enters its most difficult phase. If different neuromuscular activities always terminated in different effector-mechanisms, it would follow that differential patterns of tonic contraction could 'set' the part concerned for phasic response by lowering the thresholds of neurones in the final common path. Something like this may well occur in specific motor reactions. But there are many sustained intra-neural sequences of thought and memory where effector specificity is apparently impossible to demonstrate. To include the determinations which carry the organism through such activities as finding the solution to a mathematical equation or reviving a series of related experiences, however, we must think in terms of central effects.

Tonic contractions differ in their relative intensity and extensity, depending on the phasic acts which they sustain. Thus the tonic aspect of the conditioned foot-withdrawal is presumptively localized mainly in those muscles utilized in the fact. The set to add, on the other hand, is represented by a pattern of tension of wide extent but little intensive differentiation. These patterns do not stand for two mutually exclusive types of reaction; rather do they represent extremes of a scale, with central effects determined accordingly. Thus, when the postural pattern is highly concentrated in muscles which are to be utilized in the phasic act, the effect may be so marked that phasic response often begins without waiting for the appearance of the remainder of the stimulus-pattern. The runner who gets 'set' in response to a verbal command (s_1 in the conditioning analogy) shows a tendency to start before the gun is fired; the driver gets his car under way before he actually sees the green light; the dog lifts his foot before the shock is applied. In these cases, by hypothesis, excitation of tonic origin (either from the extra-pyramidal system or proprioceptive afferents from the muscles) is so intense or so highly localized that motor neurones in the final common path discharge to produce the phasic response. If, however, excitation of tonic origin is not sufficiently intense, and especially if its effects are not localized at some specific motor outlet, it can still act differentially to lower thresholds of higher level neurones so that these neurones are prepared to respond quickly and selectively to excitation of other origin.

Let us suppose a particular cortical pattern to be involved during the addition of numbers. This pattern is originally organized by the visual symbols and under repetitive stimulation it becomes a *dominant,*[15] capable of summating formerly unrelated afferent impulses. The majority of such impulses are derived from the proprioceptive field. Increments in the neural excitation occasioned by an increased attentive posture flow toward the cortical region of greatest instability (the dominant) and further intensify its gradient. Thus a channelization of proprioceptive energies is created and a facilitative effect develops for the next phasic 'adding' pattern which the cortical neurones must sustain. All that remains now is for some part of the original stimulus-complex (verbal instruction or symbol) to capture this pattern of centrally channelized proprioceptive excitation (which is the set to add), and the cerebral dominant may be established prior to the application of the number sequences. According to this theory, the set to add is derived in the same way as the set to expect shock in conditioning experiments; *i.e.* when set to add, the organism takes a general attentive posture and this 'tonic' excitation is directed by the 'conditioned' instruction to arouse the cerebral dominant which is basic to 'phasic' adding activity.

(4) Tonic selection of phasic response holds for all types of reactions and for all levels of integration. The argument outlined above takes on body as we survey how the proprioceptive-tonic selection of exteroceptive-phasic response may occur at different levels of neural integration. When the skin on the flank of the spinal dog is stimulated, exteroceptive impulses flow into the spinal cord and produce a scratching response. However, the motor units which respond to these impulses are already under the influence of proprioceptive excitants emanating from the associated muscles; the exteroceptive impulses produce their effects upon this background and in accordance with it. Since these volleys are stronger and more nearly simultaneous than the proprioceptive excitants,[16] they may arouse the motor neurone field to full discharge—but not without the aid of the proprioceptors. If a muscle is de-afferented so as to lose its tone, formerly effective exteroceptive impulses cause little or no response. But the proprioceptive impulses do more than merely summate with any exteroceptive impulses which find their way to motor units under their dominance. They exercise a certain restraint and determination over the effectiveness of the enteroceptive impulses as well. Thus, if the position of the limb of our spinal dog were such that the muscles were dominated by inhibitory rather than

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[15] For more detailed discussion of Ukhtomski's principle of the dominant, cf. G. L. Freeman, *An Introduction to Physiological Psychology,* 1934, 427-429.

[16] It will be recalled that in maintaining the normal tone of a muscle, only a few fibers are acting at a given instant, and so there is a weak but continuous discharge centrally of the proprioceptors.

excitatory proprioceptive impulses, the scratch-response will not be evoked. This is the situation when the same external stimulus gives first flexion and then extension of the leg.

Reactions integrated at higher neural levels indicate similar effects. Phasic responses to painful stimulation are very specific if the neuromuscular apparatus is prepared for the exteroceptive excitant, otherwise the typical generalized startle-response is made. Voluntary finger-oscillation begins as a pattern of exaggerated co-contraction in the antagonistic muscles. This 'warming-up' stage is essentially a function of the tonic system. Perceptive activities have a large postural component; selective reaction to a complex environmental field involves a general increase in reactivity and augmented tensions in muscles serving to place the end-organ appropriately. The occurrence of these reactions is generally recognized by the fixed gaze of the look-out at sea, the inclined ear and cocked head of the eavesdropper, and the concentrated expression of the tea-taster. These postural responses contribute to efficient sense impression; by hypothesis this is due to the channelization of proprioceptive excitation in specific cortical centers, thereby rendering these centers dominant in intracerebral fields.

The foregoing argument can be carried over to the reintegrational sequences of memory. One may take the set to recall the historical events connected with visually presented symbols, or one may associate these same symbols with different blocks in one's street of residence. Word-series will arouse different responses depending upon whether the subject is set to give subordinates, opposites, or synonyms. The 'control' in these controlled associations seems to be due essentially to a channelization of proprioceptive energies into highly limited cerebral fields. In thinking, the relational postures which mean "now I have it," "that is irrelevant," etc., are equally effective. While it is a long step from psychological description to physiological principle, we can probably regard the general thought-problem as involving a particular pattern of cortical stresses which is maintained by proprioceptive excitation. Work has already indicated that shifts and changes in these stresses are reflected in tissues under the dominance of the tonic reaction-system.[17]

In states of excited emotion, where the organism is functionally decorticate, the primary part of the reaction-pattern is tonic in character. Instead of highly specialized postural sets, the entire striate musculature is thrown into a state of heightened activity. This is accompanied by widespread visceral and humoral changes, with consequent central effects unchannelized.

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[17] T. M. Abel, Attitude and the galvanic skin reflex, *J. Exper. Psychol.*, 13, 1930, 47-60.

Lack of specific connection between exteroceptive excitant and phasic response is indicated in the studies of emotional expression. It is significant that the only basic physiological distinction between emotional states is the tension or relaxation of the postural substrate. In excited emotion thalamic mass-discharge prevents control over phasic S-R relations. We look to this region of the basal ganglia for centers mediating differential postural adjustment and we find them in the striatal-tectal complex.

(5) Qualitative and quantitative character of the tonic response-segment varies with the similarity of the phasic responses it sustains. Having indicated that tonic selection of phasic response is apparently a very common phenomenon, it is important to add that not all tonicly derived sets are equally limiting. There are all degrees of specificity in the phasic responses which different tonic sets anticipate and carry through. In general, the more diffuse the postural effect, the more phasic acts it will support in common. This hypothesis can be tested experimentally. Suppose an animal so trained by the conditioning technique that the following stimulus-response sequences hold.

$\left.\begin{matrix}\text{UCS}\\ \text{cs}_1\end{matrix}\right\}$ .. R_1r (left hind foot)

$\left.\begin{matrix}\text{UCS}\\ \text{cs}_2\end{matrix}\right\}$ .... R_1r or R_2r (right or left hind foot according as USC is applied)

$\left.\begin{matrix}\text{UCS}\\ \text{cs}_3\end{matrix}\right\}$ .... R_1r or R_2r or R_3r (right or left hind foot or right front foot according as UCS is applied)

$\left.\begin{matrix}\text{UCS}\\ \text{cs}_4\end{matrix}\right\}$ .... R_1r or R_2r or R_3r or R_4r (right or left hind or front foot according as USC is applied)

In this situation the unconditioned shock stimulus (UCS) may be applied to any one of the four limbs, depending upon the particular conditioned stimulus (cs) used. Thus cs_1 becomes the signal for only one phasic response(lifting of the left hind foot), cs_2 for one of two, cs_3 for one of three, and cs_4 for one of four possible responses. According to hypothesis, the postural set (r) should show quantitative and qualitative differences according as it anticipates phasic reaction in one particular limb or in one of several different localities; *i.e.* tonic adjustments would be more diffuse and generalized for cs_4 than for cs_1 since cs_4 has to encompass more possibilities.

Something of this kind seems to hold quite generally. Thus there may be a general mathematical determination which supports use of all four fundamental processes or the specific set to add. In performances of this order we should not expect qualitative differences between the postural accompaniments of these two determinations, though we might find greater tension-increments for the more general and non-specific determination.

(6) Specificity of phasic response to stimulation depends upon an ability to control and check shifts in the expression of tonic response-factors. According to our hypothesis, tonic factors in response develop through phasic reactions and in turn remain to induce and determine the character of subsequent phasic response. It is only as we neglect one part of this mutual relation that our experimental results become ambiguous and difficult of interpretation. Suggestions leading to greater specificity of phasic S-R relations through control of tonic bodily trends include:

(a) Make sure that the total experimental situation (including instruction and occasional setting) is held rigidly constant; otherwise unnoted variations in some aspects of the situation may become conditions for shifting the tonic set and hence the phasic response-segments observed.

(b) Allow sufficient time to lapse between the introduction of the subject into the experimental situation and the beginning of systematic observation; otherwise, residual effects of previous stimulation will be so marked as to upset and change the phasic reaction-pattern which is to be studied.

(c) Counter-balance all variables whose relation is being studied; otherwise bodily trends which are a function of time and repetition will not be eliminated and the data may show mainly the effect of such continuous postural adjustments as underlie fatigue, practice, boredom, expectation and apprehension.

(d) Employ some systematic check on the tonic aspects of response; otherwise uncontrolled effects of attitude and set will pass unnoticed.

The historical controls in psychological experimentation, such as are found in nonsense-syllable learning and by practice in general, have contributed much which is in line with the first three suggestions. Beyond all these, however, the psychologist must go to check upon the factors which actually govern the performance under survey. Reports by the trained observer provide such a check upon any reformulations which his task undergoes during the period of observation. But since many of our psychological problems are now attacked with naïve human subjects and lower animals, some other methodology will need to be developed in order to check on bodily trends which are at variance with the factors under investigation and control. Here we must turn to one or more of the bodily indicators, such as the galvanic skin-response, blood-pressure changes, and muscular action-potentials. It is known that any one of these processes will reflect quantitative changes and shifts in the mobilization of energies of tonic origin, though the relative advantage of each in special situations is a matter for extensive future research. Perhaps a mass recording of these processes in a variety of experiments will give a clue to their differential use in diagnosing the qualitative as well as quantitative shifts in neuromuscular action and the central effects which tonic response-segments sustain.

If our analysis of the problem of set has served its intended purpose, more questions have been raised than answered. Positive statements have been made, not in the spirit of dogmatic assertion, but in order to clarify the theory. It should be emphasized that the problem of set is a subject for experimental study, and that our hypothesis must be considered not so much by the pertinent facts which it now commands as by the lines of attack which it opens. The more important of these include (1) a new survey of conditioning phenomena, with adequate attention to the total postural adjustments involved, (2) a study of the modulating influence of tonic excitation on phasic reaction-sequences by combining stereotaxic stimulation with action-potential recording in animals, and (3) a check upon the relative efficiency of peripherally registered processes as indicators of tonic bodily trends.

In conclusion we would mention certain qualifications which are recognized in regard to the views set forth above. While giving particular attention to the influence of tonic over phasic response-factors, it is conceived that the relationship is mutual. Tonic sets do not arise *de nouveau* as determiners of phasic reactions. They are residues and originate out of previous phasic responses.[18] Furthermore, the notion of a division of labor within the total reaction-pattern sets up no hard and fast dichotomy; 'tonic' and 'phasic' may be purely relative terms, with a response-segment which is phasic in one performance serving a more static function in another. Dissociation of these 'elements' is likewise an artifact of analysis, as is any implied disjunction between functional course and initiation. The tonic factor may anticipate the phasic factor in time, but it always runs along with and is inevitably bound up with the entire reaction sequence. A momentary cross-sectional sampling of performance such as is popular with the neo-behaviorists, is not particularly informing as to the operation of our hypothesis. For crucial evidence we must return to the longitudinal type of survey used in the study of thought processes.

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[18] The mechanisms involved here cannot be envisaged fully at the present time, though cerebral architecture suggests the importance of transcortical stimulation. The extra-pyramidal field (which is presumably associated with tonic bodily effects) covers a vast cortical territory and almost any specific connection between sensory and 'true' motor area could 'set up' this field and so influence subsequent transient phenomena.

SUMMARY

The term 'set' is found so often in psychological writing that we forget that it has never been adequately defined. As used quite generally, the term refers to those vaguely defined organic states, conditions or processes which are antecedent to some overt reaction-pattern and which run along with, and so exercise a selective and determining influence upon, that reaction. Descriptive characterizations of set phenomena have been made in terms of both mental organizations and behavior, but there is a tendency for all writers to regard the problem as fundamentally neural. The present report constructs a working hypothesis of set in terms of postural responses and central nervous function. After indicating that all reactions involve both tonic and phasic response-segments, the hypothesis is developed that 'set' is a central expression of the limiting effect of proprioceptive-tonic activity upon exteroceptive-phasic activity. It is shown that at any given moment the organism is exposed to a great mass of internal and external excitants and that without some internal regulation there would be an indiscriminate diffusion of afferent discharge throughout the nervous system. This regulation is provided, by hypothesis, through a narrowing of the afferent flux into prepared adjustor and effector channels. On the one hand, proprioceptive excitation sustains selected exteroceptive excitations in the central apparatus; on the other hand, tonic contraction underlies phasic contraction. The mechanism by which specific postural adjustments or sets are developed and connected with external stimuli is suggested and a number of relevant questions are laid open to experimental attack. When the work of testing these propositions is finally completed it may be possible to regard the concept of set as physiologically superfluous and speak in the more ultimate terms of neural facilitation, lowered thresholds or altered potential-gradients. Until that time, however, its use does serve to hold together phenomena having a common matrix. The possibility of checking the government of psychological performance by means of related physiological processes is basic to more refined control in experimental situations. Ultimately we should expect that an increased knowledge of the mechanisms of set would result in a greatly improved methodology.

SHAPE AS A FUNCTION OF THE VECTOR-FIELD

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With the advent of Gestalt psychology in 1912 a new theory of perception was enunciated which had as its basic principle the assumption that the properties of a given stimulus are determined by the total situation or Gestalt.[1] Since then the results of numerous studies have established the validity of this assumption.[2] Perceived brightness, form, velocity, and color have all been shown to be determined by the stimulus-pattern in which they are found.

For the Gestalt theory to have predictive value it was necessary for it to show how the total situation determines the activities and properties of the parts within it; *i.e.* to determine the laws of organization and to state them in such a manner that the facts of psychology can be deduced from them. This problem has been solved in various ways. Köhler[3] has set up a psychophysical theory of physiological Gestalten, Koffka[4] has postulated forces of organization, Lewin[5] has ordered Gestalten to a system of constructs based on topology, and Wheeler[6] has deduced a number of laws descriptive of the dynamics of organization. All of these methods have proved useful, but, with the possible exception of the topological concepts, they are either too general, or they lack the systematization necessary to lead to deductions of a quantitative sort.[7]

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[1] The author wishes to thank Dr. J. F. Brown for suggesting the problem of this paper and for help in preparing the manuscript.

[2] G. W. Hartmann, *Gestalt Psychology,* 1935, esp. Chap. 6, The phenomena of visual perception, 93-131.

[3] W. Köhler, *Die physischen Gestalten in Ruhe und im stationären Zustand,* 1920, 173-248.

[4] K. Koffka, *Principles of Gestalt Psychology,* 1935, 38-145.

[5] K. Lewin, *Principles of Topological Psychology,* 1936, 1-231. Before Lewin developed the topological psychology he had, in collaboration with Sakuma, suggested treating position in the visual field in a fashion similar to the one developed in this paper. K. Lewin and K. Sakuma, Die Sehrichtung monokularer und binokularer Objekte bei Bewegung und das Zustandekommen des Tiefeneffektes, *Psychol. Forsch.,* 6, 1925, 298-357.

[6] R. H. Wheeler and F. T. Perkins, *Principles of Mental Development,* 1932, 16-38.

[7] R. H. Wheeler, Postulates for a theory of education: A methodology for educational research, *J. Educ. Research,* 28, 1935, 187-195. In this paper Wheeler gives three stages in the development of laws: first, the prediction that the event will occur at all; secondly, the prediction of the direction of the process; thirdly, predictions of a quantitative nature. He states that his own laws are still in the first and second stages of development.

In the recent paper by Brown and Voth[8] a theory is advanced which makes possible quantitative predictions about the properties of visual Gestalten. In their theory visual experiences are ordered to a visual field, and vector field-forces, which are functions of physical variables, are postulated to account for the interactions taking place. The effect of the first of these, the cohesive field-forces, is to attract objects and cause them to flow together. The other field-forces, the restraining field-forces, describe the characteristics of the field which give stability to the objects in the field. Their direction is opposite that of the cohesive field-forces. The strength of both field-forces at any point is a function of time, space, and the distribution of intensity in the field. If the combined cohesive field-forces are designated by $\Sigma\mathbf{C}$ and the combined restraining field-forces by $\Sigma\mathbf{R}$, three limiting field equations are possible:

$$(1)\quad \Sigma R > \Sigma C$$
$$(2)\quad \Sigma R = \Sigma C$$
$$(3)\quad \Sigma R < \Sigma C$$

From this theory Brown and Voth made the following predictions. When the first of these conditions is realized, the field is chaotic, and the phenomenon classified as autokinetic movement occurs. The second condition gives rise to stable configurations, in which Gestalt properties may be deduced from the vector-field. When the third condition is realized, movement will occur. Brown and Voth concerned themselves experimentally only with the third equation, and by it they predicted the path of seen movement from the structure of the visual field. Their successful treatment of both real and apparent movement has led to the present study which follows their suggestion that many of the Gestalt phenomena of stable configurations can be predicted from the equation (2), where $\Sigma\mathbf{R} = \Sigma\mathbf{C}$.

VECTOR-FIELD THEORY FOR SHAPE IN STABLE CONFIGURATIONS

If the Gestalt factors of stable configurations are to be accounted for by the posited field-forces, it must be possible in stable configurations to demonstrate their action experimentally. This could be done by showing their effect on objects introduced into the field, just as the presence of electric field-forces is detected with pith balls, and of magnetic field forces by the deflection of a magnetic needle. If it is possible to predict changes in the shape of geometrical figures introduced into stable configurations from the theory of the vector-field, the thesis may be looked on as verified. The present study characterizes with regard to the theory the vector-fields set up by geometrical patterns, and then seeks to verify the theory experimentally by predicting changes in the shapes of introduced objects.

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[8] J. F. Brown and A. C. Voth, The path of seen movement as a function of the vector field, this JOURNAL, 49, 1937, 543-563.

In order to treat geometrical figures as vector-fields of cohesive field-forces, it is necessary to suppose certain relations between the field-forces and the properties of the objective stimulus-pattern. If two objects are brought into the visual field, they will immediately be acted upon by cohesive and restraining field-forces whose magnitudes are functions of the physical properties of the objective stimulus-pattern. In this paper the cohesive field-forces are treated solely as functions of the spatial separations between the objects in the field. They are assumed to increase as the spatial separations between the objects decrease. Mathematically, the relation may be represented by the formula: $\Sigma C = K \cdot f[1/(C + d^n)]$, where d is the spatial separation, K and C are undetermined constants, and n is a constant not definitely determined but positive and equal to or greater than 1. If the two objects are seen as a stable configuration, by definition the $\Sigma R = \Sigma C$. This does not, however, mean that the two objects have not been attracted toward each other. Rather, it means that the two objects have been attracted toward each other until $\Sigma R = \Sigma C$.[9] Such an interpretation demands that the restraining field-forces increase as the object is displaced from its position. Therefore, it is further assumed that $\Sigma R = K_1 \cdot f(d_1)$, where K_1 is an undetermined constant, and d_1 is the displacement of the object from its position. These two assumptions concerning the magnitudes of the field-forces will be used to determine the distribution of the forces in the vector-fields set up by geometrical patterns. The effect of other factors, such as size and intensity, on the magnitude of the forces will be discussed later. Previous studies show that the shapes of objects in the visual field are also dependent upon muscular pattern[10] and 'experience.'[11] However, in this initial study certain delimitations of the problem are necessary. Muscular pattern and 'experience' are assumed to have been held constant, and only two-dimensional patterns are considered.

The mode of attack indicated leads to the assumption that a figure introduced into a structured field will be distorted in the direction of the *positions of cohesive equilibrium* until equilibrium is reached, just as a magnetic needle in a magnetic field will be rotated until equilibrium is reached. An example will serve to introduce the concept of position of cohesive equilibrium, which is useful in describing the vector properties of the fields. If three parallel lines of the same size and intensity are introduced into the visual field so that the interior line is midway between the two exterior lines, it will be in a position of equilibrium because the cohesive field-forces acting on it from the other two lines will be equal and opposite; the exterior lines will be moved inward until $\Sigma \mathbf{C} = \Sigma \mathbf{R}$. If the center line is moved so that it is no longer midway between the two exterior lines, it will no longer be in a position of equilibrium and will be distorted toward the nearer line. (For the purpose at hand, the disequilibration of the two other lines can be neglected.) If the restraining field-forces were not acting, the interior line and the line nearest it would meet in a position somewhere between their objective positions. The positions which objects in the visual field would occupy, if it were not for the action of the restraining field-forces, will be called *positions of cohesive equilibrium.* They can be determined from the assumptions made above, as will be shown in the next paragraphs. It should be possible to find situations where the restraining field-forces are weak as compared with the cohesive field-forces. In such situations the distortions should be experimentally observable, and the present paper reports such cases.

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[9] In a special case the objects might be so placed that no displacement would be necessary for equilibrium to ensue.

[10] J. Fröbes, *Lehrbuch der experimentellen Psychologie*, I, 1923, 268; D. M. Purdy, The structure of the visual world: Space perception and the perception of wholes, *Psychol. Rev.*, 42, 1935, 399-424.

[11] H. A. Carr, *Introduction to Space Perception*, 1935, 402-403.

CHARACTERIZATION OF GEOMETRICAL PATTERNS AS VECTOR-FIELDS

Three geometrical figures (Fig. 1), which will be referred to as *geometrical* fields, were used in the experiments. The first task is to determine the positions of cohesive equilibrium for these fields. Field *A*, a pattern of radiating lines, will be considered first. (In Fig. 1, *A* represents field *A*, and *A′* an enlarged section of *A* which will be needed for the deductions.) In Fig. 1 *A′* consider the two field radii 1 and 2. Let a_1 and a_2 be points on radii 1 and 2, respectively, at a distance a from the field center C. CX_1 is the line bisecting the angle between radii 1 and 2. The line perpendicular to radius 1 at a_1 and the line perpendicular to radius 2 at a_2 will meet at a point on CX_1, designated by b_1. The cohesive field-forces from radius 1 acting on the points on the line a_1b_1 tend to pull them toward radius 1 approximately along the line a_1b_1; *i.e.* the points are shifted very little from the perpendicular. The same argument applies to the points on the line b_1a_2 with respect to radius 2. However, the cohesive field-forces from radius 2 act to pull the points on a_1b_1 toward radius 2 along the lines perpendicular to it; the cohesive field-forces from radius 1 act in the same manner on the points on the line b_1a_2. These forces are stronger the closer the points are to the respective lines. (These forces are indicated by arrows in Fig. 1 *A'*.) The resultant is a curved line, $a_1b'_1a_2$, joining a_1 and a_2. This line is the position of cohesive equilibrium between the points a_1 and a_2, and is to be regarded as a minimum stress position in the field. Since this position is perpendicular to the two radii at the points of contact, as more radii are introduced into the field, the position will approach as a limit the arc of the circle, for one characteristic of a circle is that it is perpendicular to each of its radii at the point of contact. Hence, the position, $a_1b'_1a_2$, will be treated as the arc of a circle. For the entire field the position is a circle with radius a and concentric with the field. Evidence for this is obtained from the fact that a circle placed concentric with the field is not distorted in shape. By letting a vary from O to R, the field radius, a set of positions of cohesive equilibrium, concentric with the field is ob-

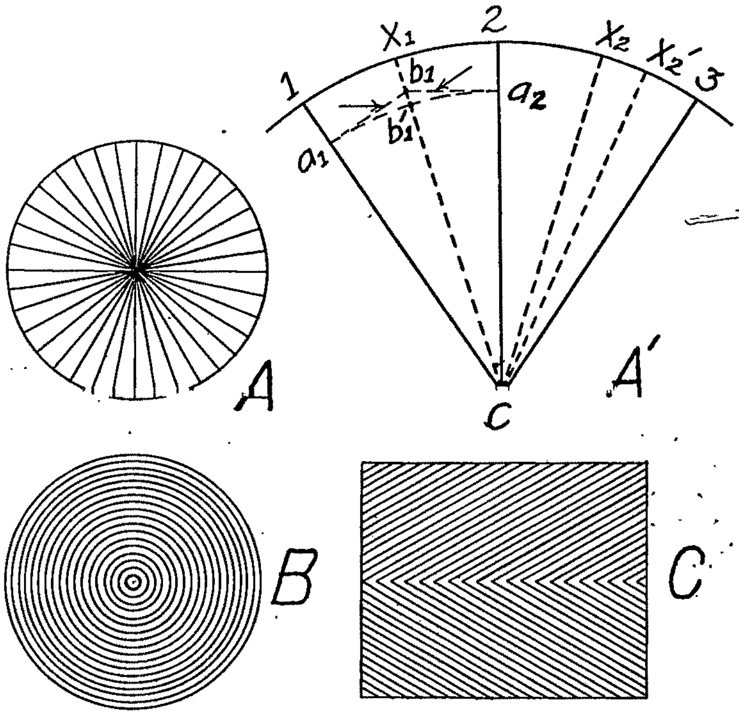

FIG. 1. THE GEOMETRICAL FIELDS USED IN THE EXPERIMENTS WITH THEIR RESPECTIVE POSITIONS OF COHESIVE EQUILIBRIUM

Positions of cohesive equilibrium are shown by the dashed lines. *A*, *B*, and *C* are the fields used. *A'* is an enlarged section of *A*.

tained. These are indicated by the dotted lines in Fig. 1 *A*.[12] The force with which an object is distorted toward these positions is inversely proportional to the distance of the object from the field-center, because the cohesive field-forces determining these positions are stronger the closer the object is to the field-center.

The bisectors of the angles between the radii, the lines CX_1 and CX_2 in Fig. 1 *A′*, are also a set of positions of cohesive equilibrium. A line in such a position will be in equilibrium; however, if it is moved slightly from that position, such as the line CX'_2 in Fig. 1 A′, it will not be distorted toward CX_2 but toward radius 3. Thus, this set of positions need not be considered here because figures will not be distorted toward them. From the above example it can be seen that the field-radii will be positions of cohesive equilibrium for lines approximately parallel to them.

Therefore, in the vector-field set up by the pattern of radiating lines, two sets of positions of cohesive equilibrium may be determined.

In Field *B* (Fig. 1 *B*), a pattern of concentric circles, the principal set of positions of cohesive equilibrium consists of field-diameters. A line in such a position is in equilibrium because the magnitudes of the cohesive field-forces acting between the line and the concentric circles are at a minimum, and the forces are balanced; *i.e.* the resultants are of equal magnitudes and are acting in opposite directions. The locus of points halfway between any two consecutive concentric circles is also an equilibrium position because the cohesive field-forces acting between the loci and the two circles are balanced. If a line coincident with the locus is moved slightly from that position, it will be distorted toward the field-diameters and not toward its original position. Thus, this set of positions of equilibrium also will not be used because figures will not be distorted toward them.

In Field *C* (Fig. 1 *C*), a pattern of angles, there are two sets of positions of cohesive equilibrium, each of which is perpendicular to a set of parallel lines in the field. A line perpendicular to a set of parallels is in equilibrium because the cohesive field-forces acting between it and the parallels are balanced and their magnitudes are at a minimum.

From the theory developed it is expected that the restraining field-forces acting on objects introduced into the fields will be partially overcome by the cohesive field-forces of the fields, and that this will result in the distortions of the objects toward the positions of cohesive equilibrium determined for the fields. (In these experiments the reorganization of the positions of cohesive equilibrium resulting from the introduction of the objects into the fields is assumed to be negligible.) Thus, as shown in Fig. 2, a circle introduced into the field of radiating lines, so that the field-center is inside the circle but does not coincide with the center of the circle, should be distorted. The part of the circle nearest the field-center will have the greater curvature because this part of the circle will be distorted toward a position of cohesive equilibrium having a greater curvature than has the circle. The part of the circle diametrically opposite will have the lesser curvature because this part of the circle will be distorted toward a position of cohesive equilibrium having less curvature than has the circle. The prediction and experimental verification of the distortions which should result when given figures are introduced into the fields is reported in the next section.

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[12] In Fig. 1, positions of cohesive equilibrium for the fields used are indicated by dashed lines. Since the theory requires that objects placed in these fields should be distorted toward these positions, the reader can determine for himself the nature of the distortions which should occur when a given figure is placed at a designated position in the field.

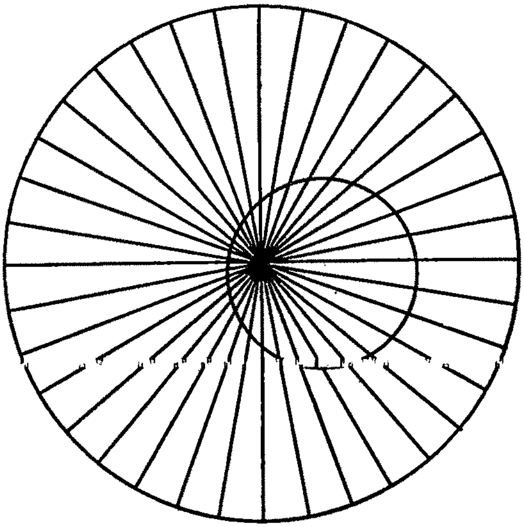

FIG. 2. DISTORTION OF A CIRCLE SUPERPOSED ON FIELD A
The field-center is inside the circle but not coincident with its center.

EXPERIMENTAL VERIFICATION OF THE THEORY

Method, apparatus and subjects. The prediction of the distortions which should result when a given figure is placed at a designated position in the field was checked by superposing the figures on the fields and comparing the distortions reported by the group of *S*s with those predicted by the theory.

To facilitate the placing of objects in the fields, both the fields and the figures to be placed in them were drawn in black India ink on white tracing paper. The figure was placed behind the field in a given position. Both figures were then placed immediately in front of a milk glass aperture in a specially constructed light box, in which the intensity of illumination was controlled by a rheostat. In this position the figure was seen as if superposed upon the field. This method makes it possible to superpose figures on fields with great rapidity and to study the changes which take place in a figure when it is moved about in a field.

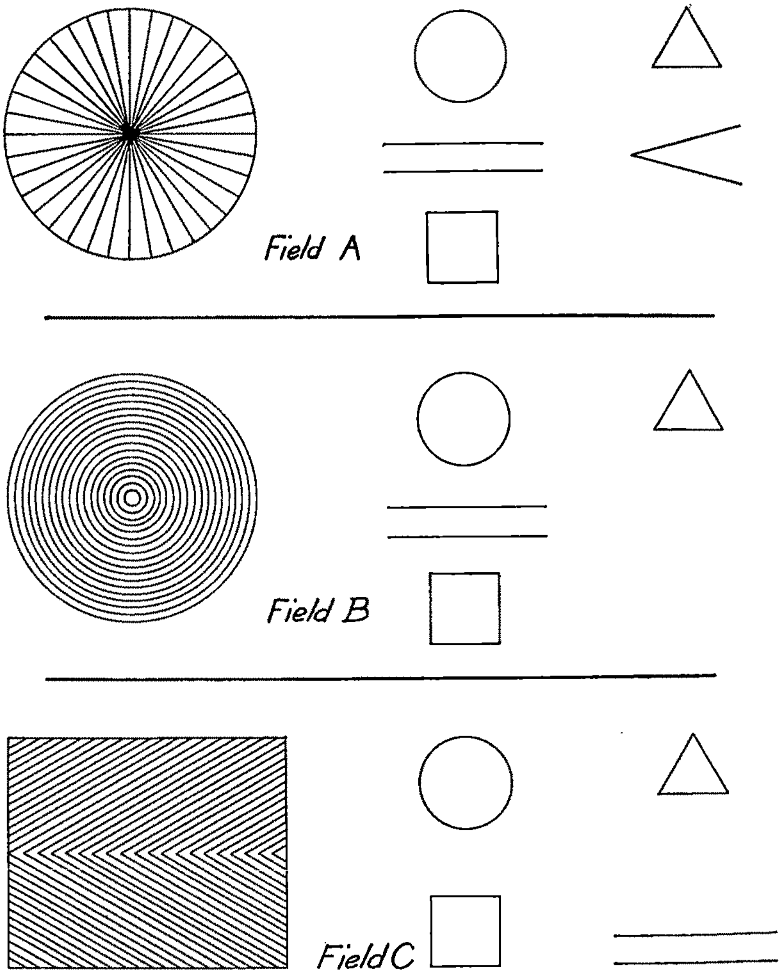

FIG. 3. THREE FIELDS WITH THE FIGURES WHICH WERE SUPERPOSED ON THEM
Both the figures and the fields are drawn to scale. See text for exact dimensions.

In the experiments to be reported, the field of radiating lines used was a circle, 10.8 cm. in diam., with 18 equally-spaced diameters drawn in it; the field of concentric circles consisted of 18 equally-spaced circles, the largest of which had a diameter of 10.8 cm.; the field of angles was a rectangle 12 x 10 cm., in which were drawn 60° angles with their corresponding sides parallel, and their vertices collinear and 0.6 cm. apart.

The figures superposed on these fields were the following: a circle of 4-cm. diam., parallel lines 7 cm. long and 1.2 cm. apart, a square 3 cm. on a side, an equilateral triangle 3 cm. on a side, and a 30° angle whose sides were 7 cm. long. The lines in all figures and fields were 0.5 mm. in width. Both figures and fields are reproduced in Fig. 3, which is drawn to scale. In the succeeding exposition it is to be understood that the figures introduced were those described above.

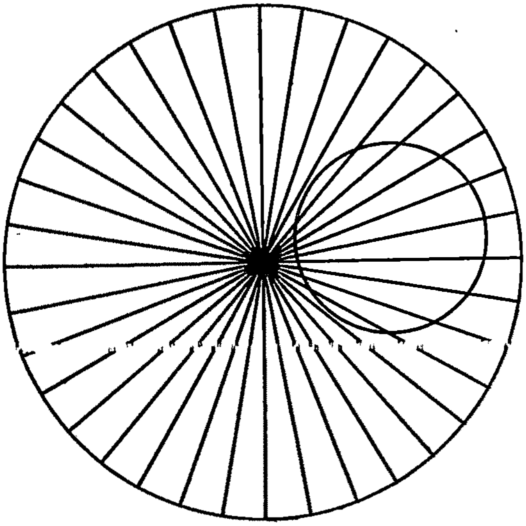

FIG. 4. DISTORTION OF A CIRCLE SUPERPOSED ON FIELD A
The field-center is outside the circle.

The *S*s, some thirty in all, were faculty members and majors in the Department of Psychology at the University of Kansas. In the experimental situation *S* was shown a figure and told that he would be asked to describe the changes occurring in the figure when it was placed in the field. Since there was complete agreement among the *S*s as to the direction of the changes which took place, the expression "verified by experiment" will be used to indicate that all *S*s reported the predicted changes. Since these experiments are such as can be reproduced on the printed page, many of the resulting distortions are shown in the accompanying figures.

Prediction and experimental verification of distortions. The first experiments were concerned with the distorting effects of Field *A*. When the circle is superposed on the field so that the center of the circle coincides with the field-center, the shape of the circle should not be distorted because the circle conforms to a position of cohesive equilibrium. No distortion was reported in the experiment. The case where the field-center is inside the circle but does not coincide with the center of the circle (Fig. 2) has already been discussed.

When the field-center is outside the circle (Fig. 4), the part of the circle nearest the field-center should have the least curvature, because

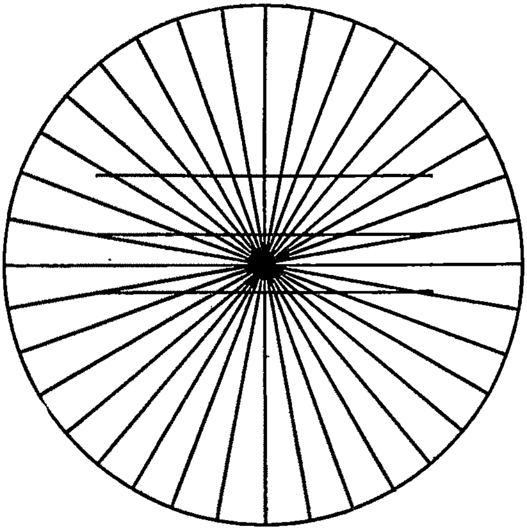

FIG. 5. DISTORTION OF PARALLEL LINES SUPERPOSED ON FIELD A

that part of the circle should be distorted to conform to a position of cohesive equilibrium having a curvature opposite that of the circle. The part of the circle diametrically opposite should have a greater curvature because, although it should have less curvature than has the circle when seen alone, it should be distorted toward lesser curvature, not opposite curvature. Then too, the distortion, without regard to direction, should be greater at the part nearest the field-center because the cohesive field-forces are stronger there. It should be obvious that these distortions all follow from the theory.

When the parallel lines are placed in the field so that the field-center is midway between them (the lower two horizontal lines in Fig. 5; Hering's figure), the distortion of the lines is in accordance with the theory that the lines should be distorted toward the positions of cohesive equilibrium in that the two lines are curved outward, and in that the distortion is greatest at the parts of the line nearest the field-center. When the parallels are bisected by a field-diameter and the field-center is not midway between them (the upper two lines in Fig. 5), both lines should be distorted to conform with circles concentric with the field; the line nearest the field-center and the parts of both lines nearest the field-center should show the greatest distortion.

When the square is introduced into the field so that the field-center is inside the square, the sides should be distorted outward as are the parallels in Fig. 5. (This distortion can be better demonstrated by using a square smaller than the one superposed in this experiment.) If the centers of the field and the square do not coincide, the parts of the square nearest the field-center should show the greatest outward distortion. When the field-center is outside the square and a diagonal of the square coincides with a field-radius (Fig. 6), the two angles of the square bisected by the radius should appear to be greater than right angles, because the sides forming them, in being distorted toward the positions of cohesive equilibrium, should be distorted in a direction which will increase the size of the angles. The angle nearest the field-center should show the greater enlargement.

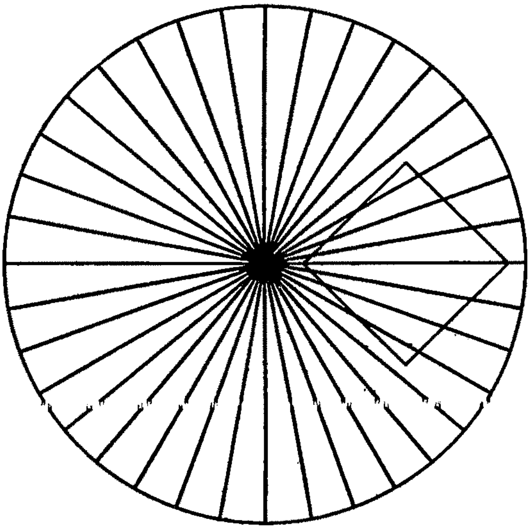

FIG. 6. DISTORTION OF A SQUARE SUPERPOSED ON FIELD A
The field-center is outside the square, and a diagonal of the square is collinear with a field-diameter.

When the equilateral triangle is placed in the field so that the field-center is inside the triangle, the sides of the triangle should be distorted outward. If the centers of the field and triangle do not coincide, the parts of the triangle nearest the field-center should show the greatest outward distortion. When the field-center is outside the triangle and one of the altitudes of the triangle coincides with a field-radius, the angle nearest the field-center should appear to be larger than the other two. The explanations for these distortions in the triangle are the same as for the similar distortions in the square.

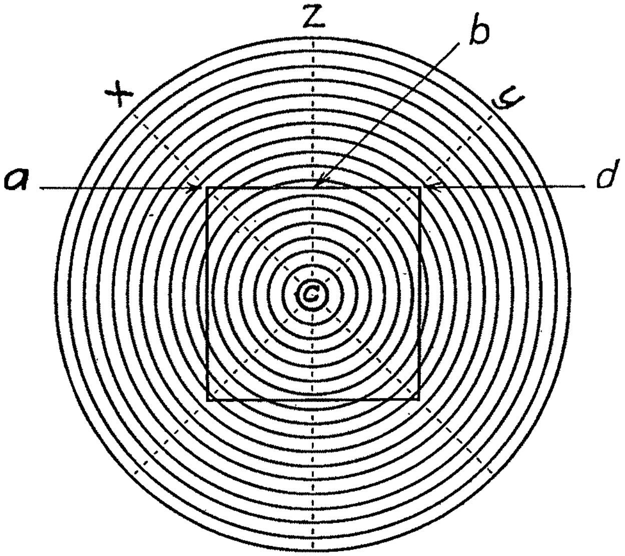

FIG. 7. DISTORTION OF A SQUARE SUPERPOSED IN A CONCENTRIC POSITION ON FIELD B

For purposes of representation the square shown in the figure is larger than the one used in the experiments.

When the 30° angle is so placed in the field that the field-center lies within the angle, the parts of the angle nearest the field-center appear to curve outward (Höfler figure).[13] This distortion is in complete agreement with the theory. In every case for all the *S*s and for other cases, the predicted distortions were verified by experiment. Consequently, the theory may be looked upon as verified, by experiment, for Field *A*.

The next experiments were concerned with Field *B*. Figures superposed on this field, if they are not in positions of cohesive equilibrium, should be distorted toward field-diameters (Fig. 1*B*). The distortion of the square in Fig. 7 is typical. The line *abd* appears to be bent inward at *b*. According to the theory, the line *ab* should be distorted toward the field-radii in the area *cxz*, and *bd* should be distorted toward the field-radii in the area *czy*. This will produce the apparent distortion of the line *abd* toward the field-center in the vicinity of *b*. Since the parallel lines, the circle, and the triangle, when placed in the field, were all distorted in the direction predicted by the theory for all *S*s and all cases, the theory may be looked upon as verified for Field *B*.

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[13] Fröbes, *op. cit.*, 271.

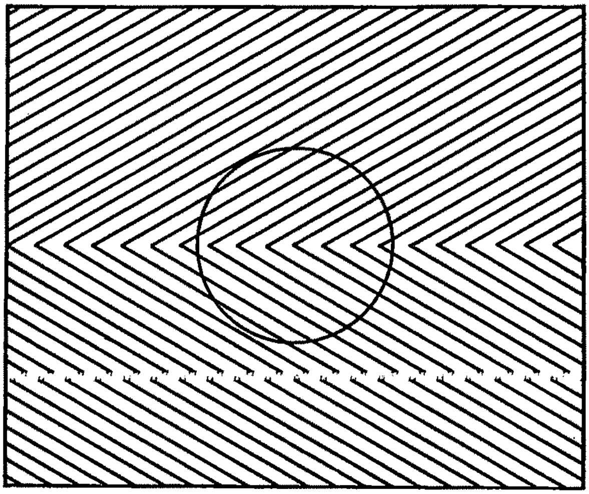

FIG. 8. DISTORTION OF A CIRCLE SUPERPOSED ON FIELD C
A diameter of the circle is collinear with the line joining the verticles of the angles.

The last experiments were concerned with Field *C*. Figures superposed on this field, if they are not in positions of cohesive equilibrium, should be distorted toward them. The distortions of the circle in Fig. 8 and of the square in Fig. 9 demonstrate the validity of this prediction. The circle in Fig. 8 has less curvature in the vicinity at the left than at the right. This is in complete agreement with the theory because the part of the circle at the right, if distorted toward the positions of cohesive equilibrium, will be distorted toward greater curvature, while the part of the circle at the left will be distorted toward lesser curvature. In Fig. 9 the angles at the left and right of the square appear larger than do the other two angles. A comparison of the positions of the sides of these angles with the location of the positions of cohesive equilibrium shows that if the sides are distorted toward them, the angles will be increased in size. The other figure superposed on the field, the parallel lines,[14] the triangle, and the angle, were all distorted in the direction predicted by the theory for all *Ss* and in all cases.

The experimental results show that the direction of the distortion of objects introduced into geometrical patterns may be successfully predicted by characterizing the patterns as vector-fields and by assuming that the field-forces are functions of the spatial separations. However, in making the predictions the effects of other physical variables have been neglected. Brown and Voth,[15] from their study of movement phenomena, suggest that the field-forces are functions of the time, space, and intensity distributions in the field. In a stable configuration time is a constant. Evidence that the intensity factor is almost a constant was obtained from the following experiment. When the intensity of a field was decreased by placing it on the light box and covering it with several sheets of plain white paper before the various figures were superposed on it, the figures were distorted almost as much as they were when

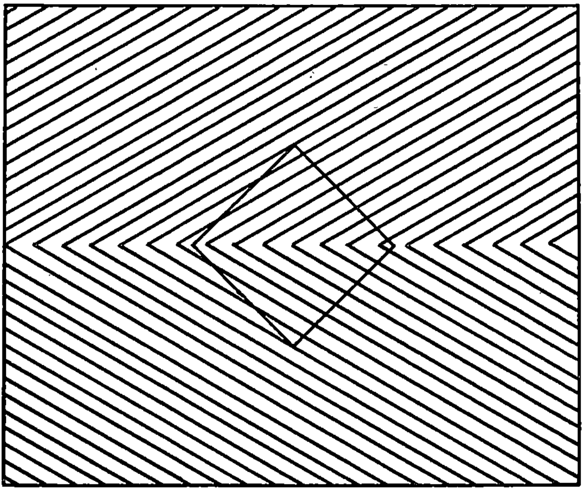

FIG. 9. DISTORTION OF A SQUARE SUPERPOSED ON FIELD C
A diagonal of the square is collinear with the line joining the vertices of the angles.

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[14] The Zöllner Illusion can be constructed by superposing on the field two lines parallel to the line joining the vertices of the angles, so that the latter lies midway between the former. In this position the lines appear to meet in the direction opposite that toward which the angles, if considered as arrowheads, are pointing. This is the direction in which they should meet if they are distorted toward the positions of cohesive equilibrium. A study of the Zöllner figure or any of its modifications will show that the parallels are distorted toward the line perpendicular to the oblique lines crossing them.

[15] Brown and Voth, *op. cit.*, 549.

both the field and the figures were of the same intensity. When the intensity of the figure was decreased relative to the field, the distortion was somewhat greater than when the figure and the field were of the same intensity. It was also found that the dotted figures were distorted in the same direction, although not to as great an extent, as were the figures made of solid lines. One would suppose that both forces are functions of the sizes of the objects in the field. This can be shown to be true, but the effect is scarcely noticeable. For example, in Baldwin's figure[16] a dash half-way between two circles of different size is seen as being slightly nearer the larger circle. Thus, it seems logical to conclude, that in the fields studied, the magnitudes of the forces are determined largely by the spatial separations.

Conclusions and Implications for Further Research

The experiments confirm the vector-field hypothesis that the equation $\Sigma\mathbf{R} = \Sigma\mathbf{C}$ holds for stable visual perceptions. They do so by showing that this assumption leads to the experimentally verifiable implication that in stable visual configurations visual spatial values are distorted with changes in $\Sigma\mathbf{C}$. This was demonstrated specifically by showing that the shapes of objects, superposed on two-dimensional geometrical patterns, are distorted in a predictable fashion by the action of the field-forces. In other words, the results show, not only that shape is determined by the total situation, but also that by ordering the total situation to a vector-field it is possible to predict the shape which an object will have in a given total situation. Thus, the vector-field concept has been demonstrated as applicable to movement phenomena and to stable configurations. The results further confirm the hypothesis that the optic cortex functions as a vector-field.[17] The empirical determination of the relation between the field-forces and the physical variables used offers the possibility of a metrical treatment of the values of visual space. The theory advanced to account for shape is stated so that it is consistent with a field-theory covering the whole of visual perception. Its experimental verification shows the possibility of a generalized attack on the values of visual space.

No attempt has been made to predict the exact position of the introduced object relative to the field. This must wait until the theory is put in a more quantitative form. Since the theory in its present form has proved useful, an attempt is under way to put it into a more precise form and to expand it to include the phenomena of shape and position in both two- and three-dimensional space.

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[16] E. C. Sanford, *A Course in Experimental Psychology,* 1898, 239.

[17] Brown and Voth, *op. cit.,* 561-562.

UNCONDITIONED STIMULUS-SUBSTITUTION IN THE CONDITIONING PROCESS

By W. J. BROGDEN, Johns Hopkins University

The usual conception of conditioning assumes that the conditioned stimulus (CS) substitutes completely for the unconditioned or original stimulus (US). The CS, however, is an effective substitute for the US only when it is followed by the US in its reënforcing capacity. Complete absence of the US brings a rapid extinction of the conditioned response (CR). Finch and Culler have pointed out that the US performs a dual function in the conditioning process.[1] First, it determines the character or pattern of response. Forelimb-flexion from a charged grid is clearly a function of the type of stimulus (electric shock) and also of its locus of application (forepaw). The CS takes over this function of the US only, that of evoking a definite behavior-pattern. Secondly, the US provides the incentive or drive needed to actuate this response. The CS does not substitute for this function of the US. Only constant reënforcement with the US maintains the excitatory tendency of the CS at a maximum. Dogs conditioned to forelimb flexion by these authors required a mean time of 14.9 periods for extinction when a buzzer (which during training sounded for the exact duration of the electric shock) was present and 4 periods when it was absent. The buzzer was neither an US nor a CS for flexion; at no time was it possible to elicit foot-withdrawal to the buzzer sounded alone. The buzzer had for a time taken over the second function of the US, that of providing the incentive or drive for response. The buzzer did not effectively take the place of shock in maintaining a consistent flexion to the CS, but did definitely retard the normal progress of experimental extinction.

It should be possible, however, to elicit a consistent CR to a CS, by substituting a reënforcing agent for the incentive function of the US. With neutral stimuli substituting for both of the functions of an original stimulus, the CR would both be evoked and maintained in the complete absence of the US. Such a response would be truly 'conditioned.' The present investigation was designed to see whether it is possible to substitute novel stimuli for each of the two functions of an US. In other words, once dogs

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[1] Glen Finch and Elmer Culler, Relation of forgetting to experimental extinction, this JOURNAL, 47, 1935, 656-662.

are conditioned with a bell to flexion from an electrically charged grid, is it possible to substitute another stimulus, *e.g.*, food, for the reënforcing function of shock? Furthermore, since food is the US for salivation, will the bell evoke a conditioned secretory response as well as a conditioned flexion response?

Experimental Procedure

Within a soundproofed, light-shielded test-room, with *E* observing through a one-way glass screen, left forelimb-flexion from an electrically charged grid was conditioned to the sound of an electric doorbell in eight mongrel dogs. With six of the animals (*Nos.* 1, 2, 3, 6, 7, 8), the duration of the bell was 10 sec.; with the other two (*Nos.* 4, 5) it was 2 sec. The shock, produced by a high voltage step-up transformer and made variable by suitable resistances in series, was given always at the end of the stimulus-period for approximately 1/10 sec. Conditioning proceeded rapidly with 20 trials per day as the test-unit. Only those conditioned flexion-responses which avoided shock were scored as positive. When one series of 100% shock-avoidance was obtained, every correct response thereafter was rewarded by a biscuit dropped from the food-box, shock being omitted altogether from the test situation. Three of the animals (*Nos.* 6, 7, 8) with a 10-sec. stimulus-duration were continued on shock as controls. The experimental animals were given the bell-food sequence for 1000 trials after the last shock reënforcement. In order to keep the factor of hunger as constant as possible, tests were made in the morning after a 17-20 hr. fast. In no case did any of the animals flex the left forelimb when given food alone. Food was therefore neither an US nor a CS for flexion. The duct of the left parotid salivary gland of the three experimental animals (*Nos.* 1, 2, 3) had been earlier made into a permanent fistula. By sealing a disk attached to a suitable hydraulic system over the fistula with wax, salivary secretion could be measured in a capillary tube on a millimeter scale above *E*'s screen in the test-room. Displacement of 1 mm. on the scale is equal to a volume of 0.00167 c.cm. The stimuli were electrically controlled. Measures of latency were made with a stop-watch. All of the customary precautions, such as variation in the time between stimuli to prevent temporal conditioning and elimination of possible clues from the operation of the apparatus were rigidly observed.

Results

Substitution of food for shock reënforcement of the conditioned flexion-response to bell was made with relative ease. Once shock was omitted there was some hesitancy on the part of all animals to eat, which in no case persisted beyond the tenth trial. As food continued to appear in the food-box after each flexion response to bell, there was a tendency for flexion to be of short duration or completely absent, replaced by the immediate turning of the head to the food-box in the anticipation of food. Also, there was a tendency for 'spontaneous' responses (flexion in the absence of bell) to appear, followed by turning of the head to the food-box. Since to none of these responses was food forthcoming, it was necessary in several cases to return to reënforcement with shock in order to reëstablish a persistent flexion for the duration of the bell. Animal 1 required shock reënforcement on 4 different occasions; Animals 2 and 4 each required one reënforcement series; and Animals 3 and 5 were immediately successful, requiring no reënforcement with shock. The final pattern of response to bell was sustained flexion until the bell was silenced or until food dropped into the box, whereupon the animal turned and ate.

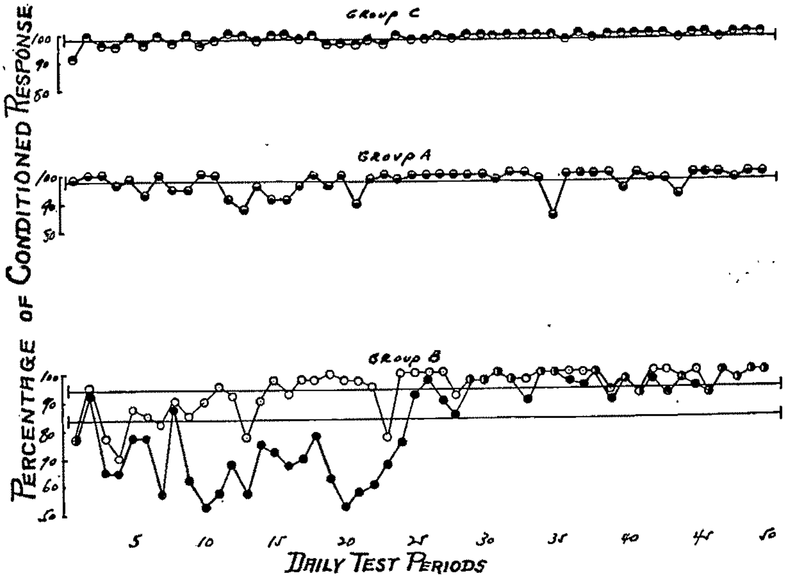

FIG. 1. THE DAILY MEAN SCORES OF THE PERCENTAGES OF CONDITIONED RESPONSES FOR THE DIFFERENT GROUPS OF ANIMALS

The straight line passing through each curve represents the mean group response for the entire experiment. Of the two scores for Group B, the filled circles represent the actual score, whereas the open circles represent the score of that group when the incomplete responses of Animal 5 are added to the actual score.

This behavior was consistent for 1000 trials after the last shock reënforcement. Animal 1 gave a mean for the 50 test-periods of 98.7% (S.E. = 0.57%), Animal 2 a mean of 97.0% (S.E. = 1.05%), and Animal 3 a mean of 97.4% (S.E. = 0.87%). These three animals, conditioned to a 10-sec. stimulus, are designated as Group A in Fig. 1, where the daily mean scores of this and the other two groups are plotted for each of the 50 test-periods. For Group A, the mean response for the whole series is 97.7% (S.E. = 0.50%). Animals 4 and 5, being conditioned to a 2-sec. stimulus, comprise Group B. They gave a mean score of 92.9% (S.E. =

1.55%), and 68.8% (S.E. = 4.04%) respectively, and a mean group score of 82.9% (S.E. = 2.30%). Animal 5 was very difficult to score, since many times its flexion-response was completed just before the end of stimulation. Such incomplete responses were never rewarded with food as they did not satisfy the response-criteria. If these responses are scored as positive, the mean response of Animal 5 becomes 95.3% (S.E. = 1.72%), a level more compatible with those of the other animals. Likewise the mean score of Group B is raised to 94.4% (S.E. = 1.11%). The daily means for this group are plotted in Fig. 1 in two curves: one in which Animal 5 is scored as the other animals; and another in which the incomplete responses are added to the actual score. Animals 6, 7, and 8 were continued with shock throughout the experiment, and with a 10-sec. stimulus, served as controls for Group A. These animals, comprising Group C, gave mean scores of 98.9% (S.E. = 0.33%), 98.9% (S.E. = 0.33%), and 98.7% (S.E. = 0.39%) respectively, and a mean group score of 98.8% (S.E. = 0.25%). If in Fig. 1 we compare Group A with its control, Group C, we find the experimental group to be less regular in response, the standard error of the mean (0.50%) being twice that of the control group (0.25%). The difference between the means of the two groups is, however, only 1.1% (S.E. = 0.56%) in favor of the controls. With a critical ratio of 1.96, the two groups do not differ significantly. Food, therefore, functioned here as did the buzzer in the experiment of Finch and Culler previously mentioned, but with complete effectiveness, since food maintained as high a level of flexion as did shock. The slightly greater irregularity of the experimental animals was probably due to variation in appetite. Shock is usually a more consistent incentive than food.

Group B under both scoring conditions of Animal 5 shows a great deal of irregularity (S.E. = 2.30% and 1.11%) and a much higher level of response in the last 500 trials than in the first 500, being 90.10% and 98.38%, and 69.44% and 96.36% respectively, the true score being given first in each case. Since the low level and irregularity of response was due, however, to Animal 5, whose behavior has been fully discussed earlier, it is most likely that animals conditioned to flexion with a stimulus of 2-sec. duration would respond as frequently and regularly when food was substituted for shock as when it was not.

The mean latency of response for Animal 1 was 2.17 sec. (S.E. = 0.03 sec.), for Animal 2, 3.93 sec. (S.E. = 0.23 sec.), and for Animal 3, 1.28 sec. (S.E. = 0.05 sec.), the mean for Group A being 2.49 sec. (S.E. = 0.08 sec.). No measures of latency were made on the two animals in Group B. In the control group, Animal 6 had a mean latency of 3.98 sec. (S.E.

= 0.01 sec.), Animal 7, 1.77 sec. (S.E. = 0.08 sec.) and Animal 8, 1.91 sec. (S.E. = 0.07 sec.). The mean for Group C is thus 2.55 sec. (S.E. = 0.06 sec.). The difference between the two groups is only 0.06 sec. (S.E. = 0.10 sec.) in favor of the control group. In spite of the tendency for the latency of Group A to increase toward the end of the series, the minute difference between the two means leads to the conclusion that latency of conditioned flexion to bell is the same, whether it is reënforced by food or by shock (see Fig. 2).

In answer to the second question of the investigation, we find that the three animals of Group A did salivate to bell, once flexion to bell was consistently reënforced with food. Salivation to bell occurred in the first period in which food was substituted for shock in both Animals 1 and 2, and in the second period in Animal 3. Conditioned secretion to bell persisted for several periods when Animal 1 was returned to shock for retraining and never disappeared during the two periods in which Animal 2 was retrained with shock. Animal 3 needed no retraining with shock. For the 1000 trials, Animal 1 secreted to bell 72.4% (S.E. = 5.05%) of the time with a mean intensity of 18.75 mm. (S.E. = 1.97 mm.); Animal 2 salivated 43.6% (S.E. = 4.50%) of the time with a mean intensity of 13.12 mm. (S.E. = 1.88 mm.); and Animal 3, 78.50% (S.E. =3.68%) of the time with a mean intensity of 32.40 mm. (S.E. = 2.70 mm.). The mean percentage response for the group was 64.4% (S.E. = 3.17%) with a mean intensity of 22.00 mm. (S.E. = 1.23 mm.). In Fig. 3 the daily group scores of frequency of conditioned flexion, frequency of conditioned salivation, and intensity of conditioned salivation are plotted. The curves of frequency and intensity of salivation follow each other quite closely. They are both quite irregular and fluctuate widely around the mean. The mean difference in frequency of flexion and frequency of salivation is 33.30% (S.E. = 3.21%) in favor of flexion. Although during the 1000

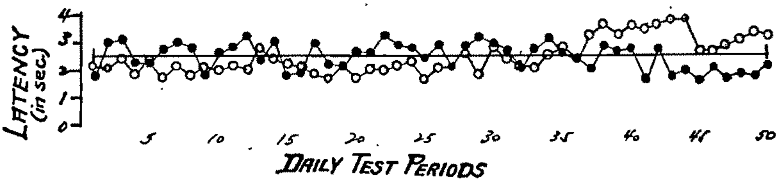

FIG. 2. THE DAILY MEAN LATENCY OF FLEXION
The open circles representing Group A and the filled Group C. Since the mean difference in latency of flexion is only 0.06 sec., the straight line represents the mean latency of response for both groups.

trials bell was followed by food 977 times, bell never consistently evoked salivary secretion. When salivation did occur it was never of very large magnitude. If we examine the results for every animal we find that Animal 1 after the first 30 days gave 16 days of 100% response, the other 4 days in no case dropping below 80%. In the first 30 days, Animal 1 gave 100% response on 3 days only. Animal 2 gave 100% on only 3 occasions in the whole series. Animal 3 secreted 100% of the time on 20 out of 50 days, but only 8 of these were consecutive, from the 17th to the 25th days. Animals 1 and 3 gave a consistent salivary response at some time or another.

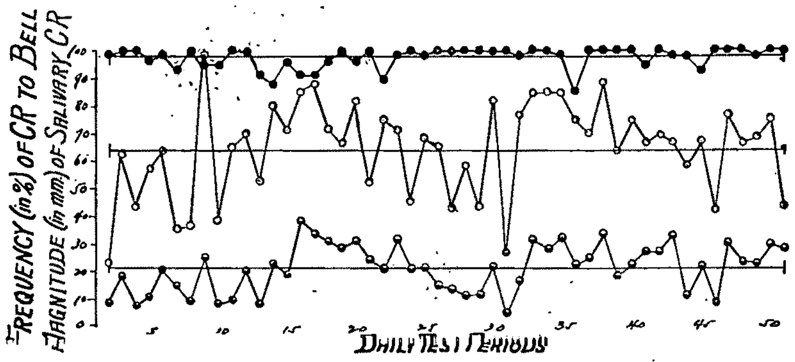

FIG. 3. THE DAILY MEAN RESPONSE OF THE THREE ANIMALS IN GROUP A
The filled circles represent frequency of conditioned flexion, the open circles frequency of conditioned salivation, and the half-filled circles intensity of conditioned salivation. One millimeter of secretion equals a volume of 0.00167 cm. The straight line passing through each curve represents the mean group response for the entire experiment.

Animal 2 never gave a very consistent response and the intensity was always variable.

The mean latency of conditioned salivation was 5.55 sec. (S.E. = 0.28 sec.) for Animal 1, 5.57 sec. (S.E. = 0.31 sec.) for Animal 2, and 3.89 sec. (S.E. = 0.21 sec.) for Animal 3. The group mean latency of salivation was 4.97 sec. (S.E. = 0.16 sec.). The difference between the mean latency of conditioned flexion and conditioned salivation was 2.48 sec. (S.E. = 0.10 sec.). In other words salivation when it occurred, began 2.48 sec. after flexion. In Fig. 4 the daily mean latencies of conditioned flexion and salivation for the group are plotted.

It is rather difficult to understand why these animals gave no consistent salivary response to bell, even though food followed bell 97.7% of the time. Several hundred trials of bell-food is usually sufficient to produce a consistent conditioned secretion to each stimulus-presentation. The experiment was continued to 1000 trials only to see whether conditioned salivation would become consistent. Flexion-failure to bell with subsequent lack of food was so rare that it can scarcely be said that conditioned salivation suffered extinction. Likewise the salivary mechanism of the animals cannot be considered pathologic, as they all gave a good unconditioned secretion and at one time or another, a consistent conditioned salivation. Nor can the several hundred trials of bell-shock prior to food substitution be considered effective in deterring the development of a conditioned secretory response. There is no apparent reason why conditioned salivation to bell should be so irregular, inconsistent, and inferior to conditioned flexion.

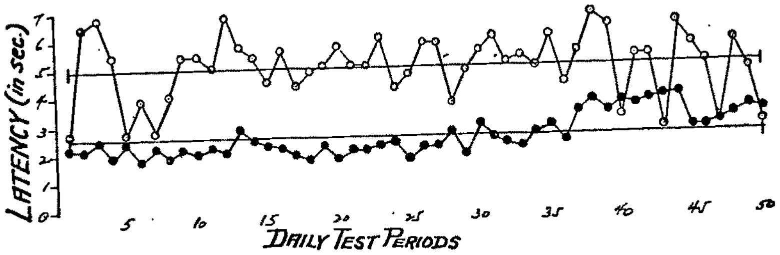

Fig. 4. The Daily Mean Latencies for Flexion and Salivation
The filled circles represent latency of conditioned flexion and the open circles represent latency of conditioned salivation. The straight lines represent the mean latency for the entire experiment.

Discussion

The importance of the dual functioning of the US in the conditioning process must be emphasized. It has long been recognized that the sense organs, particularly the distance receptors, play an important rôle in conditioning by permitting the organism to anticipate and prepare for the action of the US. It is obvious that the number of stimuli which can be utilized as signals is almost infinite, particularly in the human where a written and spoken language is an immense reservoir for such stimuli. On the other hand the number of responses which an organism can make is definitely limited. Muscles and glands can respond only in so many ways, and the permutations and combinations of coördinated grandular or muscular activity are finite. The number of simple or unconditioned responses is still more limited and there can be no more unconditioned stimuli (US) than there are unconditioned responses (UR); but just as the function of eliciting a specific response can be taken over by a neutral stimulus, so can the function of providing the incentive for response be replaced by a novel stimulus. Any given response can be made to many stimuli and can be reënforced by different incentives at different times. For example, the human grasping with the right hand is conditioned to many stimuli and is reenforced in many ways. In the experiment just reported shock elicited flexion of the left forelimb. Once conditioning was established, bell evoked flexion. It would have been possible to make many other stimuli appropriate signals by combining them with shock. Bell continued to evoke flexion when shock was omitted and food given at the end of the response. Food, neither an US nor a CS, replaced the reënforcing function of shock. At no time did food given alone call forth flexion. Flexion was therefore completely 'conditioned,' being elicited and reënforced by stimuli once neutral, long after the appropriate original stimulus had been removed from the experimental situation.

It is rarely difficult in a discussion of human behavior in terms of conditioning to define the stimulus or stimuli which evoke a given response, but it is almost impossible to isolate the unconditioned stimulus from the situation. The behavior of animals and infants is much more amenable to description in terms of the conditioned response, since the original stimuli are largely physiological in nature. In the development of behavior in the infant, neutral stimuli take over the function of the US of evoking a specific pattern of response. As long as these response-patterns are reënforced with the adequate US, the elements of the conditioned response-pattern are clearly discernable. As the infant develops, however, he begins to make responses to conditioned stimuli which are never reënforced by the unconditioned physiological stimulus. As an adult many stimuli evoke response, but little of this behavior is ever reënforced by the original or unconditioned stimulus. Yet the behavior patterns are essentially the same as when he was a child. What has happened to the US? It has been replaced by one of the many forms of social approval or disapproval. The incentive function of the US has been replaced by a social mechanism of approbation or censure, just as food replaced the reënforcing function of shock in maintaining forelimb flexion to bell. The [illegible] complex responses of humans can now be more easily discussed as conditioned responses. The CS stands out in any situation as the signal which elicits a given form of behavior. The incentive or drive which maintains the response to the CS is provided by the social setting and varies with the utility of the response. Reënforcement of response by the unconditioned physiologic stimulus is rare. In fact, responses to physiological stimuli are more often inhibited by the social setting. To receive social approbation and to avoid social censure is frequently more important than the immediate release of a physiological tension.

In a discussion of two types of CR, Skinner,[2] and Konorski and Miller,[3] agree to designate the usual Pavlovian CR as type S and the new CR (upon the formulation of which they disagree) as type R. The new type is one in which a response such as pressing a lever or flexion of the forelimb is followed by food. Skinner states that the response is evoked by no observable stimulus. The response occurs spontaneously and once it is reënforced, develops rapidly. Konorski and Miller evoked flexion by shock and gave food after flexion. Soon the animals flexed the forelimb when placed in the experimental situation to obtain food, shock being omitted thereafter. It is evident that flexion evoked by bell and reënforced by food is definitely related to the type R conditioned response, or to the so-called pseudo-type reported by Skinner,[4] where lever-depression by the rat was reënforced with food only when the response occurred during stimulation by a light. It is certain that any response must be evoked by a stimulus of some sort, whether it is clearly apparent or not. It is also more probable that any response will be elicited by a CS than by an US, since organism-survival depends upon conditioning, and any organism with even a relatively short survival-period responds to many CS. It would seem that the type R CR will appear most frequently in the form in which it is represented in this experiment. A CR is formed (flexion), being evoked by a definite CS (bell). The CS elicits the CR in a situation where reënforcement is given by a new stimulus (food), one which does not itself evoke flexion. As Skinner says of his discussion with Konorski and Miller, "The question at issue is whether we may produce contraction of the pupil according to s—contraction→reënforcement, where (for caution's sake) the reënforcing stimulus will not itself elicit contraction."[5] Certainly bell—flexion→food fulfills Skinner's conditions, since food at no time was capable of evoking flexion.

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[2] B. F. Skinner, Two types of conditioned reflex: A reply to Konorski and Miller, *J. Gen. Psychol.,* 16, 1937, 272-279.

[3] J. Konorski and S. Miller, On two types of conditioned reflex, *ibid.,* 16, 1937, 264-272; Further remarks on two types of conditioned reflex, *ibid.,* 17, 1937, 405-407.

SUMMARY OF CONCLUSIONS

(1) Substitution of food for the reënforcing function of shock in maintaining a consistent conditioned flexion-response to bell was made with relative ease in five animals.

(2) Once food effectively replaced shock, it kept conditioned flexion to bell at a mean response-level of 97.7% (S.E. = 0.50%) for Animals 1, 2, and 3 and at a mean response-level of 94.4% (S.E. = 1.10%) for Animals 4 and 5. In the first group (Group A), the bell was of 10-sec. duration; in the second group (Group B), it was of 2-sec. duration.

(3) The difference in level of response between Group A and its control group, Group C, consisting of three animals continued with shock for 1000 trials, was 1.1% (S.E. = 0.56%) in favor of the control group.

(4) The difference in latency of flexion between Group A and Group C was 0.06 sec. (S.E. = 0.10 sec.) in favor of the controls.

(5) Conditioned flexion to bell reënforced by food does not differ in frequency and latency of response from conditioned flexion to bell reënforced by shock in the animals studied.

(6) Since food given alone at no time evoked flexion in any of the animals, it was neither an US nor a CS for foot-withdrawal. Food, therefore, effectively substituted for the reënforcing function of shock, the US for forelimb-flexion.

(7) Group A, with salivary fistula, gave conditioned salivary secretion 64.4% (S.E. = 3.17%) of the time during the whole series of 1000 trials. The mean intensity of response was 22.00 mm. (S.E. = 1.23 mm.). Condi-

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[4] Skinner, Two types of conditioned reflex and a pseudo-type, *ibid.,* 12, 1935, 66-77.

[5] Skinner, *op. cit., J. Gen. Psychol.,* 16, 1937, 279.

tioned salivation occurred 33.30% of the time less than did flexion and its latency was 2.48 sec. greater than that of conditioned flexion. Conditioned salivation at different times, for short periods, was as consistent as was flexion, but in none of the animals did it persist for long. There is no apparent reason why conditioned salivation should be so inconsistent and inferior to conditioned flexion, particularly when bell was followed by food 977 times in the 1000 trials.

(8) The importance of the dual rôle of the unconditioned stimulus in conditioning as pointed out by Finch and Culler is emphasized.

(9) Conditioned flexion to bell reënforced by food is discussed as similar to the type R CR of Skinner, and of Konorski and Miller

THE PSYCHOLOGICAL EFFECTS OF BENZEDRINE SULFATE

By EDWARD C. REIFENSTEIN, JR., and EUGENE DAVIDOFF, Syracuse University

Benzedrine sulfate is a drug of considerable interest because of its stimulating action on the central nervous system. This property of the drug has been successfully utilized in medicine in the treatment of narcolepsy, postencephalitic Parkinsonism, certain depressive mental states and similar conditions. Psychologists are employing the drug to secure additional data on the functioning of the human mind. In addition, benzedrine sulfate has an action on the sympathetic nervous system which we have previously referred to as a pseudo-sympathomimetic effect.

We have used benzedrine sulfate in normal subjects and mentally disordered patients since January 1936.[1] On the basis of this experience we are presenting a summary of the psychological responses in over 200 persons thus medicated. For purposes of description the reactions to the drug may be roughly divided into those relating to mental, to speech, to motor, and to affectual function. We recognize that this division is artificial, and that these responses are manifestations of a total individual reacting as a unit, and therefore cannot be evaluated except in relation to the total situation. In analyzing each case the previous mental status was used as a base line in evaluating the response.

We have characterized the action of benzedrine sulfate as variable, uncertain, unpredictable, and at times paradoxic.[2] From the psychological manifestations enumerated below the pattern of reaction of the central nervous system is readily seen to be similarly variable and paradoxic. To account for such variations we have suggested as a possible explanation (presented in detail in previous communications)[3]

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[*] Accepted for publication July 6, 1938. From the Department of Psychiatry, College of Medicine, Syracuse University, and the Syracuse Psychopathic Hospital, Dr. Harry A. Steckel, Director. Read before Section I, at the Ottawa meeting of the American Association for the Advancement of Science, June 29, 1938. We are indebted to Mr. Theodore B. Wallace of the Smith, Kline and French Laboratories, Philadelphia, for benzedrine sulfate tablets.

[1] Eugene Davidoff, A clinical study of the effect of benzedrine on self-absorbed patients, *Psychiat. Quar.*, 10, 1936, 652-659. E. Davidoff and E. C. Reifenstein, Jr., The stimulating action of benzedrine sulfate: A comparative study of the responses of normal persons and of depressed patients, *J. Amer. Med. Assn.*, 108, 1937, 1770-1776; A method of studying some of the physiologic actions of benzedrine sulfate, *J. Lab. & Clin. Med.*, 23, 1938, 700-711; Treatment of schizophrenia with sympathomimetic drugs: benzedrine sulfate, *Psychiat. Quar.*, 13, 1939, (in press); The results of eighteen months of benzedrine sulfate therapy in psychiatry, *Amer. J. Psychiat.*, (in press); E. C. Reifenstein, Jr., and E. Davidoff, Intravenous benzedrine sulfate as an antagonist to intravenous soluble amytal, *Proc. Soc. Exper. Biol. & Med.*, 38, 1938, 181-184; The treatment of alcoholic psychoses with benzedrine sulfate, *J. Amer. Med. Assn.*, 110, 1938, 1811-1813; The present status of benzedrine sulfate therapy, *N. Y. State Med. J.*, (in press).

[2] *Op. cit.*, *J. Amer. Med. Assn.*, 108, 1937, 1770-1776; *J. Lab. & Clin. Med.*, 23, 1938, 700-711.

[3] *Op. cit.*, *J. Lab. & Clin. Med.*, 23, 1938, 700-711.

that the response of a given individual depends on the interaction of a number of factors among which must be included: the physical, physiological and psychological status of the medicated individual; the several actions of benzedrine sulfate on the central nervous system, the sympathetic nervous system, and possibly on other components of the nervous system; and the factors introduced by the personality and interpretation of the investigator. The rôle of compensatory delayed reactions involving the parasympathetic as well as the sympathetic nervous system and the work of Pavlov and Sherrington must be considered in this connection. We have suggested that the drug renders a given personality more adaptable in a given environmental situation by increasing the number of available pathways of coördinate action.

Effects of benzedrine sulfate. In our experience benzedrine sulfate has produced the following effects.

(1) Stimulation of mental response: (a) Major effects. Acceleration of the stream of mental activity and increase in content of thought; increase in coördinate mental activity and general efficiency; increase in perception and alertness; increase in initiative; increase in attention and meticulousness; decrease in confusion; decrease in sleep requirement and insomnia; decrease in fatigue; decrease in reticence and improved performance on psychometric tests. *(b) Paradoxic effects.* Retardation of the stream of mental activity; inability to concentrate; forgetfulness; dullness; increase in fatigue, malaise and drowsiness; decrease in perception and alertness; decrease in attention; confusion; transitory delirium; tactile and other somatic hallucinations paresthesias and anesthesias, heart consciousness, and generalized sensations of warmth.

(2) Stimulation of speech function: (a) Major effects. Increase in speech activity, disappearance of mutism, talkativeness to the point of loquaciousness; increase in accessibility with willingness to discuss personal problems, ventilation of conflicts, and elaboration of delusions and hallucinations; and increase in coherent and relevant speech. *(b) Paradoxic effects.* Decrease in speech activity; decrease in logical progression of thought with increase in incoherent and irrelevant speech.

(3) Stimulation of motor function: (a) Major effects. Increase in motor activity; increase in coördinate physical activity with increase in physical efficiency; tremulousness, restlessness, extreme hyperkinesis; and aggravation of convulsive seizures. *(b) Paradoxic effects.* Decrease in motor activity with decrease in physical efficiency.

(4) Stimulation of affectual function: (a) Major effects. Elevation of mood with cheerfulness, a feeling of well-being exhilaration, euphoria, elation; uncontrollable laughter; facetiousness; a state of ready irritability and increased irritation; increase in drive, urge and spontaneity; decrease in shyness; increase in confidence; hair-trigger-like reactions with sudden changeability of mood; agitation, impatience, impulsiveness, and self assertiveness approaching aggressiveness in some; aggravation of assaultiveness, and homicidal tendencies in mentally ill persons. *(b) Paradoxic effects.* Lowering of mood with depression or melancholy; uncontrollable crying; aggravation of suicidal tendencies; resentfulness; surliness; anxiety; and increase in worry.

In a given individual we have found that the responses frequently occurred in unpredictable combinations; for example, an annoying restless irritability was often encountered. In general, mental, speech and motor activity is more affected than elevation of mood. There is no preliminary indication as to the type of reaction that will obtain in a given person. In some patients the unpleasant effects appeared to lessen after a few days of medication; in others these seemed to accumulate. Tolerance to the drug is established in some individuals so that the same dose has little effect after a time. We have observed the formation of a habit in certain persons, particularly in neurotic types who crave medication.[4] Individuals with strong alcoholic habits especially relish the drug. The physiological effects appeared to predominate over the psychological in some instances.

These observations on the psychological effects produced by benzedrine sulfate in normal persons and in patients have been confirmed by numerous investigators.[5]

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[4] *Op. cit., Psychiat. Quar.,* 13, 1939, (in press); *N. Y. State Med. J.,* (in press).

[5] M. Prinzmetal and W. Bloomberg, The use of benzedrine for the treatment of narcolepsy, *J. Amer. Med Assn.,* 105, 1935, 2051-2054.

S. A. Peoples and E. Guttmann, Hypertension produced by benzedrine: Its psychological accompaniments, *Lancet,* 1, 1936, 1107-1109.

A. Myerson and M. Ritvo, Benzedrine sulfate and its value in spasm of the gastro-intestinal tract, *J. Amer. Med. Assn.,* 107, 1936, 24-26.

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A. Myerson, Effect of benzedrine on mood and fatigue in normal and neurotic persons, *Arch. Neurol. & Psychiat.,* 36, 1936, 816-822.

J. H. Fisher, Cardiovascular effects of benzedrine sulfate, *Lancet,* 1, 1937, 52.

D. M. O'Connor, Benzedrine, *Brit. Med. J.,* 1, 1937, 43.

M. H. Nathanson, The central action of beta-aminopropylbenzene (benzedrine), *J. Amer. Med. Assn.,* 108, 1937, 528-531.

M. J. Shapiro, Benzedrine in the treatment of narcolepsy, *Minn. Med.,* 20, 1937, 28-31.

D. L. Wilbur, A. R. MacLean, and E. V. Allen, Clinical observations on the effect of benzedrine sulfate, *Prof. Staff Meetings Mayo Clinic,* 12, 1937, 97-104; Clinical observations on the effect of benzedrine sulfate: A study of patients with states of chronic exhaustion, depression, and psychoneurosis, *J. Amer. Med. Assn.,* 109, 1937, 549-554.

P. J. Reiter, Erfaringer on benzedrin (Foreløbig Meddelese), *Ugeskr. f. Laeger,* 99, 1937, 459.

H. Storz and R. Kirk, Über eine neue blutdricksteigernde Substanz, *Deut. Med. Wochnsch.,* 63, 1937, 393.

J. Delmond and R. Vercier, Benzedrine as a stimulant in nervous fatigue and vegitative disorders, *Ann. méd.-psychol.,* 95, 1937, 323-328.

E. Guttmann, and W. Sargent, Observations on benzedrine, *Brit. Med. J.,* 1, 1937, 1013-1015.

P. Solomon, R. S. Mitchell, and M. Prinzmetal, The use of benzedrine sulfate in postencephalitic Parkinson's disease, *J. Amer. Med. Assn.,* 108, 1937, 1765-1770.

C. Bradley, The behavior of children receiving benzedrine, *Amer. J. Psychiat.,* 94, 1937, 577-585.

H. Ulrich, Narcolepsy: Its treatment with benzedrine sulfate, *New Eng. J. Med.,* 217, 1937, 696-701.

P. G. Schube, M. C. McNanamy, C. E. Trapp, and A. Myerson, The effect of benzedrine sulfate on certain abnormal mental states, *Amer. J. Psychiat.,* 94, 1937, 27-32.

In this connection the data accumulated by Gwynn and Yater on the effect of the drug in 147 students is worth citing.[6] The students analyzed their own reactions and concluded that the chief effect of the medication was as follows: psychic stimulation, 71; insomnia, 28; nervousness, 13; with palpitation, headaches, vertigo, dryness of the throat, fatigue, depression, euphoria and epigastric pain as the outstanding response reported by the other subjects. Only two observed no effect. In answer to specific questions the following results were obtained in the same group of students: 126 suffered from insomnia, 113 felt 'peppy,' 81 complained of nervousness, 72 became exhilarated, 61 noticed increased power of concentration, 42 were more talkative, 19 had headaches, 8 observed decreased power of concentration, and a small number recorded vertigo, depression, uneasiness, fullness or throbbing in the head, and fatigue. Schilder, after utilizing the drug during psychoanalysis in further confirmation, states "the results are seemingly due not alone to the structure of the drug but also to the structure of the personality."[7]

The sleep-combatting action of the drug has been clearly demonstrated by the disappearance or amelioration of sleep attacks in patients with narcolepsy.[8] Molitch and Poliakoff utilized this same action in overcoming nocturnal enuresis in juvenile patients who had previously failed to respond to the call to urination because of excessively deep slumber.[9] In previous publications we have demonstrated that benzedrine sulfate produces sufficient stimulation of the central nervous system to arouse patients comatose from mild doses of barbiturates and from alcohol.[10]

Psychometric tests given before and after benzedrine sulfate administration have furnished additional evidence of the stimulating action of the drug on the central nervous system. People and Guttmann found improved performance on the con-

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D. E. Donley, Observation on the use of benzedrine in the psychoses, *Ohio State Med. J.*, 33, 1937, 1229-1232.

J. Hill, Benzedrine in sea-sickness, *Brit. Med. J.*, 2, 1937, 1109-1112.

A. M. Meerloo, Benzedrine sulfate as a brain stimulant, *Nederl. Tijdsch. v. Geneesk.* (Amsterdam), 81, 1937, 5797-5800.

D. J. Sobin, Stramonium plus benzedrine for the relief of postencephalitic sequelae, *J. Speech Disorders*, 2, 1937, 205-206.

L. F. Woolley, The clinical effects of benzedrine sulfate in mental patients with retarded activity, *Psychiat. Quar.*, 12, 1938, 66-83.

P. L. Davis and W. B. Stewart, The use of benzedrine sulfate in postencephalitic Parkinsonism, *J. Amer. Med. Assn.*, 110, 1938, 1890-1892.

[6] H. B. Gwynn and W. M. Yater, A study of temporary use of benzedrine sulfate in 147 supposedly normal young men, *Med. Ann. D. C.*, 6, 1937, 356-359.

[7] P. Schilder, The psychological effect of benzedrine sulfate, *J. Nerv. & Ment. Dis.*, 87, 1938, 584-587.

[8] Prinzmetal and Bloomberg, *op. cit.;* H. Ulrich, C. E. Trapp, and B. Vidgoff, The treatment of narcolepsy with benzedrine sulfate, *Ann. Int. Med.*, 9, 1936, 1213-1221; Davidoff, *op. cit.;* Guttmann and Sargent, *op. cit.;* Nathanson, *op. cit.;* Shapiro, *op. cit.;* W. Bloomberg, The use of benzedrine in postencephalitic Parkinsonism (discussion), *J. Nerv. & Ment. Dis.*, 85, 1937, 202; H. N. Tihen, Treatment of narcolepsy with benzedrine sulfate, *J. Kansas Med. Soc.*, 38, 1937, 208; Ulrich, *op. cit.;* D. Young and W. B. Scoville, Paranoid psychosis in narcolepsy and possible danger of benzedrine treatment, *Med. Clin. N. Amer.*, 22, 1938, 637-647.

[9] M. Molitch and S. Poliakoff, The effect of benzedrine sulfate on enuresis, *Arch. Ped.*, 54, 1937, 499-501.

[10] *Op. cit., Proc. Soc. Exper. Biol. & Med.*, 38, 1938, 181-184; *Psychiat. Quar.*, 13, 1939, (in press).

tinuous addition test.[11] Sargent and Blackburn reported that 46 mentally disordered patients showed an average increase of 8% in the response to Cattel's Intelligence tests.[12] Molitch and Sullivan found the performance of 50 boys on the New Stanford Achievement Test improved in 96%.[13] Molitch and Eccles[14] found improved performance in 93 boys in tests of the strength of grip (Smedley dynameter), motor coördination (Witmer form board test), memory for design (reproduction of card designs), and one of three highly standardized intelligence tests (Kent E.G.Y., Morgan, and Kuhlman-Anderson). Bradley observed improvement in school performance in half of 30 behaviour problem children.[15] We have noted more elaboration of detail and more coördination of activity in the Rorschach test in two cases.[16] McNamara and Miller were unable to demonstrate better performance in arithmetic problems;[17] their period of study was not, however, sufficiently prolonged, and the observations were made at too long an interval after the administration of the drug. Barmack,[18] with a similar study of arithmetic problems, demonstrated that benzedrine sulfate increased the amount but not the efficiency of mental work performed, and prevented the development of boredom, strain, irritation, fatigue, sleepiness, and inactiveness in continuous, repetitive work. He concluded that the effect of benzedrine was principally on the inclination rather than on the ability to perform such tasks.

In our experience, persons with inherently low mental levels showed little alteration in intelligence scores after benzedrine sulfate, although patients with early toxic or infectious psychoses were apt to increase their rating as the abnormal mental state cleared. It seems likely that the general efficiency rather than the specific ability is influenced by the drug in most cases.

Relatively few studies of the effect of benzedrine sulfate on the central nervous system have been made on animals. Alles,[19] and Detrick et al.[20] observed that benzedrine sulfate tended to awaken animals from anesthesia with barbiturates. Ehrich and Krumbhaar reported that the drug subcutaneously produced excitement in albino rats.[21] The animals began to run around excessively, gnaw their cages and splash in their dishes of water. Some showed very queer movements, ran backwards or in circles, had difficulty walking, often sat on their hind legs pawing the air and falling over backwards. All of the animals fought a great deal more than the control rats. Two were found gnawing their own thoracic skin. Skinner and Heron stated that benzedrine sulfate would restore to full efficiency a conditioned reflex which had been allowed to become partially extinguished by a number of days of non-performance.[22] That the drug would cause rats to produce a greater output of work was clearly demonstrated by Zieve.[23] His animals were confined in revolving activity drums, the revolutions of which were counted before, during and after the drug had been administered. Approximately twice as much work was performed by the animals under the influence of the drug. Wentink reported that benzedrine sulfate increased the rate of response of rats to conditioned reflexes 142%.[24]

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[11] Peoples and Guttmann, *op. cit.*

[12] W. Sargent and J. M. Blackburn, The effect of benzedrine sulfate on intelligence scores, *Lancet,* 2, 1936, 1385-1387.

[13] M. Molitch and J. P. Sullivan, The effect of benzedrine sulfate on children taking the new Stanford achievement test, *Amer. J. Orthopsychiat.,* 7, 1937, 519-522.

[14] M. Molitch and A. K. Eccles, The effect of benzedrine sulfate on the intelligence scores of children, *Amer. J. Psychiat.,* 94, 1937, 587-590.

[15] Bradley, *op. cit.*

[16] Davidoff and Reifenstein, *op. cit., Amer. J. Psychiat.,* (in press).

[17] W. J. McNamara and R. E. Miller, The effect of benzedrine sulfate on mental work, *Psychol. Rec.,* 1, 1937, 78-84.

[18] J. E. Barmack, The effect of benzedrine sulfate (benzyl methyl carbinamine) upon the report of boredom and other factors, *J. Psychol.,* 5, 1938, 125-133.

[19] G. A. Alles, The comparative psychologic actions of dl-B-phenylisopropylamines: I. Pressor effect and tozicity, *J. Pharmacol. & Exper. Therap.,* 47, 1933, 339-354.

[20] L. E. Detrick, R. Millikan, F. S. Modern, and C. H. Thienes, On the pharmacology of phenylisopropylamine (Benzedrine), *ibid.,* 60, 1937, 56-68.

[21] W. E. Ehrich and F. B. Krumbhaar, The effect of large doses of benzedrine

Although none of our subjects were aware of the nature of the medication or of the symptoms produced by it, the psychic effect of the administration itself cannot be disregarded entirely. The following are illustrative cases.

Case 1 (Normal). Four hours after the first medication the subject became very active. He telephoned for flowers to be sent to a sick friend, but shortly after, becoming impatient, he hurried several miles to the florist shop and delivered the gift in person. He said "I wanted action, and I got it!" Later he complained of fatigue. He slept well that night. The second day he noticed a general feeling of depression. The next day he was restless "felt like reeling" and was mentally dull and retarded. On the fourth day he "felt terrible and groggy. On the next day he was "pepped up" and mentally alert. The following day he was annoyed by trivial errors in his work. Throughout the remainder of the period he continued to sleep poorly, made many of the same complaints and toward the end became forgetful.

Case 2 (Normal). Four hours after medication on the first day the patient complained of a 'nervous' feeling in the stomach, followed by abdominal pain and nausea. Two hours later she felt better, was more active and ambitious, but was restless. She slept only three hours. On the next day shortly after the medication she became more active but was not able to accomplish much and described herself as 'flighty.' She stated that trivial matters appeared exaggerated to her. That evening she was fatigued, and during the night again slept poorly. On the third day she found it an effort to work, complained of restlessness, exhaustion and excessive fatigue. In the evening, however, she was more active and cheerful. On the following day she noticed marked tremor of the hands; later she was active and had a desire to complete her work, but noticed difficulty in connecting her words. In the evening although she felt much fatigued, she was not sleepy and continued active until 4 A.M. The next morning she complained of vertigo, partial syncope, and

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sulfate on the albino rat: Functional and tissues changes, *Ann. Int. Med.,* 10, 1937, 1874-1888.

[22] B. F. Skinner and W. T. Heron, Effect of caffeine and benzedrine upon conditioning and extinction, *Psychol. Rec.,* 1, 1937, 340-346.

[23] L. Zieve, Effect of benzedrine on activity, *ibid.,* 1, 1937, 393-396.

[24] E. A. Wentink, The effects of certain drugs and hormones upon conditioning, *J. Exper. Psychol.,* 22, 1938, 151-163.

extreme anxiety. She became very fearful and at her request the medication was discontinued. She continued to complain of excessive fatigue for several days thereafter.

Case 3 (Manic depressive, depressed). Prior to medication the patient was depressed, physically retarded, underproductive, hopeless in his outlook and lacking in ambition. He became talkative within an hour after taking the drug. Three hours later he was "feeling much better" but complained of fatigue. He did not sleep well. The second day he appeared depressed three hours after taking the drug, but from six to eight hours later he stated that he felt "good" and he slept well that night. On the fourth day he remarked that he was very happy, cheerful and alert. On the sixth day he was jovial. By the tenth day he was singing; he stated that he felt "great" and appeared to have reached approximately his normal state. He was quite active. On the twelfth day he was depressed shortly after taking the drug but toward evening appeared to be in better spirits. On the fourteenth day he was very talkative but complained of depression. Throughout the period he worked more efficiently in the ward. In general his mood appeared less affected than his speech response, motor activity and general efficiency.

Case 4 (Dementia precox, catatonic type). Prior to medication the patient was markedly underproductive, self-absorbed, very retarded, inactive, untidy, apathetic, dull, listless, timid, and passively resistive. She refused to move or to do anything and had to be spoon-fed. She experienced delusions, and auditory hallucinations.

Two hours after receiving the drug on the first day the patient began to talk and told her husband that she felt uncomfortable. The next day she became more resistive and uncoöperative, but worked a puzzle, smiled occasionally and stated that she felt better. That night she was unable to sleep, became fearful and restless, and finally much agitated, and begged to be taken home. The following day she began to eat spontaneously, assisted willingly with the ward work, and became more talkative and coöperative. She recognized her hallucinations as 'queer' ideas, which she stated were disappearing. She was more alert, more appreciative of her surroundings and smiled spontaneously, but continued fearful at times. On the morning of the seventh day of medication the patient complained of feeling worse, was tearful, more uncoöperative and fearful but assisted in the hospital work. On the following day she was resistive, agitated and uncoöperative prior to medication, but afterward became cheerful and talkative. In the evening she was seclusive and was unable to sleep. After two weeks the patient assisted with the ward work, and applied herself well in occupational therapy, but was able to sleep only three hours at night and continued irritable, confused, and fearful of impending disaster. After the 25th day of medication the patient began to show definite improvement, took interest in her personal appearance, showed more purposeful activity. She continued to become more coördinate in her activities, became interested in her surroundings and began to ask questions, was cheerful, friendly and talked freely about herself. She remarked that she felt like a different person.

Case 5 (Alcoholic psychosis, delirium tremens). Prior to medication the patient was confused, delirious, disoriented, overproductive, complained of visual hallucinations of snakes and worms four to eight feet long which he was pulling from his mouth, and of elephants which he saw on the wall; he then lapsed into stupor. After a dose of benzedrine sulfate intravenously he aroused from the stupor and began again to talk of the snakes. Following another dose of the drug he suddenly remarked that the snakes had disappeared, became better oriented, much less confused, but very restless. In several hours he was cheerful and happy, stated that he felt very well, was facetious, talkative and spoke relevantly and coherently of the events prior to his admission. The next day he assisted in the ward work, attended occupational therapy, occupied himself in reading magazines and listening to the radio. He was discharged to his home ten days after admission, completely recovered.

Case 6 (Normal). Within six hours the subject noted increased activity and alertness. He called his superior officer on the telephone, insisted on taking an examination for which he was not eligible and became quite agitated during the discussion. He was embarrassed about the incident a few hours later. On the third day he noted exaggerated and prolonged physical and mental alertness. On the fourth day general sluggishness and malaise. On the seventh day he was depressed. After the eighth day he observed, in addition, increased physical and mental activity; feeling of warmth; and decreased requirement of sleep. After the drug was discontinued he felt depressed and fatigued for several days.

Case 7 (Traumatic psychosis, post-traumatic personality disorder). Prior to medication the patient was confused, depressed, dull and apathetic, retarded, quiet, rather uncommunicative, but talked about his accident and experienced auditory hallucinations. On the first day of medication he was worried, restless, anxious and complaining. On the the next day he was fatigued and weak, but much more talkative and friendly with marked improvement in recent and remote memory. At times he appeared moderately depressed, and seclusive, stating that he did not feel like living. The following day he was extremely talkative, more active, and willing to assist in the ward work. His IQ rating was found to have risen from 68 to 79. The next day he felt much better, laughed, told his story freely, and was cheerful and coöperative. He continued to improve during the rest of his stay in the hospital.

Case 8 (Psychoneurosis, hysteria). Prior to medication the patient was quiet, depressed, anxious and tearful, with retarded speech. He was harrowed by obsessional memories of war experiences and marital difficulties, and had numerous somatic complaints. Within four hours he became perturbed and agitated. He appeared to have a marked reaction and voluntarily went to bed. Motor activity was much diminished and he fell asleep for about an hour. The next morning, within three hours he complained of fatigue. He again experienced a reaction and voluntarily went to sleep. Several hours later, however, he felt much better and became very talkative. He experienced the same symptoms on the third and fourth days but became more cheerful. The fifth day he appeared delirious for a time and complained of "water ebbing up and down his body to his neck." These sensations soon disappeared. The next day he could not remember anything that had happened and was confused. On the seventh day he was improved and became jovial and talkative.

although the motor activity increased only slightly, and there appeared to be no change in his general efficiency. Toward the end he was occasionally drowsy.

Case 9 (Depressed alcoholic). Prior to medication the patient was depressed, physically retarded, dull and underproductive. One hour after taking the drug he felt considerable motor and mental acceleration and stated that he felt "good." There was a marked increase in talkativeness. At the end of two hours he was confused and unsteady on his feet but insisted that he was feeling "very strong." After three hours he was restless, talkative, whistled constantly, appeared very cheerful and for the first time since his admission he asked to go on the porch. He continued to be cheerful and to pace about for the rest of the day.

Case 10 (Dementia precox, paranoid type). Prior to medication the patient was quiet, seclusive, and depressed. He had attempted suicide with phenobarbital. A few hours after taking benzedrine sulfate he became more active, and cheerful. On the second day he was irritable and depressed, seclusive, with diminished physical activity. Later he was restless and unable to sleep. On the third day he was more talkative, and more active, walking up and down the hall. However, he became fearful and complained of anorexia. On the next day he was much more cheerful and coöperative but stated that he felt 'nervous.' On the fifth day he expressed regret for a previous outburst of temper, showed considerable physical activity, laughed and talked considerably, began to sing to himself. He slept well that night. The next morning he complained of feeling 'terrible' and had nausea, cold hands and excessive restlessness. The following day he was better again, and was cheerful and coöperative. During the rest of the period of administration he continued to have variable reactions, at times cheerful, coöperative and physically active; at other times restless and irritable. At this time he uncovered in considerable detail his paranoid reactions toward his family, his physicians, and the hospital, all of which he had previously withheld.

SUMMARY

Benzedrine sulfate stimulates the central nervous system, producing a variety of psychologic reactions which are unpredictable and at times paradoxic. A description of these reactions in 200 normal persons and mentally deranged patients is herein presented, with illustrative cases.

The stimulating effect of the drug has been demonstrated clinically in the amelioration of narcoleptic sleep attacks, in the dispelling of coma due to barbiturates and to alcohol, and in the increased performance on psychometric tests. Limited confirmatory data has been obtained from a few animal experiments. The need for further controlled objective psychologic investigation of the action of benzedrine sulfate is indicated.

FORGETTING OF MEANINGFUL MATERIAL DURING SLEEP AND WAKING

By EDWIN B. NEWMAN, Swarthmore College

The marked difference between the forgetting curves obtained during intervals of sleep and waking argues very strongly against the conventional theory that accounts for forgetting in terms of the law of disuse.[1] This difference suggests rather that the process of forgetting is an active process influenced by activities interpolated between original learning and testing. If this conclusion is accepted, and it has been demonstrated conclusively by a number of recent writers,[2] the comparison of forgetting curves obtained during sleep and during waking should give us an instrument with which to study the effectiveness of various factors essential to retention.

Using meaningful material in some preliminary experiments, we attempted to obtain results similar to those of Jenkins and Dallenbach and of Van Ormer. Short stories were read by the observers and reproduced after equal intervals of sleep and waking. No reliable difference was found between the two conditions, a result which we were inclined to attribute to faulty technique. Control experiments and more careful analysis of the results, however, showed that our technique was not faulty, and that a consistent and reliable difference between meaningful and nonsense material was to be expected. To make this comparison more clear, the following formal experiments were performed.

EXPERIMENTAL PROCEDURE

The material used consisted of three short stories, each about three hundred words in length and constructed so that it contained a number of items which were not essential to the plot. After some experimentation, the material was so arranged that each story contained an approximately equal number of essential and non-

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[1] This study is the result of work done in undergraduate honors seminars at Swarthmore College. Credit is due to Louise Watkins for the original distinction between essential and non-essential material, to Deborah Wing for the final form of the stories used, and to Marion Rous and Margaret Chase for a major part of the experimentation and the final scoring.

[2] The earlier work on sleep and forgetting has been adequately summarized by E. B. Van Ormer, (Retention after intervals of sleep and of waking, *Arch. Psychol.*, 21, 1932, (no. 137), 1-49) and will not be repeated here. Cf. also, R. Heine, Ueber Wiederkennen und rückwirkende Hemmung, *Zsch. f. Psychol.*, 68, 1914, 161-236; and J. G. Jenkins and K. M. Dallenbach, Obliviscence during sleep and waking, this JOURNAL, 35, 1924, 605-612. The theory that forgetting is a function of retroactive inhibition has been clearly stated in J. A. McGeoch, Forgetting and the law of disuse, *Psychol. Rev.*, 39, 1932, 352-370, and in subsequent papers by the same writer.

essential points. The stories are reproduced below. The essential material is printed in italics, and the twelve parenthetical numbers mark the twelve essential points used in scoring. The non-essential material was divided correspondingly into twelve non-essential points.

THE LONG EXILE (L)

One beautiful day in the middle of summer *a young farmer named Ivan started out from his farm for the fair* (1) singing gaily, for he had many vegetables to sell. *On the way he met a merchant and they journeyed together, stopping at the same hotel for the night* (2). The next morning Ivan started off early while the sun was just rising, but as he was riding happily on his way, *he was stopped by some soldiers who questioned him carefully as to his whereabouts the night before* (3). *In searching his belongings they found a blood-stained knife in his bag* (4). *To Ivan's frantic questioning, they finally told him that the merchant with whom he had spent the night had been found murdered in his bed* (5). By questioning all who might know about the case, *the police had concluded that, since Ivan was the only one that had been with the merchant, the guilt pointed to him* (6). *With this evidence he was exiled for life* (7), never again to see his family and friends or the beautiful fields of his own town. Many long years he was in prison, gradually growing older and grayer while the best part of his life slipped away from him slowly. *One day a new group of convicts arrived to pay the penalty for their crimes* (8). There were a great many of them—old and crippled, young and in good health. In talking to them as was his custom with a new group, *Ivan chanced upon the startling and amazing discovery that one of these convicts was the real murderer of the merchant, for whose death he, Ivan, was paying the penalty* (9). On this discovery his whole soul was torn in a great struggle; *here was his chance to be set free* (10), to see his homeland again; yet with a sickening heart he realized what it would mean to the other convict, dooming him to life imprisonment in place of his original short sentence. The convict had just left his wife and children who were sick from the famine and needed his care. He hoped to return soon, whereas there was no one that needed Ivan or would be there to welcome him after so many years. With calmness and deliberation he kept his secret to himself and worked steadily in the mines, growing feebler and feebler. *After some time the real murderer, weighed down by his own conscience, confessed his guilt* (11). *On the day they came to release Ivan,* a day in the middle of spring, that was sunny and warm, *they found that he was dead* (12).

THE PERFECT ALIBI (P)

One day a group of three army intelligence officers walked into the office of detective Jim Dudley, and asked his assistance. *A sergeant at Camp-Dix had disappeared three months before* (1). During that time they had tried to locate him, but with no success. *They had just now discovered his body in a clump of bushes, and it was clear that he had been murdered* (2). Dudley arrived on the scene with a large staff of detectives that started to work. *They soon established the fact that the victim must have been slain by someone in his own company* (3). However, no one was found who had any particular motive for killing him, but it was established that he had been a small-scale loan shark. *The next step was to question each man in the company as to his actions on the day of the sergeant's disappearance* (4). A stenographic report was made of each man's story to be used for future reference. *In examining the stories, those men who were unable to tell anything about their activities on the day were immediately dismissed from consideration* (5). Naturally, the news was received with many sighs of relief. *However, there were ten who could give some account of their activities on that day, and these men were questioned for further details* (6). The preliminary reports hadn't furnished any definite clues to the murderer. *This time nine became hopelessly involved in contradictions and they were eliminated* (7). Since Dudley's work was carried on secretly, no one yet knew clearly the purpose behind it all. *He discovered that the tenth man was still sure of his actions, not only on the day of the murder, but the day after and* *before* (8). When questioned he showed no signs of nervousness or guilt. *But Dudley knew he had his murderer* (9), *for nobody who was not repeating a story he had made up and memorized could remember that much about his action three months before* (10). If anyone should try it they'd find it almost impossible to remember the barest outline. *Having spotted his man in this way, Dudley found that the sergeant had lent money to the prisoner who was finding it hard to repay* (11). *He then had little difficulty in forcing the prisoner to confess that to wipe out the debt he had wiped out the sergeant* (12). The confession was carefully recorded and signed by the prisoner to prevent any repudiation.

The Work of Art (W)

One cold day in January, *Sasha burst into the doctor's consulting room carrying a big package under his arm,* carefully wrapped up in newspaper and tied with a piece of heavy cord. *When the doctor came out, he handed him the bundle* (1), patting it gently as though it were very precious, and *sputtered out, "Mama says to thank you for saving my life. We want you to have this present as a token of our gratitude"* (2). *He explained that it had been left in his father's antique shop when he died and they had kept it as a souvenir ever since then* (3). "Mama and I keep on my father's business now," he added. Sasha unwrapped the present for the doctor, taking the paper away carefully as though wishing to create suspense. *What was revealed was a candelabra* (4) of old bronze and artistic workmanship. *On the pedestal stood two female figures in the costume of Eve and in attitudes for the description of which one would have neither the courage nor the fitting temperament* (5). The doctor, taking in the figures, spoke in a non-committal tone to the effect that it was a fine piece of work well done; *but he tried to refuse the gift, saying that it wasn't quite proper for his consulting-room* (6). *Sasha not only wouldn't listen to his refusal, but he said sadly that his only regret was that he didn't have the other member of the pair* (7). He spoke in reverent tones of art that should be appreciated for its own sake. *When Sasha went away, the doctor, still having the gift, determined to give it to a bachelor-friend of his who might appreciate it* (8). The bachelor accepted the gift, but concerned with his own reputation, *decided he'd better pass it on to a comedian that he knew, who might have more use for such a piece of art* (9). He took it to the stage dressing-room where the present was greatly admired by all his friends. *The comedian realized he couldn't keep it, and when his hairdresser, who always had good ideas, suggested selling it to an antique shop owned by a woman and her son, he carried out her suggestion immediately* (10). After two days, when the doctor had completely forgotten the episode, *Sasha burst into his room with a big bundle under his arm, crying, "Doctor, happily for you we have succeeded in picking up the pair to your candelabra* (11). Mama is so happy that we can make our present complete," and he set down the candelabra before the doctor. *The doctor tried to say something, but said nothing. He could not speak* (12).

Eleven *Ss*, all college students, took part in the final experiment. They read each story at a different time of day and reproduced it approximately 8 hours after reading. The order of the stories and the relation between the story learned and the time of learning was rotated for the various *Ss*, and the order for each *S* is indicated by the letters in Table I. Comparisons were not made at other than the 8-hr. interval because we expected to find the maximum difference between sleep and waking with this interval. The scoring of the stories was done by three independent judges in order to obtain as great objectivity as was possible. Each judge was given a list of the 12 essential and 12 non-essential points and was asked to compare the reproduction with the original story in terms of these points. It was found that the judges were in substantial agreement about the amount of material reproduced, even though they disagreed in some instances about individual points. The percentages of Table I are means of the ratings of the three judges.

Results

The results of the experiment are presented in Table I, in which every percentage in the body of the table represents the proportion of the items recalled by one *S* in his reproduction of one story. In the last line are the median results for each kind of material at the different times of day. These show first that the essential material is always reproduced much better than the non-essential material: the averages are 86% and 30% respectively. However, the *essential* material is reproduced equally well at the three different times of day. Eighty-seven percentage of the material was reproduced after 8 hr. of sleep, while 84% and 87% were reproduced after 8 hr. of waking activities during the morning and afternoon respectively—a result which confirms our preliminary findings. On the other hand, the *non-essential* material shows a different picture. After 8 hr. of sleep, 47% of the material was reproduced, while but 25% and 19% was reproduced after the two waking intervals. Almost exactly half as much material was reproduced after the waking intervals as was retained after sleep. The non-essential material behaves, therefore, in very nearly the same manner as did the nonsense syllables of previous investigators.

TABLE I

Percentage of Material Reproduced by Each S after an Interval of Eight Hours

Morning and Afternoon=reproduction after waking activities. Night=reproduction after sleep. Every percentage is the mean of ratings by three judges. L, P and W indicate the story which was used as material.

S	Essential material			Non-essential material		
	Morning	Afternoon	Night	Morning	Afternoon	Night
A	P 68	L 96	W 74	P 19	L 46	W 47
B	W 84	P 81	L 92	W 25	P 19	L 33
C	L 93	W 74	P 85	L 18	W 10	P 32
D	L 95	W 88	P 78	L 41	W 17	P 48
E	P 74	L 94	W 85	P 35	L 19	W 36
F	W 93	P 83	L 96	W 23	P 18	L 40
G	W 70	L 87	P 78	W 25	L 33	P 48
H	L 71	P 84	W 87	L 27	P 15	W 47
I	W 70	P 72	L 92	W 0	P 11	L 29
J	L 92	W 99	P 93	L 64	W 86	P 72
K	P 93	L 95	W 100	P 48	L 65	W 80
Median	84	87	87	25	19	47

Detailed statistical comparisons are scarcely justified with such a limited number of cases. The scores compared for each individual *S* show, however, the same result as do the scores for the group. With the non-essential material, 10 of the 11 *S*s show a clear-cut superiority for the sleeping interval over the average of the day intervals. With the essential material, 7 *S*s show some superiority for the sleeping interval, while 4 reproduced more after the waking interval. *J* represents an exception to the general trend, largely because she reproduced all the stories with almost verbatim exactness.

Discussion

The results of the present experiment lend support to the contention that forgetting is essentially a matter of interference—proactive or retroactive inhibition. There is a clear-cut parallel between forgetting and inhibition in the results for the non-essential material; this material which is much less well remembered when compared with the essential material is also the material which shows the marked gain as a result of interpolating sleep rather than waking. Forgetting is associated with inhibition in this case. Up to this point the findings and interpretations are in agreement with those of Heine, Jenkins and Dallenbach, and Van Ormer; but now we can go one step further, showing that meaningfulness of the material is an important condition of retroactive inhibition. Material as meaningful as the stories used in these experiments has inherent in it some factor which effectively prevents retroactive inhibition.

This conclusion is not particularly surprising. It is implicit in the relationship which has been assumed to hold between forgetting and retroaction when we recall how little forgetting usually occurs with meaningful material. McGeoch and McKinney found relatively small amounts of retroactive inhibition when using poetry and prose as their learning materials.[3] On the other hand, other studies have pointed to quite the opposite conclusion. McGeoch and McDonald showed that the inhibitory effect of interpolated material could be increased by making each item in the interpolated list a synonym of the corresponding item in the original list, and that this inhibition was the stronger, the more perfectly were the two words synonymous.[4] That this effect was the result of the meaningfulness of the words and not the result of serial position was clearly shown by McGeoch and McGeoch.[5] Thus, in these experiments the increase of meaningfulness of the material increased rather than decreased interference.

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[3] J. A. McGeoch and F. McKinney, Retroactive inhibition in the learning of poetry, this Journal, 46, 1934, 19-33; The susceptibility of prose to retroactive inhibition, *ibid.*, 429-436. They state: "To ask why poetry should suffer so small a loss from retroactive inhibition is tantamount to asking why meaningful material is forgotten less rapidly than non-meaningful, an inquiry which points beyond the intent of our discussion" (p. 28). They suggest, however, that "it may be that relative degrees of organization [*sic!*] of the original and interpolated materials are very important in determining amount of retroactive inhibition" (p. 31 f.).

[4] J. A. McGeoch and W. T. McDonald, Meaningful relation and retroactive inhibition, this Journal, 43, 1931, 579-588.

[5] J. A. McGeoch and G. O. McGeoch, Studies in retroactive inhibition: VI. The influence of the relative serial positions of interpolated synonyms, *J. Exper. Psychol.*, 19, 1936, 1-23.

This ambiguity in prediction that is introduced by the meaningfulness of the material requires some hypothesis about the nature of meaningfulness and the way in which it operates to aid or hinder learning. A theory of meaning in terms of associated context, the items of which are connected by indifferent bonds to the verbal core, has difficulty in explaining these results. If a mere increase in the number of bonds between the verbal core and other items outside the list to be learned can be so effective in producing inhibition (or negative transfer) in the case of the synonyms, why should it not be equally effective in the case of the prose and poetry? It must be clear that some concept other than that of associations, bonds, identical elements or the like must be supplied to differentiate between the two sets of results. Such a concept would require that meaning reside in the structure or organization of the material to be learned, in the organized 'sense' of the story, in a 'context' of meaning which does not consist of indifferent bonds. This view would hold that meaning *may* consist in part of 'contextual' material, but it would add two things. The first is that the relations of core to context are integral with the material, consisting of such relations as equivalence, inclusion, superordination, subordination, centeredness, and terminal, intermediate, progressive or symmetrical character. The second is that the *totality* of these relations frequently, if not invariably, forms a structure which has whole-properties of its own. The neatness of the solution to a problem, the incompleteness of a continued story are limited illustrations of such whole-properties.

The case for this concept of meaning is particularly strong in the present experiment. It cannot be argued that the difference between the more meaningful and less meaningful material is to be found in the number of previous associations with the individual units of sensible or nonsense material. No one could reasonably hold that the particular words which form the non-essential items had numbers of connections different from the words comprising the essential items. It must be clear that the structure of the story as a whole and the relation of the individual items to this structure are the determining factors which account for the differences found in this experiment.

Conversely, this concept is able to explain the interference in the case of the synonyms as interpolated material. The series of adjectives forms at best a sequence in which the relation of each item to the succeeding one is extremely peripheral in the meaningful structure of each word. On the other hand, there is a relation between the synonyms in the two lists which is not only strong, but which has the additional property of being central to the meaningful structure, and also the even more important property of the symmetrical relation of equivalence. Finally, there is in the list as a whole no structure which may serve as a framework for the individual adjectives. The sequence of adjectives is, in our sense, meaningless.

An excellent example of the operation of just such factors as those mentioned above is to be found in the experiments of Köhler and Von Restorff in which it was shown that interference or inhibition is produced, even within a single series, by the massing of equivalent, homogeneous material.[6] This interference is reduced when the material is heterogeneous, but a much larger reduction is obtained when the series has at least a minimal amount of organized character—in their experiments, a figure-ground character where one heterogeneous element stands out on a homogeneous ground. Correspondingly, the present results show that the organization furnished by the meaningful plot of the story is effective in preventing that state of crowding of unrelated items which in other situations is the essential condition for interference and for forgetting.

SUMMARY

(1) Ideas essential to the plot of a story are reproduced equally well after intervals of sleep and waking (87% compared with 86%), while non-essential items are reproduced twice as well after an interval of sleep as after an interval of waking (47% compared with 23%).

(2) This result argues that the organized character of the material is a condition significantly affecting retroactive inhibition and forgetting.

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[6] H. von Restorff, Ueber die Wirkung von Bereichsbildungen im Spurenfeld, *Psychol. Forsch.*, 18, 1933, 299-342, esp. 313-328.

OXYGEN-TENSION AND 'THE HIGHER MENTAL PROCESSES'

By MADISON BENTLEY, Library of Congress

Recent physiological studies by Ernest Gellhorn and associates bear upon the effect on the central nervous system of reducing oxygen in the respiratory air. These studies fall within a large experimental literature of the last two decades, a literature which Gellhorn found to be defective on the side of sensory damage done by low oxygen-tension.[1] Once embarked upon the quest for local damage, however, this physiologist was carried from the senses to the brain and from the brain to that indefinite and dangerous region of 'the higher mental processes.' As regards central effects of low alveolar oxygen, this author came to believe that "such divers functions of the central nervous system as visual intensity discrimination, the latent period and intensity of negative after-images, auditory acuity, muscular coördination, associations and simple mental tasks . . . are impaired under approximately the same degree of oxygen deficiency."[2]

As to the effect upon the 'simpler sensory functions' of stimulus, differential limen, and time-interval between photic excitation of the eye and certain after-effects, little need be said in this context beyond the observation that the Gellhorn experiments have neglected many checks, refinements, and methodical precautions which careful psychologists have found to be necessary in such determinations.[3]

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* Accepted for publication June 1, 1938.

[1] E. Gellhorn and I. Spiesman, Influence of variations of O_2 and CO_2 tension in inspired air upon hearing, *Proc. Soc. Exper. Biol. & Med.*, 32, 1934, 46-47.

[2] E. Gellhorn, The influence of carbon dioxide in combatting the effect of oxygen deficiency on psychic processes, with remarks on the fundamental relationship between psychic and physiologic reactions, *Amer. J. Psychiat.*, 93, 1937, 1413-1434. Other references important in the present context are E. Gellhorn, The integrated action of the organism exemplified by studies on anoxemia, *Sigma Xi Quart.*, 25, 1937, 156-165; E. Gellhorn and A. Joslyn, The influence of oxygen want, hyperpnea, and carbon dioxide excess on psychic processes, *J. Psychol.*, 3, 1937, 161-168. Experimental procedures are related in *Amer. J. Physiol.*, 112, 1935, 519-528, 620-626, 662-668; 115, 1936, 679-684; 117, 1936, 75-78, and in E. Gellhorn and S. H. Kraines, Word associations as affected by deficient oxygen, excess of carbon dioxide and hyperpnea, *Arch. Neurol. & Psychiat.*, 38, 1937, 491-504. In his orientation, Gellhorn had the advantage of a general review of the subject in R. A. McFarland, The psychological effects of oxygen deprivation (anoxemia) on human behavior, *Arch. Psychol.*, 22, 1932, (no. 145), 1-135 (with 191 titles).

[3] A single instance of the great methodical difficulties encountered in the measurement of acuteness and range of apprehension of the sensible qualities of objects is A. D. Freiberg, 'Fluctuations of attention,' *etc.*, this JOURNAL, 49, 1937, 23-36, 173-197. With respect to the neurologist's approach to psychological problems, see T. A. Ryan, Neural resources of psychological performance, *ibid.*, 48, 1936, 193-220; M. Bentley, The psychologist's uses of neurology, *ibid.*, 49, 1937, 233-264 (esp. 235-245,

When experimenting upon those neural processes and states which suggest the 'phenomena' which they distantly call 'psychic' or 'mental' (usually prefixed by a 'so-called'), the physiologist and the functional neurologist are wont to imply that sensations and sensory differences are organic things to be plucked out of the body by the subject, who names them 'red,' 'green,' 'higher,' 'the same,' 'different,' and the like; but when something 'really psychical' is at hand (as in remembering, associating, and thinking), the physiologist then reluctantly brings in a ghostly mind, which proceeds to use its 'higher mental powers.'[4] An inevitable result of this misconception of matters psychological is that the determination of 'sensations,' 'discrimination,' and other 'sensory phenomena' is assumed to involve nothing more than the designation of a physical agent (stimulus), of a physiological effect upon the receptoral organ, a local stir-up in the brain, and a simple report upon quality, duration, difference, or the like. The psychological side of 'sensory' experimenting is, as a matter of hard fact, much more complicated than that, complicated by factors, over and above 'stimulus' and an *Elementarfunktion,* which are in play during the experimental occasion and complicated again by reason of the difficulties of expert and adequate report from the subject or observer.[5]

Beyond the problem of primary central effects of a reduction of oxygen-tension, however, Gellhorn has carried his researches into the realm of psychophysiological relationships, to the "fundamental problem as to how physiologic and psychic processes are interrelated."[6] There he seeks "to establish the dependence of psychic phenomena on physiologic factors by quantitative methods," "to determine the fundamental factors upon which the integrity of psychic processes depends, and to investigate whether any quantitative alteration in any one such factor alters both physiologic and psychic processes."[7]

Had Gellhorn been familiar with the history of experimental studies

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260-261), The nature and uses of experiment in psychology, *ibid.,* 50, 1937, 452-469 (esp. 456-459), and Retrospect and prospect, *ibid.,* 51, 1938, 357-360. As a comment upon the physiologist's 'double-man' view of the organism, see M. Bentley, Mind, body, and soul in medical psychology, *ibid.,* 45, 1933, 577-591 (esp. 581, 586-588), and *The Problem of Mental Disorder,* 1934, 281-289.

[4] In combatting the opinion that some such 'psychic' agent as attention is responsible for the auditory effects of anoxia, Gellhorn argues for "changes in the nervous mechanism involved in the sensation itself" (*Amer. J. Physiol.,* 112, 1935, 524). It is to be doubted whether theories which set attention against sensation, with corresponding neurological speculations over 'seats' and 'elementary functions,' are any longer useful.

[5] Upon the difference between 'observer' and 'subject' in psychology, see comments by the present writer in this JOURNAL, 41, 1929, 682-683; 42, 1930, 320, and in his *New Field of Psychology,* 1934, 29, 270-274.

[6] *Amer. J. Psychiat.,* 93, 1937, 1413.

[7] *Ibid.,* 1414.

of psychophysical relationships, from E. H. Weber and G. Th. Fechner to the present, it is likely that he would have approached his experimental program with more sophistication and greater reserve.[8]

What really appears from the more 'psychological' studies of Gellhorn (when stripped of the indefinite and distant reference to 'psychic phenomena') is that the organism's capabilities in perceiving, distinguishing, and reporting-upon simple objects and their simpler aspects are infringed in certain ways and to a describable degree when respiration is modified *in a given experimental context;*[9] for in all such performances of the organism *context* must be taken into account quite as carefully as reduction of oxygen or increase of carbon dioxide. As every careful observer of the psychological operations of the body now knows, procedure and issue of organic activity rest quite as much upon the task set and upon the formal, occasional, and self-initiated instructions under which the body proceeds as upon the specific receptoral agent called 'stimulus' or upon the alveolar gases. Indeed stimulus taken without the whole occasion is a misleading abstraction.[10] An informing instance is the 'associational' experiment of Gellhorn and Kraines,[11] wherein the Wundtian method of associative verbal pairs, employing the Kent and Rosanoff tables, was used with variations of oxygen-tension, rate and depth of inspiration, and amount of inspired carbon dioxide.[12]

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[8] A hint of the volume and range of the first half-century of this history may be had from the condensed summaries of problems, methods, and results in E. B. Titchener, *Experimental Psychology, Quantitative,* 2 vols., 1905. The last thirty years of the investigation of the interrelation of "physiologic and psychic processes" have been amply reviewed in recent psychological writings. Such physiological authors (*e.g.,* Sherrington, Barcroft, Bard, Cannon, and Ranson) as interpret their neurological results in mental or other psychological terms are always read by the informed psychologist in the light of this long history.

[9] When we go behind the many researches on anoxia which came out of aviation during and following the World War, we find that Gellhorn's type of study owes more than is commonly credited by physiologists and psychiatrists to the pattern of psychological experimentation set by Emil Kraepelin (1856-1926), who researched for more than forty years upon bodily conditions underlying the conduct of psychological tasks. For an account of Kraepelin's work see *The Problem of Mental Disorder,* 1934, 290-295. All functional neurologists and other physiologists who are ambitious to enter the field of the psychologist would do well to familiarize themselves with the nine meaty volumes of the *Psychologische Arbeiten* (1895-1927) as well as with current psychological methods, problems, and terminologies.

[10] O. F. Weber and M. Bentley, The relation of 'instruction' to the psychosomatic functions, *Psychol. Monog.,* 35, 1926, (no. 163), 1-15.

[11] E. Gellhorn and S. Kraines, *Science,* 83, 1936, 266-267. Cf. Gellhorn, *et al., J. Psychol.,* 3, 1937, 161-168; *Amer. J. Psychiat.,* 93, 1937, 1413-1434, and *Sigma Xi Quart.,* 25, 1937, 156-165.

[12] The Kent-Rosanoff list was adapted from S. Sommer, who supplied 66 of the 100 words. In choosing from this list, Kent and Rosanoff were careful "to avoid such words as are especially liable to call up personal experiences." See G. H. Kent and A. J. Rosanoff, A study of association in insanity, *Amer. J. Insan.,* 67, 1910, 37-96, 317-390 (esp. 37).

Gellhorn and Kraines seemed to find that a certain deficiency of oxygen (7.3%-11% oxygen in the inspired air) brought about "a marked shift in the type of response toward the more individual reactions,"[13]—the 'more individual reactions' being those which occur with relative infrequency in the Kent-Rosanoff lists, which were based upon 1000 'normal' individuals each responding to the self-same 100 stimulus-words. The infrequent words are presumably more directly related than the others to the life, experiences, and person of the particular responder. Gellhorn speaks of them as 'unusual' responses. He divides his results into the following four groups: O-Group, words which appear nowhere in the K-R 1000 responses to a given word (therefore wholly 'individual' words); 1-Group, words which appear there 1-15 times; 2-Group, words which appear 16-100 times; and 3-Group, words which are found in the tables more than 100 times (the most 'usual' words).

Since Gellhorn made the common mistake of assuming that the stimulus-word is the sole 'outside' determining element in the 'normal' associative occasion, he naturally infers that a 'shift toward the unusual' is to be accounted for by the variable tension of oxygen, the only variable in which his interest lies.

In order to demonstrate the presence and efficacy of other factors in the experimental occasion, we have repeated the method of paired associates as used by Gellhorn, only substituting for deficient oxygen some form of instruction—either formal, occasional or self-initiated—, an instruction, however, which may be, and frequently is, included without intent in such experiments. The psychological factor of instruction was designed in our experiments to direct the associative trend toward an 'individual' or biographical response.

In the earlier reports[14] of the Gellhorn experiments, the account is too meager for ready verification. From a somewhat fuller description[15] it appears that the distribution with respect to 'usualness of response' (see curves for control in chart on page 491) gives the ratio for the two halves of the experiment (*i.e.* first and second 50 stimulus-words). These latter form the points of reference from which departure is reckoned under change in oxygen or carbon dioxide. Since no individual measures are set down, it is again impossible to check the calculation. It looks as if two of the lines of the graph were rough measures of reliability and the other two (for O_2-lack and CO_2-excess) were indicators of a shift 'toward the left' in the distribution of responses.

As an assurance against discomfort and apprehension, this last article explains that an additional group of *S*s (20 freshman students in medicine) was first given

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[13] *Science*, 83, 1936, 267.

[14] *Science*, 1936, and *Amer. J. Psychiat.*, 1937 (as cited above).

[15] *Arch. Neurol. & Psychiat.*, 38, 1937, 491-504.

50 words without the Douglas bag and then 50 words with the bag but with the (false) statement that *S* was now subjected to an oxygen-deficit. In spite of the suspicion of a time-error and of the (probable) discovery by *S* that neither inconvenience nor danger awaited him (notwithstanding the experimenter's untruth), the authors are convinced that "these control experiments prove beyond doubt that the shift to the left is the result of a characteristic effect on word associations of the gases inhaled."[16]

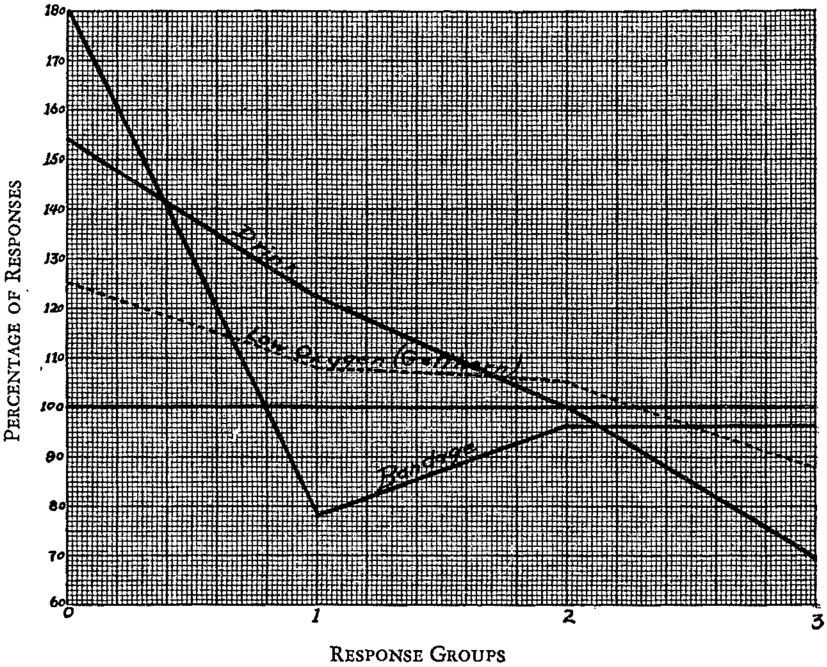

FIG. 1. EFFECT OF OCCASIONAL INSTRUCTION UPON RESPONSES
The percentages are expressed in terms of the Control experiments, the results of which are expressed by the solid line at 100. 0 = individual words, *i.e.* response-words not found in the Kent-Rosanoff tables; 1 = response-words found from 1-15 times; 2 = response-words found from 16-100 times; and 3 = response-words found over 100 times.

Using the same K-R word-list, we substituted, then, for Gellhorn's changes in the respiratory air a modification of the experimental occasion which promised to produce a similar effect upon verbal responses. In our first set of experiments, *O* was supplied with a small beaker containing slightly tinted water.[17] To the instruction to pronounce the first word which followed the list-word, was added the requirement that, at a preliminary 'now,' a sip of the water be drunk. Fifty of the K-R words were orally given, each after a sip by *O*, and the other (equivalent) fifty words without the drink.

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[16] *Ibid.*, 495.

[17] This experiment was carried out in the Psychological Laboratory of Cornell University under the able direction of Dr. Mary S. Ryan.

Our results (Fig. 1 and Table I) are cast in the same form as Gellhorn and Kraines,[18] save that our control results (no drink) are set in Fig. 1 as a base-line of 100% and the drinking results are related to it. This does not significantly change the curve. The four abscissal entries (0, 1, 2, 3,) refer to the groups characterized above.

TABLE I

DISTRIBUTION OF RESPONSE WORDS IN 'DRINK' AND 'CONTROL' SERIES

O	0		1		2		3	
	Drink	Cont.	Drink	Cont.	Drink	Cont.	Drink	Cont.
C	7	11	15	5	12	11	16	23
D	15	4	12	12	14	13	9	21
L	12	7	12	14	13	14	13	15
N	9	6	14	12	9	10	18	22
Totals	43	28	53	43	48	48	56	81

0=Response-words not found in Kent-Rosanoff tables; 1=Response-words found 1–15 times; 2=Response-words found 16–100 times; 3=Response-words found over 100 times.

Exactly the same major difference appears here as in the Gellhorn experiments, i.e., more infrequent words (in the sense of the K-R tables) and fewer words from Groups 2 and 3 (above 15 times in the tables). With us the 'shift toward the left' is somewhat wider than Gellhorn's.

From a scrutiny of our response-words, taken together with the methodical reports of our *O*s, it appears that the preliminary sip of colored water, taken just before the verbal pairing, inclined the organism toward a biographical reference. As a result, many of the response-words openly referred to *O*'s perceptive, memorial, and emotive situations in drinking something which had a medicinal look, while still other words appeared from the reports to be only less obviously connected with the same theme.

Ryan and Werner have explored the conditions under which biographical reference, centering in *somatic objects* and other varieties of *self-object* are created by the organism, as well as the numerous kinds and classes into which these objects fall.[19] Since these objects are autobiographical, it follows that the verbal representatives of them, in the method of paired associates, fall almost exclusively under the K-R designation 'individual responses,' *i.e.* outside the community of reference for such a human group as K and R's 1000 *S*s.

Everyone who has observed the ease with which current topics of interest (war, a flight to the stratosphere, the world's series in baseball, or a catchy tune) litter the avenues of informal conversation will have no surprise for the outcome of the writer's injection of the stimulus-word 'turkey' into a series of K-R words given through several years to some thousands of students in a certain lecture-course. When this word was given just before the great November feast, the most-frequent response-word was 'Thanksgiving,' a term which rarely appeared when no feast was at hand.

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[18] *Science, op. cit.,* 266.

[19] H. C. Werner, A descriptive study of somatic objects, this JOURNAL, 51, 1938, 225-234; M. S. Ryan, An experimental approach to the self-object, *ibid.,* 235-252.

In a second set of experiments the colored drink was replaced by a bandage so closely bound about *O*'s upper arm that circulation was slightly impeded in arm and hand. The general procedure was the same as before, save for necessary modifications in the formal and occasional instructions. The response-words were distributed as follows with respect to their 'unusualness' in the sense of the K-R tables.

TABLE II

Distribution of Response-Words in 'Bandage' and 'Control' Series

O	0		1		2		3	
	Band.	Cont.	Band.	Cont.	Band.	Cont.	Band.	Cont.
J	2	4	7	9	11	14	30	23
G	6	2	7	12	16	10	21	26
M	13	5	10	15	12	13	15	17
W	15	10	12	10	13	17	10	13
Totals	36	21	36	46	52	54	76	79

A significant difference among our *O*s came out in the reports. *J* reported that the bandage had neither disturbed nor distracted him, while *G*, *M*, and *W* remarked that the constriction frequently brought the turgid arm and hand into the field of perceived objects and events. It concordantly appears from the figures in the 'bandaged' columns that the last three *O*s together returned twice as many O-group or 'individual' responses under a noticeable constriction as they returned in the corresponding control series (34 and 17), whereas *J* shows no consistent differences in distribution throughout the set. It is also worth noting that for *G*, *M*, and *W* a high percentage (nearly one-half of the excess of 17) was drawn from the adjacent 1-group, suggesting (albeit more results are called for) that it is especially the verbal occasions for the *relatively* rare response-words (to be found 1-15 times in the standard tables) which are here susceptible to the experimental occasion, the occasion which tends to weight the associative trends in the direction of the setting and of the bodily state and condition of the observing organism. Anyone who has babbled of the pleasures of the table when hungry or who has inclined toward pessimistic comment when tired need not be told that the injection of a disturbed bodily self into the perceptive, memorial, or emotive field is as likely a determining condition of verbal formations as is a hypothetical change in excitability of the cerebral cortex by way of a diffused chemical agent. The relevance of our alleged factor is further supported by the frequency with which *O*'s expansion of his verbal reports revealed a reference to the bodily self, its state, and its setting, at the moment when the stimulus-word was presented.[20] At all events, it is plain that our control of the experimental situation and of the temper and tuning of *O* has brought about a further deflection of responses toward the unique and individual than occurred under the reduction of oxygen-tension in the researches referred to.

It appears likely that the Gellhorn experiments would profit by further checks and controls, especially since nothing short of a surgical or legal threat to bodily or social integrity is better designed to evoke an inquisitive, rebellious, or apprehensive reference to the various forms of the empirical 'self' than an interference with breathing. One has only to recall how the best and most courageous aces of the air trembled, reddened, and agonized before a simple test upon the rebreathing apparatus in the Medical Research Laboratory or how one was oneself disturbed, uncomfortable, and decidedly self-referring when O_2-tension dropped in the army-experiments to realize how ambiguous is the relation between respiration and 'the higher mental processes.'

The tendency toward the creation of somatic and other self-referring objects under low oxygen is abundantly exemplified also in the experimental field in which Gellhorn's own studies lie. Here are instances.

Wespi, who used the questionary, word series, verbal memories, perceived pictures, reaction-times, cancellation, paired associates, addition, and writing from dictation, found under low oxygen an excessive number of self-referring (*egozentrische*) and situational (*situationskonstellierte*) response-words, as well as 'senseless' syllables (rhyme, alliteration, repetition).[21] He found, as others had, great numbers of external (*äussere*) associations, indicating that the experimental occasion—the perceptive-memorial-emotive 'present' of the observer—was here determinative, rather than those *innere* conditions which dispose the organism toward the 'usual' non-biographical responses of the K-R tables. Moreover, Wespi's subjects reported notable change of mood and emotive tuning (gaiety, intoxication, apathy) from which this experimenter inferred (as Gellhorn also has) an immediate derivation from low oxygen. The more careful students of organic effects of alcohol and drugs have taught us temperance in that direction!

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[20] The popularity of the method of verbal associates derives, without a doubt, from its apparent simplicity and from the ease with which results are obtained, without special training of experimenter and subject—results which appear on their face to be of vast importance in psychiatrical, psychoanalytical, and criminological diagnosis. Like all other experiments involving verbal formulation, these really demand great skill in operation and high reserve from the interpreter. For cautions against the inexpert use and the illicit interpretation of the associational methods, see M. Bentley, *The Problem of Mental Disorder*, 1934, 298-300. The assumption that associational experiments with words evoke 'the higher mental processes' requires examination. As a matter of fact, the psychological facts commonly involved are of an extremely simple, automatized sort. They are much like the trigger responses of the finger on the release key in highly practiced simple reactions to a repeated sound or light. The unwarranted assumption often tempts the neurologist to bizarre theories of the cerebral and psychical processes involved.

[21] H. Wespi, Ueber psychische Insuffizienzerscheinungen bei vermindertem Luftdruck, *Arbeitsphysiol.*, 7, 1934, 484-516 (esp. 502-505).

Johnson and Paschal likewise speak of a "noticeable disturbance" of the subject, which declined as the experiments progressed.[22] These experimenters, who were alive to possible complications from the whole occasion in which the subject was imbedded, well knew, however, that the final emotive adjustment which the subject sometimes made did not eliminate the effective factors, leaving a fixed and static organism, on the one hand, and a simple change in cerebral excitability, on the other. The increase in functional output, for example, often observed with high oxygen-deficit in the rebreather, they laid to a self-pacing effort to overcome a handicap from the depleted air.[23]

Lowson[24] too found many body-objects and body-states incidental to exhilaration, irritable mood, lack of composure, fear, headache, muddlement, etc., under anoxia, all moments woven into the experimental occasion and all designed to affect both the ability of *S* to report and the character of the report itself. It may well be observed here that Gellhorn himself notes, under "somatic symptoms" (sic), such body-states and body-objects as were reported 'dizziness,' 'feeling of warmth,' 'decrease in hearing and seeing,' 'fulness in the head,' 'sleepiness,' and 'ringing in the ears.'

The same sort of issue of experimentation appears under the heading of 'complaints made by the subject' in a recent study of anoxia by McFarland.[25] Frequent complaints freely voiced by *S* include headache, respiratory changes or difficulties, excessive sleepiness, dizziness, difficulty in concentrating, sensory impairment, lassitude, indifference and fatigue. A list of 30 frequent complaints "observed in previous experiments" was also checked by each *S*.[26]

The main observation pertinent to all these reportable matters incident to experiments upon deficiency of oxygen, excess of carbon dioxide, compensation of anoxia by carbon dioxide, deep breathing, and tasks done with incentives to autobiographical reference, is that the experimental situation includes, for the performing and accomplishing *S*, a large group of psychological factors which notably affect performance and output, although they may be only incidentally connected with the processes and sequels of respiration.[27] This observation does not tend to deny that there are immediate and direct effects upon bodily mechanisms which follow quantitative changes in respiratory air. It does go to show, however, that no changes in organic status or in functional output (whether or not the "so-called psyche" is invoked) can be adequately or safely described or explained unless these fundamental conditions and factors are competently taken into account.

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[22] H. M. Johnson and F. C. Paschal, Psychological effects of deprivation of oxygen: Deterioration of performance as indicated by a new substitution-test, *J. Psychobiol.*, 2, 1920, 193-236.

[23] Gellhorn may have overlooked this common increase in effort and intent when he announced a "supernormal phase" of accomplishment at the end of the anoxic period. See his accounts in *Amer. J. Physiol.*, 112, 1935, 522; 115, 1936, 682.

[24] J. P. Lowson, The effects of deprivation of oxygen upon mental processes, *Brit. J. Psychol.*, 13, 1923, 417-434.

[25] R. A. McFarland, *The Effects of Oxygen Deprivation (High Altitude) on the Human Organism*, Department of Commerce, Bur. of Air Commerce, Safety and Planning Div., Washington, Report no. 13, 1938.

[26] *Ibid.*, 44-45.

A large segment of physiological research seeks to correlate environmental objects, processes, and agents with consequent modifications within the organism. Thus is the energy-change at the receptor related to receptoral and neural processes which are released by stimulus; and thus are the atmospheric gases related to subsequent respiratory, circulatory, neural and other processes within the body. In both these types of investigation, progress is limited by our exceedingly inadequate knowledge of the functions of the central nervous system, especially of the cerebrum. Neural physiologists are wont to supplement this scanty knowledge of cerebral functions (1) by invoking speculative doctrines of local and integrated mechanisms and operations of the brain and (2) by interpolating psychological 'phenomena' into their bodily accounts, the interpolation most commonly guided by a dualistic notion of 'mind' as running parallel to brain-process.

Both devices are dangerous: current theories of cerebral functioning are amply sustained by tradition but badly supported by fact, while psychological interpolation is hopeless when couched in the gross terms of elementary sensation, higher mental process, and other 'psychic phenomena.'

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[27] In other experiments (addition of numbers, memory, handwriting, cancellation, descrimination, etc.) Gellhorn found similar 'psychic' effects of anoxia, which he believed to be due to a change in cerebral excitability, *i.e.* a primary result of alveolar deprivation. Had he not been so certain of his neural theory and so impressed by his alleged exposition of a complete "psycho-physiological" parallelism, he might have been more observant of the factors which we have noted. Something of this sort was strongly hinted at by his critics (Olkon, Neymann, Solomon, and Pollock) after the reading of one of the associational papers (Gellhorn and Kraines, *Arch. Neurol. & Psychiat.*, 38, 1937, 502-504). The fact that the same kind of 'psychic' damage was wrought by an excess of CO_2 and under hyperpnea also increases the difficulties which any simple 'excitability' theory encounters. Again, relief upon the addition of 3% CO_2 in the anoxic condition requires further examination of the situational factors which we have demonstrated. The "subjective symptoms" of headache, dizziness, fullness of the head, etc., which Gellhorn found in anoxia, but relieved on the addition of CO_2 (*Amer. J. Psychiat., loc. cit.*, 1429), should be regarded as something more than 'symptoms' and 'complaints.' They probably played an important part in the entire experimental setting and in the accomplishment of the subject.

In the text above, the phrase "psychological factors which notably affect performance and output" may disturb the neurologist, who is, as a rule, organicist, parallelist, interactionist, epiphenomenist, or believer in a dynamic psyche, as his mixed traditions and the immediate occasion may together suggest. 'Psychological' is not to say 'mental' in any existential sense, since the body is to be regarded as the only energetic agent and effective substrate to performance, whether performance is referred for description to physiologist or to psychologist.

In looking for the main cerebral and psychological consequents of deficient alveolar oxygen, no one could object to Gellhorn's desire "to tear down the borders" between physiology and psychology *provided* no relevant facts and factors are ignored and *provided* methods competent to a full description are employed.[28] It is deficiency in both of these provisions, however, that has been dwelt upon in the present discussion. Unless and until competence on both sides is united in the same investigator, it would seem to be expedient for the neurologist to restrict himself to a descriptive account of the relevant bodily processes treated in a physiological way, and for the psychologist to proceed in a psychological manner, drawing from general physiology and from functional neurology only the best authenticated facts and the soundest principles of bodily mechanism and operation.[29]

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[28] "To tear down the borders existing between psychology and physiology and to show by quantitative measurements that alterations in any physiologic conditions leading (etc.) . . . alter 'psychic' phenomena in a similar fashion," *Sigma Xi Quart., loc. cit.,* 157. Cf. *Amer. J. Psychiat.,* 93, 1937, 1433..

[29] M. Bentley, The psychologist's uses of neurology, this JOURNAL, 49, 1937, 233-264 (esp. 261).

AGE AND THE EFFECTS OF ROTATION

By SALVATORE RUSSO, Auburn Collegiate Center, and KARL M. DALLENBACH, Cornell University

It is commonly believed that children enjoy activities involving bodily rotation (riding on the merry-go-round, whirling on the piano stool, or spinning on the heels) and that adults dislike and avoid such activities. Holsopple, writing upon this point, states that "children whirl themselves violently about, bringing nearly all their canals into a position susceptible for stimulation, and are merely amused by the effects which would not be tolerable to an adult."[1] Apparently what is pleasurable for children becomes unpleasant for adults. At some time, therefore, during the transition between childhood and adulthood the affective character changes. When specifically does the affective change take place? Does it appear gradually or suddenly? Is there a period or age of indifference? Our purpose in undertaking the present study was to obtain answers to those questions. At the same time we also took the opportunity to note the uniformity at different age-levels of two phenomena incidental to rotation, past-pointing and the falling-reaction.

METHOD AND PROCEDURE

The *Ss*, 213 in number and ranging in age between 5 and 21 yr., were drawn at random from the public schools and the Collegiate Center of Auburn, N.Y.[2] Participation was voluntary, a free response to a request for subjects in a psychological experiment. The character of the experiments was not divulged until the *Ss* reported at the laboratory. This precaution was taken to insure an unbiased group, since persons having a dislike for rotation might, with prior knowledge, have been disinclined to volunteer. For the same reason we asked the *Ss* not to discuss the experiments with other pupils who had not as yet served. That we received their coöperation in this matter is shown by the fact that none of the *Ss* knew what to expect when he came to us.

The *Ss* reported at the laboratory between 3–4 P.M. on school days in groups of 3 or 4. Their height and weight were measured so as to afford an index of physiological age. Then they were taken one at a time to the experimental room, which was equipped with a Cornell rotation table.[3] To allay uneasiness, the experiment was first explained and then demonstrated by being performed upon the experimenter.[4] *S*,

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[1] J. Q. Holsopple, Space and the non-auditory labyrinth, in *The Foundations of Experimental Psychology*, 1929, 427.

[2] Our thanks are due to Dr. C. J. Hetherington, Superintendent of the Auburn schools, for his coöperation in affording us access to the *Ss*, and to Mr. Joseph Corcoran, who served as an assistant in this study.

[3] K. M. Dallenbach, An inexpensive rotation table, this JOURNAL, 42, 1930, 637.

[4] S. Russo, the junior author. Within a short time he became habituated to rotation and it had no effect upon him.

therefore, saw for himself that nothing harmful would happen to him, and he also knew by instruction and example what the procedure would be.

After receiving these instructions, *S* was blindfolded and placed in an upright position in the chair upon the rotation table, and the table was rotated. When the rotation was brought to a sudden stop, his behavior was noted and his report taken. If he did not offer a report upon affectivity, he was questioned specifically regarding it. After a few minutes' rest, the procedure was repeated with rotation in reversed direction. The first direction of rotation was alternated with every other *S*. Neither direction, consequently, had a positional advantage in the group of *Ss* as a whole.

This experiment was followed by two tests of past-pointing and two tests of the falling-reaction. In the tests of past-pointing, *S*, in accordance with instructions, opened his eyes when rotation was interrupted, struck at a target held at arm's length directly in front of him, and then held his arm loosely allowing it to 'drift.' The error in pointing and the drift were measured and recorded. Head movements were also observed and recorded.

In the tests of the falling-reaction, rotation was sagittal (head on shoulder or on knees facing sideways) and lateral (chin on chest with face down or head back with face up). When the rotation ceased, *S* brought his head and body to the normal posture. His movements were recorded together with his report.

Rotation was constant in duration and speed: 10 turns at the rate of 1 turn in 2 sec.[5] While executed manually, practice and the aid of an assistant who checked the turns with a stopwatch gave it a high degree of uniformity.

We first sought, with 49 *Ss*, to discover the critical age-level by bracketing. We started at ages 9 and 16 yr. and then gradually widened the bracket to 5 and 21 yr. When it became evident that there was no age-level within this range at which all the *Ss* agreed upon affectivity, we abandoned that objective and turned to the investigation of the adolescent years—ages 10–16 yr. inclusive. Within those age-limits we experimented upon 164 *Ss*.

RESULTS

(1) Affectivity. The affective reports are classified in Table I, which shows—for sex, age-level, and direction of rotation—the number of *Ss* reporting pleasant (*P*), indifferent (*I*), or unpleasant (*U*).

(a) Age-difference. The most important results revealed in Table I are: (i) rotation is rarely reported as indifferent—in only 8% of the experiments; and (ii) rotation is not uniformly affective at any age-level. Both of these results confirm our initial experiment. The belief that all children enjoy rotation, and that all adults dislike it, is not borne out in this study. It is a false generalization perhaps based on the fact that more of the younger than the older children enjoy the experience: among our *Ss* 52% reported *P* at the 10-yr. level and only 25% at the 16-yr. level.

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[5] The speed at which R. Bárány states he obtained the best results (Weitere Untersuchungen über den vom Vestibularapparat des Ohres reflektorisch ausgelösten rhythmischen Nystagmus und seine Begleiterscheinungen, *Monat. f. Ohrenhk.*, 41, 1908, 497.

The table shows furthermore that pleasantness tends to decrease with age and indifference to increase, while unpleasantness, after an initial sudden increase, tends to remain stationary. There is a strong indication that the affectivity of rotation shifts at the 11-yr. age-level. At the 10-yr. level, reports of pleasantness and unpleasantness practically balance each other. After that age, *P* predominates over *U* in a ratio of about 2 to 1. The only exception occurs early at the 12-yr. level, where the ratio is decreased to about 5-4 (51-39).[6]

TABLE I

AFFECTIVE REPORTS DISTRIBUTED ACCORDING TO CHRONOLOGICAL AGE

Age (in yr.)	Rotation	Boys			Girls			Total			Percentage		
		P	*I*	*U*	*P*	*I*	*U*	*P*	*I*	*U*	*P*	*I*	*U*
10	Clockwise	7	0	3	5	0	7	12	0	10	55	0	45
	Counter	5	0	5	6	0	6	11	0	11	50	0	50
	Total	12	0	8	11	0	13	23	0	21	52	0	48
11	Clockwise	4	1	7	4	0	5	8	1	12	38	5	57
	Counter	2	0	10	4	0	5	6	0	15	29	0	71
	Total	6	1	17	8	0	10	14	1	27	34	2	64
12	Clockwise	8	1	12	6	3	5	14	4	17	40	11	49
	Counter	8	1	12	5	2	7	13	3	19	37	9	54
	Total	16	2	24	11	5	12	27	7	36	39	10	51
13	Clockwise	4	2	6	2	1	6	6	3	12	29	14	57
	Counter	3	2	7	1	1	7	4	3	14	19	14	67
	Total	7	4	13	3	2	13	10	6	26	24	14	62
14	Clockwise	7	0	12	4	0	7	11	0	19	37	0	63
	Counter	7	0	12	1	3	7	8	3	19	27	10	63
	Total	14	0	24	5	3	14	19	3	38	32	5	63
15	Clockwise	3	1	8	4	0	9	7	1	17	28	4	68
	Counter	4	2	6	4	1	8	8	3	14	32	12	56
	Total	7	3	14	8	1	17	15	4	31	30	8	62
16	Clockwise	0	2	3	3	1	1	3	3	4	30	30	40
	Counter	0	1	4	2	1	2	2	2	6	20	20	60
	Total	0	3	7	5	2	3	5	5	10	25	25	50
Grand total	Clockwise	33	7	51	28	5	40	61	12	91	37	7	56
	Counter	29	6	56	23	8	42	52	14	98	32	8	60
	Total	62	13	107	51	13	82	113	26	189	34	8	58
%	Clockwise	36	8	56	38	7	55	37	7	56			
	Counter	32	7	61	32	10	58	32	8	60			
	Total	34	7	58	35	9	56	34	8	58			

(b) Maturational difference. All individuals, it is well known, do not reach the same degree of physical development at the same age. Some mature early, others late. It is possible, therefore, that the affective shift is

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[6] We have no knowledge of when or how the change occurs in the development of a given individual. That is another problem which well deserves investigation. A group of young children, selected because they like rotation, should be tested at intervals over a course of several years; and the affective shifts, if and as they occur, should be correlated with physiological as well as chronological age. We cannot safely infer from our group-results to the individual.

more closely correlated with physiological than with chronological age. To test that possibility we correlated the affective reports with height and weight—the only indices of physiological age available to us. These correlations, computed separately for each sex and for the group as a whole, were however little better than chance. Except for the extremes in weight, the affective reports are practically uniform in the different quartiles of height and weight (see Table II). Since the extremes in weight correlate with chronological age (the light *Ss* being younger and the heavy, older) the affective differences in these two weight-groups are merely chronological differences over again. Hence we feel justified in concluding that neither physiological nor chronological age alone accounts for the affective change.

TABLE II

AFFECTIVE REPORTS DISTRIBUTED ACCORDING TO THE SS' HEIGHT AND WEIGHT

Report	Height (quartiles)				Weight (quartiles)			
	1st	2nd	3rd	4th	1st	2nd	3rd	4th
P	41%	33%	28%	36%	40%	35%	40%	22%
I	6%	10%	9%	7%	5%	6%	7%	13%
U	53%	57%	63%	57%	55%	59%	53%	65%

Its cause must be sought among the adventitious factors in individual development.

(c) Sex differences. The differences between the sexes are insignificant. The affective reports of the boys and girls are, as Table I reveals, very similar at every age-level, and the totals show practically identical percentages of *P, I,* and *U* reports. Of the 91 boys, 34% reported *P;* 7% *I,* and 58% *U;* of the 73 girls, 35% reported *P,* 9% *I,* and 56% *U.* Whatever it is that causes the affective shift, it must be something to which the two sexes are equally susceptible and sensitive.

(d) Differences in direction of rotation. The differences in affectivity between clockwise and counter-clockwise rotation were found (Table I) to be slight. Clockwise rotation was, however, reported as *P* more frequently and as *U* less frequently than counter-clockwise rotation. The differences, though small, are consistent for *P* and *U* in both sexes. Handedness, though not recorded, can in no way be responsible for these results, since the differences between the two directions of rotation are not of the order of the handedness-differences in the general population. We have no explanation of the rotational differences. Their significance for the entire group is slight: the *P* difference of +9 and the *U* difference of —7 being only 82 and 77 times chance respectively.[7] It may well be, therefore, that

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[7] The two arrays treated by the method of χ^2 yields a P of 0.33. (Cf. R. A. Fisher, *Statistical Methods for Research Workers,* 1928, 82 ff.)

despite the consistency in the distribution of the results among the two sexes we are dealing here with chance.

(2) Corollary experiments. (a) Past-pointing. All of our 213 *S*s participated in the post-rotational pointing-experiments and they all, it should be remembered, served without prior knowledge. Of these 183 (86%) yielded the usual results; they past-pointed in the direction of rotation. Of the 30 (14%) not conforming to this rule, 20 cross-pointed (*i.e.* pointed in the direction opposite to that of the rotation) when rotation was clockwise, 12 cross-pointed when it was counter-clockwise, and 2 cross-pointed after rotation in both directions.[8]

(b) Arm-drift. The direction of the drift of *S*'s arm after pointing subsequently to the stroke at the target agreed in every case with the direction of the error in pointing. If *S*. past-pointed, he past-drifted; if he cross-pointed, he cross-drifted. The two types of post-rotational arm movement (pointing and drifting) are apparently both conditioned by the same or similar determinants.

(c) Head-movements. Compensatory head-movements, frequently observed in animals,[9] were noted in 68 (32%) of our *S*s, who turned their heads in the direction opposite to that of the rotation. Of the remainder, 85 (40%) did not move their heads at all, and 38 (28%) moved it in the direction of rotation.

(d) Falling-reaction. Only 124 of our *S*s participated in the falling-reaction experiments. Of these, 107 (87%) gave the usual results; a perception of falling in a direction negative to that of rotation, and a compensatory movement in the positive direction.[10] The remaining *S*s (17 in number, or 13%) gave discrepant results:[11] either the *perception* of falling was lacking, vague, uncertain, or in an unusual direction; or the *compensatory movement* was lacking, faulty, or in an unusual direction. For 5 of these 17 instances, abnormal conditions in the ears were definitely responsible.[12] The others are probably to be explained in the same way, but

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[8] Hastings obtained similar aberrant results. "Out of 700 applicants," he states, "47 cross-pointed with one or both arms on turning (Reactions of the normal labyrinth, *J. Otol., Rhinol., and Laryngol.*, 27, 1918, 486).

[9] B. C. Gruenberg (Compensatory motions and the semi-circular canals, *J. Exper. Zool.*, 4, 1907, 447-467) reports that the frog turns its head away from the direction of rotation, and S. S. Maxwell (*Labyrinth and Equilibrium*, 1923, 16) states that the frog and pigeon turn their head in the direction opposite to that of rotation.

[10] I. H. Jones, *Equilibrium and Vertigo*, 1918, 8.

[11] Hastings obtained similar results. He writes that "the falling reaction was found to be abnormal 11 to 25 times on turning right, and 10 in 254 times on turning left" (*op. cit.*, 486).

[12] To suppurating ears that had to be lanced, to mastoid operations, and, in one case, to infantile paralysis with spinal curvature and asymmetrical development of the right side.

no confirmatory evidence is at hand since the *S*s in question did not report any auditory anomalies and an otologist was not available for aural examination.

SUMMARY

The results of this examination of 213 *S*s from 5 to 21 yr. of age to determine the effects of rotation may be summarized as follows.

(1) Rotation is rarely indifferent and it is not uniformly affective at any age-level.

(2) A greater proportion of young than of old children like the experience.

(3) The transition from liking to disliking in the rising age-groups is marked at the 11-yr. age-level.

(4) The affective shift is not correlated with maturational changes as measured by height and weight, for neither of these indices correlates with the shift.

(5) The causes of the affective shift must be sought among the adventitious factors in development.

(6) The sex-differences are insignificant. The cause of the affective shift, consequently, must be something to which the two sexes are equally susceptible and sensitive.

(7) Differences in affectivity between clockwise and counter-clockwise rotation are slight and, though consistent, are probably due to chance.

(8) The large majority (86%) of the *S*s past-point. A small number give anomalous results. Mostly they cross-point when rotation is in one direction and past-point when it is in the other. Only 2 *S*s (1%) cross-point consistently after rotation in both directions.

(9) The arm, held loosely after pointing, drifts in the direction of the error in pointing.

(10) Compensatory head movements during rotation were made by only about a third (32%) of our *S*s. The largest proportion (40%) did not move their heads and 28% moved it in the direction of rotation.

(11) The normal falling-reaction was reported by a large majority (87%) of our *S*s. Some of the irregularities reported, and probably all, are due to anomalous conditions of the *S*s' ears.

A PRELIMINARY STUDY OF THE RÔLE OF POSTURAL ADJUSTMENT IN CONDITIONING

By G. L. FREEMAN, Northwestern University

In an earlier discussion of the rôle of postural adjustments in the government of overt reaction,[1] the writer suggested that tonic response segments may come to anticipate or 'set' the associated phasic response in the process of repeated stimulation and 'conditioning.' This paper presents a preliminary attempt at experimental check of the hypothesis. Done on only one animal, the study is reported in the hope that laboratories primarily interested in the conditioning technique will be encouraged to continue what seems to be a promising line of attack.

Procedure. The experiments took the form of recording neuromuscular activity in the four limbs of a dog during various conditioned-unconditioned stimulus-combinations. The same unconditioned stimulus (shock) was used in all cases, but was applied to different paws accordingly as the conditioned stimulus was varied. Conditioned stimuli were four tones ranging from 100 to 1000 ~ and hereafter referred to as CS_1, CS_2, CS_3, and CS_4. Combinations of tone (3 sec.) and shock (0.2 sec.) were as follows:

CS_1-UCS, always applied to right rear paw;
CS_2-UCS, applied to right or left rear paw in a random order;
CS_3-UCS, applied to right or left rear paw or right front paw in random order;
CS_4-UCS, applied to right or left rear or front paw in random order.

By design the conditioned stimuli differed in the specificity of the response which they anticipated and our problem was to find out if the postural antecedents of the unconditioned limb movements would reflect the experimental variations. The animal was suspended in the conditioning harness and small galvanized iron shocking plates were attached to each paw. These plates also formed part of a delicate lever arrangement which marked on smoked paper the flexion-extension movements of each limb. In a few instances, 6 mm. gold-plated electrodes were cemented to the flexor muscles and connected through a suitable amplifier to a cathode ray oscillograph. A commutating switch permitted viewing of action potential phenomena from any one of the four limbs, but simultaneous recording was not possible. In this work, as in administering the electric shock to the animal, the 'indifferent electrode' was attached to the base of the tail.

In training, the individual trials were spaced about 30 sec. apart and at first the shock was invariably applied to the right rear paw (the CS_1-UCS combination). When conditioned reactions (tentative movements in the right rear limb) appeared, this procedure was interspersed with shocks to one or the other of the rear paws (the CS_2-UCS combination) and after differentiation in postural preparation appeared upon presentation of the appropriate conditioned stimulus, the training was further complicated by the addition of the CS_4-UCS combination in which a given

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[1] G. L. Freeman, The problem of set, this JOURNAL, 52, 1939, 16-30.

tone was indiscriminately followed by shock to any one of the four paws. Finally another tone (CS_3) was also introduced with shock to one of three paws. In all, the experiments were of over a month's duration, with the new discrimination problems added at the rate of about one per week.

Results. Since records are incomplete for the total training period, only approximate values can be given for the relative ease of 'conditioning' to our four experimental variables. Decreased anticipatory postural activity in *all* limbs except the one which received the shock appeared within the first 20 presentations of the CS_1-UCS combination. To induce postural differentiation between the specific CS_1 stimulus and the CS_2 stimulus which signalized shock to one of the rear paws took over a week of intensive training; further differentiation, as between the CS_1 or CS_2 stimulus and the CS_4 stimulus (shock to any one of the four paws) took even longer. The final addition of CS_3 (shock to one of three paws) never exhibited a specifically differentiated postural adjustment and the animal tended to respond to the tone either by tentative movements of the rear limbs (as to CS_2) or more generally, by movements of all limbs (as to CS_4). To a certain extent, the difficulty experienced with the CS_3 conditioned stimulus was probably a function of its placement in the training series; that is, had it been introduced following CS_1 the animal might readily have shown differentiation between preparation for shock to one of three paws and the other conditions.

Action potential records indicated some slight differentiation in response to CS_4 and CS_3 stimuli, but both here and elsewhere, the fact that the records were obtained from one muscle group at a time does not give them much weight. About all that can be said of this part of the experiment is in the suggestions which it carries for further work. Differing degrees of electrical activity in the four muscle groups occurred under the conditions studied, and the indication is that if these would provide a very sensitive index of antecedent postural adjustments if recorded simultaneously and with suitable controls. This would require either a set of four matched amplifiers or a system permitting a more rapid commutation of the signals through the final output stage than was used in this experiment.

The patterns of postural adjustment which accompany the presentation of different conditioned stimuli are most readily studied in connection with the kymograph records of limb movements, a few of which are shown in Fig. 1. Record A was taken on the animal during the third presentation of CS_1-UCS combination and indicates that as yet the animal has not come to respond to the tone as the signal for shock. Instead the activity in all limbs is continued prior to, during, and after the combination is applied. Especially important, however, as a forerunner of a conditioned postural reaction to the same stimulus is the heightened activity in the limb (right rear) which received the shock. This prolonging of the unconditioned reaction was characteristic in all situations, especially in the early stages of training.[2]

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[2] Q. F. Curtis (Experimental neurosis in the pig, *Psychol. Bull.*, 1937, 34, 723) has found, however, that the unconditioned shock stimulus is followed by a period of comparative relaxation in the reacting member before anticipatory behavior to the next shock begins. It may be that our results are a function of the more complicated situation in which the animal was placed. We did observe, toward the end of training in the CS_1-UCS combination, a shift in the pattern of post-stimulation activity: that is, instead of a gradual reduction to the time of presenting the next CS-UCS combinations, the UCS seemed to serve as a trigger to discharge a mounting tension which began to build up again in anticipation of the next stimulus.

Record B shows a conditioned postural reaction to stimulus S_1 taken during the second week of the tests. It should be noted that the animal is now resting fairly quietly in the harness and that the conditioned stimulus S_1 is the signal for localized anticipatory tension in the part which is to receive the shock. The heightened flexor activity of the right front limb which follows the unconditioned response of the right rear limb is perhaps a homolateral irradiation phenomenon. Occasional omission of the shock stimulus was found to have little effect on the fundamental character of this postural adjustment; that is, the anticipatory contraction of the right rear limb was followed at the appropriate time by more vigorous withdrawal movements.

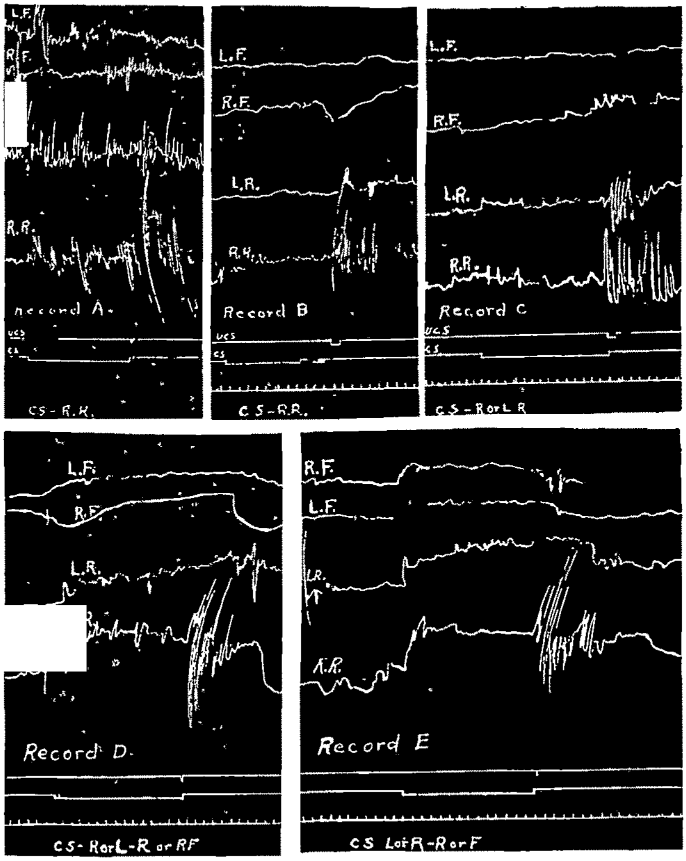

FIG. 1. PATTERNS OF POSTURAL ADJUSTMENT WHICH ACCOMPANY THE PRESENTATION OF DIFFERENT CONDITIONED STIMULI

Record C, taken after considerable training with the combination of CS_2 and shock to one of the rear limbs, shows greatest anticipatory activity in the hind quarters and indicates that the more non-specific the conditioned stimulus, the more diffuse is the postural preparation. Also of considerable interest is the appearance of contralateral reactions as well as homolateral spread following administration of the shock stimulus.

Record D was taken near the close of the experiment and is the response of the animal to the CS_3 stimulus with shock delivered to one of three places. It will be seen that there is little differentiation between the anticipatory reaction of the left front limb, which definitely will not receive a shock, and the other limbs which may. Instead, the pattern of postural adjustment closely resembles that made to CS_4 (see next record).

Record E was taken the same day as Record D. The conditions differed only in that the signal (CS_4) for shock to one of four possible limbs was given. In response to it the animal shows anticipatory movements in all limbs with marked contractions in the one which actually receives the shock.

It should be mentioned that we have selected for comparative purposes only records in which the unconditioned stimulus was delivered to the right rear paw. Similar records could be exhibited in which the spread of anticipatory postural adjustments varied with the conditioned stimulus utilized and where the unconditioned shock was delivered to some other limb. During the final week of training the various stimulus combinations were alternated in fairly close succession, with the result that the animal became confused, lost postural differentiation and tended to respond in a generalized fashion to all tones. In fact, kicking became so violent in one instance as to completely wreck the lever arrangements. Since we were interested primarily in the demonstration of postural differentiation, we have left the study of its confusion under conditions demanding rapid intraorganic readjustments for detailed analysis at some future time.

Discussion. The existence of different postural antecedents in our several conditioning experiments suggests a 'set' explanation in the selection of the overt reactions which were made.[3] When the dog is first placed in the harness there is aroused an attitude of apprehension which expresses itself in terms of a generalized postural adjustment (see Record A in Fig. 1). The dog does not 'know' from what quarter to expect punishment or reward and is consequently prepared for a wide range of possibilities. This general postural adjustment is active when the combination of tone (CS_1) and shock (UCS) is first applied and continues in heightened degree in the bodily part which actually receives the shock (see Record A). Consequently when the conditioned stimulus is next applied it meets a deferential postural adjustment with major neuromuscular activity centered in the limb which received the unconditioned shock stimulus. Repetition of the stimulus combination results in the 'capture' of the postural set by the conditioned stimulus; that is, the tone becomes independently capable of arousing the postural adjustment as when given in a series with other stimuli which are not a signal for the application of the unconditioned stimulus to this particular limb (see Record B). This postural set is a conditioned response, preparing the organism for the unconditioned withdrawal reaction to shock. It differs from the unconditioned response in degree, and probably in character as well,[4] and it may upon temporary removal of the unconditioned stimulus produce an appropriately timed conditioned withdrawal reaction.[5]

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[3] For a summary of literature pertaining to the rôle of preparatory sets and tension in conditioning experiments see O. H. Mowrer, Preparatory set (expectancy): A determinent in motivation and learning, *Psychol. Rev.*, 45, 1938, 62-91.

The experiments indicate that not all conditioned postural adjustments are equally limiting in the unconditioned responses which they prepare for and anticipate. Thus, when a conditioned stimulus (CS_2) is given which signals the forthcoming application of shock to one of the two rear limbs (instead of the specific CS_1 signal) the animal becomes 'set' with heightened tensions in those parts which may receive the shock (see Record C). Likewise, there is an attempt at differential postural adjustment when a conditioned stimulus (CS_3) signals a shock to one of three possible loci (see Record D). An even more generalized set anticipates a fourfold possibility (see Record E) and it differs from the generalized apprehensive posture only in its occurrence in response to a specific conditioned tonal stimulus (CS_4).

In our theoretical analysis of conditioning in relation to set phenomena, we hypothecated that the conditioned postural adjustment carried a different order of response component than did the overt withdrawal reaction. For this we argued as follows:

"The stimulus configuration (s_1 tone, S_1 shock) evokes a response pattern (r_1R_1) which has both tonic and phasic elements. In the resulting complex it is reasonable to assume that tonic and phasic segments may be captured by different parts of the stimulus configuration. The phasic element (R_1) will be associated with the most dominant part of the stimulus configuration (S_1) and its character will be determined by 'innate' connections. This is the unconditioned response of Pavlovian terminology. . . . There is a tendency for the postural adjustment of apprehension (r_1) to be associated with the remaining tone stimulus (s_1). But this s_1r_1 connection does not remain unmodified by the S_1R_1 connection, for under repetitive stimulation the posture of generalized apprehension is early changed into specialized adjustment in one part of the body. The pattern of anticipatory tension is now localized in the leg to be stimulated and the dog turns his head in that direction. This qualitative and quantitative shift in r_1 may be accomplished without special reference to s_1. What probably happens is that the tonic response element r_1 persists longer in that part of the body making the phasic reaction R_1, and that this differential posture carries over to the next application of the stimulus combination where its connection with the conditioned stimulus s_1 continues. . . . Since the tonic factor outlasts the phasic factor, it will at first be the only reaction component connected with the stimulus complex when the unconditioned stimulus S_1 is withdrawn. But since r_1 has been built up by training to be anticipatory of R_1 it may now activate this phasic response through proprioceptive channels. In this scheme the first effect of the tonic response r_1 is to lower the threshold for the phasic response R_1 *via* proprioceptive afferents to the motoneurone field of R_1. Later, if r_1 is sufficiently intense it can itself supply a full substitute stimulus to give a weak approximation of R_1."[6]

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[4] Other writers have commented on the lack of similarity between conditioned and unconditioned responses; cf. E. G. Wever, The upper limit of hearing in the cat, *J. Comp. Psychol.,* 10, 1930, 231-232; C. C. Tolman, *Comparative Psychology,* 1934, 317-408; E. R. Hilgard and A. A. Campbell, Vincent curves of conditioning, *J. Exper. Psychol.,* 21, 1937, 310-319; E. R. Guthrie, *The Psychology of Learning,* 1935, 258.

[5] This type of anticipatory phenomenon has been noted in other work; cf. G. L. Freeman, The galvanic phenomenon and conditioned responses, *J. Gen. Psychol.,* 3, 1930, 524-539, and E. R. Hilgard, Conditioned eyelid reactions to a light stimulus based on the reflex wink to sound, *Psychol., Monog.,* 41, 1931 (no. 184), 1-50.

While still adhering to the above account in principle, we see that the conditioned postural adjustment might also be regarded as a tentative approximation of an about-to-be overt withdrawal reaction.[7] In other words, this experiment is not a critical test either of the hypothesis outlined above, or of an alternate explanation which makes no distinction between tonic and phasic response elements.[8] Action potential records might with suitable controls indicate whether the conditioned 'set' of a muscle differs qualitatively as well as quantitatively from the reaction of that muscle to the unconditioned stimulus. No acceptable evidence for this assumption was found in the records which we obtained, but more adequate instrumental procedures might reveal such differentia. As they stand the results show that the more specific the stimulus-response connections in conditioning experiments, the more diffuse is the postural adjustment.

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[6] G. L. Freeman, *op. cit.*

[7] Mowrer (*op. cit.*, 73), however, thinks that while the postural set is modified by training, it is not fundamentally a learned phenomena but is due to "a tendency on the part of every reaction, once activated, to show following a relatively brief period of refraction, a more prolonged period of increased excitability."

[8] R. S. Woodworth, *Psychology*, 1934, 327; cf. also this JOURNAL, 51, 1938, 130.

AUDITORY MEMORY SPAN AS TESTED BY SPEECH SOUNDS

By VIRGIL A. ANDERSON, Stanford University

Memory span, defined as the number of discrete elements grasped in a given moment of attention and organized into a unity for purposes of immediate reproduction or immediate use, is determined in various ways.[1] One method is to determine the largest number of relatively discrete units that an individual is able to retain and reproduce immediately after one presentation. Various materials have been used, but the types most frequently employed have been digits, letters, short words, and nonsense syllables, with digits and letters predominating.[2]

While many of the investigators have dealt with the problem of memory span in general without reference to the particular type of mental imagery employed by the subjects, a number of them have attempted to construct a test that would involve one type of mental imagery in relative isolation.[3] The sensory faculty usually selected for study is either the visual or the auditory. It has apparently been assumed, however, that the method of presentation itself was capable of determining the kind of ideation involved in the test. Hence, visual presentation has been used to determine the visual span and auditory presentation for the auditory span.

Careful study of this problem, however, leads to the conclusion that other factors, in addition to the method of presentation, are operative in determining the type of ideation that the subject will use.[4] Among these factors are the nature of the material employed in the test and the method of reproduction. Previous experimenters have often failed to control these factors, having simply used digits, for example, with graphic reproduction for both the visual and the auditory tests. With material so readily visualized as digits, especially when reproduction is written, it appears safe to conclude that no valid claims can be made for auditory memory, even though the presentation may be oral.

Object. The purpose of the present investigation was to study the characteristics of the auditory memory span, and to determine what use is made of memory span and what part it plays in various mental activities, when stimulus-material of a predominantly auditory nature is used.

Procedure. Isolated speech sounds were selected as being the most difficult material to visualize and the most free from possible meaningful associations. When these sounds are given auditorially and are sounded, not merely named, and with oral reproduction, there is a minimum opportunity for visual imagery to function. With respect to some of the sounds of English, for example, it can be said that there

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[1] Twenty-seven different ways of determining the memory span are described by J. P. Guilford and K. M. Dallenbach in an article in which they propose still another way (The determination of memory span by the method of constant stimuli, this JOURNAL, 36, 1925, 621-628).

[2] For the various materials used in experiments on memory span see A. B. Blankenship, Memory span: A review of the literature, *Psychol. Bull.*, 35, 1938, 4 f.

[3] Cf. Blankenship, *op. cit.*, 5 f.

[4] V. A. C. Henmon, The relation between mode of presentation and retention, *Psychol. Rev.*, 19, 1912, 79-96.

does not exist, for the individual untrained in phonetics, a specific visual symbol which always represents that sound and that sound alone.

Accordingly, two tests were devised, the one employing eight English vowels and diphthongs; namely, [i] key, [æ] hat, [eɪ] say, [aɪ] lie, [a] father, [ɔ] awe, [oʊ] oh, [u] who, and the other test consisting of the six voiceless consonants, p, t, k, sh (she), s, and f. The tests were given separately. The sounds were presented strictly as *sounds,* with no reference to the alphabetical or other visual symbols which are used to represent them. They were sounded in a monotone without rhythm or accent at the rate of one per second with a short period of silence separating them so that each one formed a complete sound-unit in itself. Reproduction was immediate and oral. A total of 132 students from the University of Wisconsin were tested.

Methods of scoring. Two methods of scoring the reports of the *S*s were used. One was the weighted-score method described by Peatman and Locke.[5] Briefly, this method consists in establishing a basal score, which is that series immediately preceding the first failure. To this basal score is added a unit or fraction of a unit for each success above it regardless of intervening failures. Since each of the present tests was composed of three group of series, one-third of a unit was added to the basal score for each success above it. In addition to the weighted score, a raw score was also computed by a method similar to that used for scoring the digit span tests found in the Stanford Revision of the Binet-Simon Scale.[6] That is, three trials were given for each length of series, and the raw score was taken as the longest series correctly reproduced immediately preceding the first failure on all three trials.

Results. (1) The span for consonants was found to be materially lower than that for vowels. The vowel test yielded a mean weighted score for the group of 4.73 and a raw score of 5.46. The mean weighted consonant score was 3.83 and the raw score was 4.66. All of these values are substantially below the commonly accepted norms for digits. No significant sex differences were found, the slight differences which did exist always being in favor of the women.

(2) A reliability coefficient of 0.86 was obtained for the vowel test from a retest of a college class of 30 *S*s after a semester of phonetic training. The method was that of self-correlation using the Pearson Product-Moment formula. The reliability of the consonant test was considerably lower, only 0.31. While performance on the consonant test was definitely improved by the phonetic training, there being a difference of 0.20 between the mean weighted scores at the beginning and at the end of the semester, the vowel test scores remained relatively constant, showing a small loss, rather than a gain, of —0.05 between means.

These results indicate that the auditory memory span for vowel sounds is relatively independent of phonetic training. In the case of the consonant test, however, such training tends to improve performance but not proportionally for all members of a group.

(3) A correlation coefficient of +0.47 ±0.046 was found when vowel scores were compared with consonant scores. This was interpreted to reflect the possible influence of various extraneous factors which were discovered to affect performance

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[5] J. G. Peatman and N. T. Locke, Studies in the methodology of the digit span test, *Arch. Psychol.,* 25, 1934, (no. 167), 1-35.

[6] L. M. Terman, *The Measurement of Intelligence,* 1916, 1-362.

on the consonant test to a greater degree than on the vowel test. These factors, which are only mentioned here by way of summary, include the influence of phonetic training on consonant scores mentioned in *(2)*, the relative ability of the *S*s to make fine discriminations among speech sounds when hearing them pronounced, the greater readiness with which consonants are visualized by the average *S*, and the difficulty which some *S*s experience in attempting to reproduce isolated consonant sounds orally.

(4) Pitch discrimination was found not to be related to auditory memory span for the group studied. This was indicated by the low correlation of +0.12 between scores on the Seashore Sense of Pitch test and weighted scores on the vowel sounds test for a group of 55 *S*s.

(5) Within the limits of the group studied, there was no evident relationship between auditory memory span, as measured by these tests, and auditory acuity as measured on an audiometer. Elaborate hearing tests were given to a group of 28 *S*s included in the present study.[7] No defects of hearing were found among those *S*s making the lowest scores on the memory span tests within this group nor did those for whom a slight hearing loss was discovered present the shortest memory spans.

TABLE I

Showing Relationship of Mean Auditory Memory Span Scores to Years of Foreign Language Study

Years Studied	No. Ss	Mean vowel scores		Years Studied	No. Ss	Mean vowel scores	
		Weighted	Raw			Weighted	Raw
1	1	3.00	5.00	6	21	4.70	5.45
2	10	4.90	5.86	7	23	5.06	5.76
3	17	4.20	5.04	8	12	4.86	5.58
4	13	4.56	5.33	9	4	5.16	5.75
5	25	4.49	5.32	10+	6	5.33	6.12

(6) Significant evidence was discovered of a positive relationship between auditory memory span and ability in and learning of foreign language. In the first place, when the high school and college grades earned by the *S*s in foreign language study were correlated with their weighted auditory memory span scores for vowels, the coefficient of correlation obtained from the Pearson formula was found to be +0.34 ±0.05. Since this *r* is almost seven times its probable error, some degree of correlation is indicated. It is not particularly high, however, and may reflect in part the fact that a still higher correlation was found between memory span scores and general university grade point averages, as will be shown in *(7)*. It should also be noted that foreign language grades are based upon written work as well as oral performance, and that school grades in general constitute at best a doubtful indicator of ability.

Of more interest and possibly more importance were the results obtained from a study of other items relative to the *S*s' foreign language background and achievements. Table I shows a comparison between years of foreign language study and the mean weighted auditory memory span scores for vowels made by those *Ss* whose total time of language study is represented by those particular years. It can be seen that with the exception of the groups represented by the second and seventh years, the relationship is very close. In any case, a high degree of correspondence is indicated.

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[7] Merle Ansberry, Auditory deficiencies of acuity and their correction with prosthetic appliances, Ph.D. Thesis, University of Wisconsin, 1937.

One should not conclude from this, however, that foreign language study increases memory span, or even that it improves performance on the tests. Rather, the more likely conclusion appears to be that those individuals possessing superior auditory memory spans found foreign language study comparatively easy and interesting, received relatively good grades in it, and as a result took more years of it than those less fortunate students whose shorter memory spans, among other things perhaps, made foreign language study more difficult and arduous. A number of facts appear to support this conclusion. In the first place, the results obtained from the retest of the group discussed in section *(2)*, after a semester of phonetic training, indicate that the study of a foreign language would likely have little effect upon the auditory memory span scores for vowels. Of similar import are the results obtained from a comparison of memory span scores made by *Ss* coming from homes in which a foreign language was spoken (in addition to English) with those coming from homes in which only English was used. As Table II shows, the mean scores made by *Ss* from bi-lingual families are actually lower than those made by *Ss* from mono-lingual families.

TABLE II

SHOWING RELATION OF MEAN AUDITORY MEMORY SPAN SCORES TO FOREIGN LANGUAGE BACKGROUND AND ACHIEVEMENT

Background and achievement		No. Ss	Mean vowel scores	
			Weighted	Raw
A. Foreign language spoken in in the home:	Yes	41	4.61	5.34
	No	91	4.74	5.51
B. Foreign languages learned:	Easily	47	5.00	5.73
	Average	57	4.59	5.36
	With difficulty	28	4.36	5.11
C. Study of foreign language:	Enjoyed	46	4.92	5.65
	Average	64	4.61	5.37
	Disliked	20	4.53	5.35
D. Speaks a foreign language:	Not at all	27	4.50	5.29
	With difficulty	56	4.70	5.52
	Fairly well	46	4.87	5.46
	Fluently	4	5.25	6.50

That memory span may on the contrary contribute to language ability, is further suggested by the remaining sections of Table II. In every instance, save one of the raw scores in Section D, there is a perfect correspondence between mean auditory memory span scores and indicated foreign language aptitude and achievement.

(7) Auditory memory span is positively related, but only moderately, to the capacity to do school work, to the extent that that capacity is indicated by school grades. A correlation coefficient, using the Pearson formula, of +0.37 ±0.052 was

obtained between weighted memory span scores for vowels and University of Wisconsin grade point averages.

(8) Auditory memory is also involved to a moderate degree in the complex of abilities measured by the University English Usage Examination, which the University of Wisconsin gives routinely to entering freshmen. When percentile rankings on the English Examination were correlated with the weighted scores on the vowel test, a coefficient of +0.35 ±0.07 was obtained.

(9) Auditory memory span for vowel sounds bears a slightly lower degree of relationship to general intelligence. Two measures of intelligence were used: The American Council Psychological Examination, which is routinely given to entering freshmen, and intelligence quotients. Percentile rankings on the first measure yielded a correlation coefficient with weighted memory span scores for vowels of +0.307 ±0.058. The second measure showed a correlation of +0.325 ±0.059.

Summary. On the basis of the findings brought out in this study, the following conclusions are indicated.

(1) The memory span for speech sounds is materially lower than the accepted norms for digits.

(2) The auditory memory span for speech sounds, particularly vowels, appears to be independent of phonetic training and a foreign language environment. The vowel sounds test was found to be highly reliable.

(3) Neither pitch discrimination nor auditory acuity is related to auditory memory, as measured by the vowel sounds test.

(4) A relationship is indicated between auditory memory span for speech sounds and ability in and learning of foreign language. The findings indicate that this relationship is in the nature of a partial dependence of language skill upon memory span.

(5) Auditory memory span is positively related, but only to a moderate degree, to school grades, knowledge of English usage, and general intelligence. It is worthy of note that the obtained coefficients of correlation with all of these measures are very similar.

(6) Speech sounds, and especially vowel sounds are highly desirable materials for a test of the auditory memory span.

EXTINCTION, SPONTANEOUS RECOVERY,* AND FORGETTING

By G. H. S. Razran, Columbia University

If an extinguished conditioned response (CR) has recovered some of its lost ground after a lapse of a certain time, we usually assume that the amount of recovery is in some way directly proportional to the amount of extinction. If the CR has been only partly extinguished, another legitimate postulate appears to be that the quantitative total of the recovery is affected in a degree by some forgetting of the non-extinguished portion of the CR. A third contention that the recovery itself suffers some forgetting is also not without merit. To illustrate the possible workings of these three hypotheses: Let us suppose that a CR which had been extinguished to 40% of its normal value was retested after a lapse of 48 hr. and that during this time recovery was 50% of the amount of extinction and forgetting 25% of the amount of conditioning. Then, if we hold only to the first hypothesis, the recovered CR should be 70% of its normal value (40 plus 50% of 60). Add the second supposition and the result will be only 60% (minus 25% of 40), while the third assumption would further reduce the recovery by a small percentage (the precise amount depending upon the time curves of recovery and forgetting). Now, all this is not just a matter of theory, but is subject to rigid experimental verification. Either by keeping recovery time constant and varying the degrees of extinction or by varying time and holding extinction constant, we obtain data from which we may determine not only the number of constituent factors of spontaneous recovery but also their exact individual contributions. In the present experiment the first and easier method of inquiry was chosen; namely, recovery times was always 48 hr. and extinction ranged from 27% to 67%.

Method and procedure. All the 12 adult human subjects of this experiment previously had salivary CRs to various visual and auditory stimuli. The old CRs were, however, also previously extinguished and all the *S*s re-formed new CRs to a red colored 15-w. light. The reconditioning was very fast and the new CRs were soon quite stable and regular, ranging from means of 198 to 316 mg. of net conditioned salivation in 1-min. periods. The *S*s were then divided into 6 equated groups and each group was given a different number of extinction trials: 4, 8, 12, 16, 20, and 24. The intervals between extinction trials were 2 min.—1 min. of which was a control trial—and the extinction trials themselves were of 1-min. duration. All the CRs were retested, 48 hr. after extinction, for spontaneous recovery.

The method of measuring saliva and the writer's general CR methodology have been described before and will not be detailed here.[1] The saliva is measured by increments in weights of dental cotton rolls (Johnson and Johnson, No. 3, 0.5 x 1.5 inches) inserted under *S*'s tongue for short periods of time, usually 1 min. To prevent evaporation, scale-corrosion and absorption, the rolls are weighed in small envelopes and reweighed in the envelopes immediately after their removal from *S*'s

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* Accepted for publication December 7, 1938.

[1] G. H. S. Razran, Conditioned responses: An experimental study and a theoretical analysis, *Arch. Psychol.*, 28, 1935 (no. 191), 1-124; A simple technique for controlling subjective attitudes in the salivary conditioning of adult human subjects (in press, *Science*).

mouth. The general methodology of the CR involves ruling out interfering subjective attitudes by misinforming the *Ss* about the true purpose of the experiment. As a rule this can be achieved only when a *series* of stimuli is presented during a comparatively long eating period (2-4 min.) and not with the traditional single-stimuli-plus-brief-reënforcements method. In the present study, the eating periods were 2 min. in length during the course of which the 15-w. lights were presented intermittingly 48 times (24 times in the 1-min. test-periods). The *Ss* were misinformed about the purpose of the study by being told that it was "to find out the effect of eye-fatigue upon digestion." The experiment was conducted in a fairly soundproof room, between 3 and 5 P.M. In no case did any *S* show any evidence that he "caught on" to the true purpose of the experiment.

Results and conclusions. All the results are presented in Table I. In the first three rows of the table are given the magnitudes of the initial, extinguished, and recovered CRs in mg. of conditioned salivation per 1-min. periods, every entry being a mean of 2 *Ss*. The next three rows show the extinguished CR, the recovered CR, and the amount of extinction as percentages of the values of the initial CR. Then, come two rows in which recovery is presented both when no forgetting of the non-extinguished portion of the CR is assumed and when 14% of such forgetting is postulated. Finally, the last three rows sum up in a way the entire findings of the experiment in revealing recovery as a percentage of extinction in accordance with the three hypotheses discussed in the beginning of the article; namely, (1) when no-

TABLE I

Spontaneous Recovery of Salivary Conditioned Responses of 12 Adult Human Ss After 48 Hr., as Dependent upon the Degree of Extinction and Forgetting

Entries in first three rows represent means of net conditioned salivation of 2 Ss in mg. of saliva per 1-min. periods. Entries in the next five rows are expressed as percentages of initial preextinction conditioned salivation. The last three rows present recovery as a percentage of extinction.

Experimental conditions	Extinction stages (number of extinction trials before recovery)					
	4	8	12	16	20	24
Magnitudes of initial CRs	316	272	295	236	250	218
Magnitudes of extinguished CRs	231	160	162	109	98	72
Magnitudes of recovered CRs	248	201	211	161	164	137
Extinguished CRs as percentages of initial CRs	73	59	55	46	39	33
Percentage of extinction	27	41	45	54	61	67
Recovered CRs as percentages of initial CRs	78	74	72	69	66	63
Percentage recovery when no forgetting is assumed	5	15	17	23	27	30
Percentage recovery when 14% forgetting is assumed	15.2	23.3	24.7	29.4	32.5	34.6
Recovery as a percentage of extinction when no forgetting is assumed	17.8	36.6	38.0	42.6	44.3	44.7
Recovery as a percentage of extinction when 14% forgetting is assumed	56.3	56.7	54.9	54.5	53.2	51.7
Recovery as a percentage of extinction when an additional 5% of forgetting of recovery itself is assumed	57.1	57.9	56.1	56.0	54.8	53.4

forgetting of the non-extinguished portions of the CRs is assumed, (2) when such forgetting (14%) is posited, and (3) when an additional forgetting (5%) of the recovery itself is further supposed.

Examining the results we see rather clearly that the hypothesis that spontaneous recovery is solely a function of extinction is much less satisfactory than the one in which a factor of forgetting is also assumed. In the first case when no forgetting is postulated, the dependence of recovery upon extinction is extremely irregular, ranging from 17.8% to 44.7%; where a forgetting of 14% is postulated, the relation becomes nearly linear, varying only from 51.7% to 56.7%. (Even greater linearity is attained when some additional forgetting of the recovery itself is postulated, but the difference is small.) We may, therefore, conclude: (a) that the amount of spontaneous recovery of a partly extinguished CR is a composite function of the amount of extinction and of the amount of forgetting of the non-extinguished portion of the CR; and (b) that the slope of recovery is much steeper than that of forgetting. After 48 hr. the amount of forgetting was 14% and of recovery 53%-57%.[2]

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[2] It is interesting to treat in this manner the results of C. I. Hovland (*J. Exper. Psychol.*, 21, 1937, 52), whose study is the only one that is in any way comparable with that of the writer. Hovland found that, after 24 hr. a CR which had been extinguished to 65.1% of its normal value recovered 12.6%, while a CR of 90.5% of its initial effectiveness lost 7.8%. This apparent contradiction is readily resolved by assuming a factor of forgetting and solving the simultaneous equations of $9.5x - 90.5y = -7.8$, and $34.9x - 65.1y = 12.6$; where x and y are respectively the rates of recovery and forgetting. The obtained values are $x = 0.64.6$ and $y = 0.15.4$; which are very much in line with what is known about the rate of forgetting on non-extinguished CRs and the rate of recovery of CRs that had been completely extinguished.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CORNELL UNIVERSITY

LXXXVIII. ACQUISITION BY CHILDREN OF THE CAUSE-EFFECT RELATIONSHIP

By JOHN IRVING LACEY and KARL M. DALLENBACH

In 1926 there was reported the first of a series of studies of children's learning of logical relationships.[1] In that study the relationship of opposition was investigated with two children. Neither of the two understood the meaning of opposition at the beginning of the test, but the older child, a 6 yr. old boy, learned it immediately upon gaining insight into the nature of the test problem. The younger child, a 3 yr. old girl, was, however, unable to learn it despite coaching with numerous illustrative examples. Thus age seemed to be a factor in the learning, and learning seemed to be attained suddenly and not in the gradual manner required by the trial-and-error theories.

Because of the theoretical significance of these results, the study was extended in 1929 to 100 school children whose ages ranged from 5 to 7½ yr.[2] The new results confirmed the anticipations of the earlier study. Age was found to be a condition and learning was found to come suddenly with the acquisition of insight. Children of the social status of the group under investigation know the meaning of opposition without special instruction by the end of the eighth or the beginning of their ninth year and in the age-group 5·3-5·9 there is a probability of 0.5 of learning the relation after a small amount of instruction.

Recently, Schooley and Hartmann broadened the scope of the study to include the relationships of whole-part, part-whole, action-agent, agent-action, attribute-substance, substance-attribute, genus-species, and species-genus.[3] They followed the method adopted in the second study, except that they decreased the number of test-words from 25 to 10. The conclusion they reached was that the learning of standard logical relationships is not a slow, gradual, trial-and-error process, but rather a sudden and insightful process, giving evidence of permanent mastery. Their results indicate, for children of the social status of their *S*s, a probability of 0.5 of learning the relationships that they studied at the age-levels 5·1-5·7 to 6·4-6·10.

The purpose of the present study is to carry the investigation to the relation of cause-effect.

METHOD

Selection of test-words. The cause-effect relationship is difficult both for the children who give the replies and for the experimenter who has to decide whether

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* Accepted for publication February 15, 1938. The experiments were conducted in the Ithaca public schools. We acknowledge our indebtedness to Mr. C. L. Kulp, Superintendent of the Ithaca Schools, and to the teachers who assisted us in this study.

[1] K. M. Dallenbach, A note on the immediacy of understanding a relation, *Psychol. Forsch.*, 7, 1926, 268 f.

[2] George Kreezer and K. M. Dallenbach, Learning the relation of opposition, this JOURNAL, 41, 1929, 432-441.

[3] Mary Schooley and G. W. Hartmann, The rôle of insight in the learning of logical relations, this JOURNAL, 49, 1937, 287-292.

the replies, frequently ambiguous, are adequate. Much depends upon the test-words. We, therefore, determined empirically, in a preliminary experiment, what words were meaningful in a cause-effect context for children of ages 5 to 7½ yr. We selected the words from Thorndike's list.[4] Our two criteria of selection were: (1) that the words fit easily into a question; and (2) that they be 'heavily weighted' by Thorndike. The words selected were then put into simple questions to which the answer was a categorical 'yes' or 'no.' Sample questions are:

(1) Would you *burn* yourself if you put your hand in a fire?
(10) Would you be *thirsty* if you did not drink anything?
(20) Would *night* come if the sun stayed in the sky?

Four-page booklets, convenient for handling, were prepared with numbered lines containing the words "yes" and "no." The *Ss* were instructed to listen carefully to the questions, read one at a time, and to underline the appropriate word, "yes" or "no." They were also cautioned not to whisper, answer aloud, or copy from their neighbors. On the whole, excellent coöperation was secured.

This preliminary investigation covered pupils in three class-rooms: one kindergarten and two first-grade. In all, we secured 94 papers which we could use. We scored the papers, summed the number of times every sentence was answered correctly, and translated the sums into percentages. When the rank-order coefficient of correlation between the percentages for the words and the Thorndike weighting was calculated and translated into a product-moment coefficient,[5] we found that $r = +0.06$, P.E., 0.12.

The lack of covariability between our results and Thorndike's weights can be explained in several ways. The possibility that directly concerned us, however, was that our sampling had been poor. To check this possibility, the papers were divided at random into halves and the rank-order coefficient between the halves was calculated, and translated, as before, into a product-moment coefficient. We found $r = +0.86$, P.E., 0.06, which indicates that our sampling was reasonably reliable. We, therefore, selected as test-words for the main experiment, the words (see Section E under Procedure) in those sentences which had been most often answered correctly.

Subjects. Age and school attendance were the only criteria in the selection of the *Ss*. Since the work of Schooley and Hartmann demonstrated no reliable sex difference, we paid no attention to that factor. Twenty children were chosen for every one of the following age-groups: 5·0-5·6 (Group I); 5·7-5·12 (Group II); 6·0-6·6 (Group III); 6·7-6·12 (Group IV); 7·0-7·6 (Group V); 7·7-7·12 (Group VI); 8·0-8·6 (Group VII); 8·7-8·12 (Group VIII).

Procedure. Our procedure was comparable in all respects with that of Schooley and Hartmann. When *S* was brought to the experimental room, he was first put at his ease. Then he was asked:

(A) "Would you like to play a game of 'cause-effect' with me this morning?" After securing the child's assent, *E* asked, "Do you know what cause and effect mean?"

(B) To a *negative* answer, *E* said, "Yes you do. Tell me what is the cause of *sleep*." To an *affirmative* answer, *E* said, "All right, tell me what is the cause of *sleep*."

(C) To an *incorrect* answer in (B), *E* said, "No, you are wrong; that is not the cause of sleep. Now tell me what is the cause of a *blister*."

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[4] E. L. Thorndike, *The Teacher's Word Book,* 1921, 1-134.

[5] H. E. Garrett, *Statistics in Psychology and Education,* 1925, 192.

To a *correct* answer, *E* said, "That's right; now tell me the cause of a *blister*."

(D) To an *incorrect* answer in (C), *E* said, "No, you are wrong. Now listen. People *sleep* because they are tired; people get *blisters* because they burn themselves. The cause of sleep, then, is being tired; the cause of a blister is a burn. Do you understand now what cause and effect mean?"

To a *correct* answer, *E* said, "That's right," and then repeated the child's answers and concluded with "Do you understand now what cause and effect mean?"

(E) To a *negative* answer in (D), *E* said, "Well, I'll see if you do. Tell me what is the cause of (1) *bleeding*—(2) *smile,* (3) *smoke,* (4) *being hungry,* (5) *melting,* (6) *eating,* (7) *drinking,* (8) *sickness,* (9) *punishment,* (10) *being thirsty?*"

To an *affirmative* answer, *E* said, "All right, I'll see if you do. Tell me what is the cause of *bleeding*," etc.

(F) As *S* returned his answers, *E* simply replied "right" or "wrong." If *S* did not show, by his answers to the first 6 words of the test list, that he understood the relationship, *E* again told him the correct answer to *sleep* and to *blister* and then repeated the first six words, saying "right" when a correct answer was returned and supplying a correct answer when an error was made. The last four words were given without correction or comment.

S was credited with having learned the cause-effect relationship if he replied correctly to any one of the first six words of the test-list and to all the remaining words. We did not demand scientifically accurate causes, but merely common-sense explanations which would indicate that the relationship was understood. An 'after-this-therefore-because-of-this' explanation sufficed.

RESULTS

(1) Age at which the relation is understood. Table I analyzes and summarizes the results of the experiment. The first four sections of our procedure were designed to elicit the following information: (1) Does the child affirm that he understands the meaning of cause and effect when those terms are used alone? (Section A); (2) Does the child understand the meaning of the terms when used in specific questions? (Sections B and C); (3) Does the child affirm an understanding after this instruction? (Section D).

To the question in Section A ("Do you know what cause and effect mean?"), only 2 out of 160 *S*s answered "yes." Yet in Sections B and C 67 were able to give correct answers to specific questions. Of these, 35 answered correctly in both sections, 7 in B alone, and 25 in C alone. To the question in Section D ("Do you understand now what cause and effect mean?"—practically a repetition of the question in Section A) 156 *S*s answered "yes;" and though many (84) demonstrated later in Sections E and F that they did understand, almost as many (76) failed, according to our criteria, to do so.

Table I shows, in Sections B and C, that knowledge of the cause-effect relationship is correlated with age. The younger children failed in these preparatory sections of the experiment; the older children, as their age-level advanced, more and more demonstrated knowledge though they had just disclaimed it in Section A. The result, that children in general understand the meaning of cause and effect by the end of their eighth or the beginning of their ninth year, corroborates those obtained by the earlier experimenters with other relations.[6]

(2) Age at which the relation is learned. Sections E and F in Table I show the

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[6] Kreezer and Dallenbach, *op. cit.,* 434 f.; Schooley and Hartmann, *op. cit.,* 290.

number of *S*s in the different age-groups who demonstrated that they had, according to our criterion, learned this relationship. The percentage of success varies with age. Since the frequencies follow in general the distribution of an ogive curve—the inversion of the first-order at Age-Group VI is probably an error of sampling—an age-limen was calculated by the method of constant stimuli,[7] as in the earlier studies.[8] The value obtained was L = 6.81 (h = 0.25).

TABLE I

Comprehension of Cause and Effect at Different Ages and in Different Sections of the Experiment

Section	Answer	Age-Group*								
		I	II	III	IV	V	VI	VII	VIII	Totals
A	Yes	0	0	1	0	1	0	0	0	2
	No	20	20	19	20	19	20	20	20	158
B and C	Both correct	1	3	1	3	4	6	8	9	35
B alone	Correct	0	0	1	0	1	2	3	0	7
C alone	Correct	1	3	0	4	4	3	5	5	25
D	Yes	20	19	20	20	18	20	19	20	156
	No	0	1	0	0	2	0	1	0	4
E	Correct	3	7	5	9	7	9	13	17	70
F	Correct	1	1	5	2	5	0	0	0	14
E and F	Correct: No.	4	8	10	11	12	9	13	17	84
	%	20%	40%	50%	55%	60%	45%	65%	85%	
	Failures:									
	Instable	15	9	9	9	8	11	7	3	71
	Complete	1	3	1	0	0	0	0	0	5
	Total	16	12	10	9	8	11	7	3	76

* I=5 yr. 0 mo. to 5 yr. 6 mo.; II=5·7–5·12; III=6·0–6·6; IV=6·7–6·12; V=7·0–7·6; VI=7·7–7·12; VII=8·0–8·6; VIII=8·7–8·12.

Our age-groups, it will be recalled, have 6-mo. ranges. Since the group-median was used in the calculations, the value for L must also be construed as a median and the age-group to which it corresponds should be considered as extending from 6.56-7.06 yr. These computations indicate, therefore, that children of the 6·7-6·12 age-group have, under the conditions of this study, a probability of 0.5 of learning the cause-effect relationship.

Of the logical relations thus far considered, the age-limen for cause-effect is the highest (see the accompanying summary). The spread of the various age-limens is not wide; it extends over a period of only 17 mo., from 5·4 to 6·9. The narrow range

Relationship	Age-limen (yr. & mo.)	Relationship	Age-limen (yr. & mo.)
action-agent	5·4	genus-species	5·11
agent-action	5·4	whole-part	5·11
opposite	5·6	part-whole	6·1
attribute-substance	5·10	species-genus	6·7
substance-attribute	5·11	cause-effect	6·9

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[7] Cf. E. G. Boring, Urban's tables and the method of constant stimuli, this Journal, 28, 1917, 280-293.

[8] Kreezer and Dallenbach, *op. cit.*, 439; Schooley and Hartmann, *op. cit.*, 290 f.

of these limens suggests that we are dealing with a capacity or an ability that depends upon maturation. The failure of the younger children to learn the meaning of the various relations studied supports that view. Despite numerous repetitions of illustrative examples, the younger children simply had not matured sufficiently to learn the relational significance.

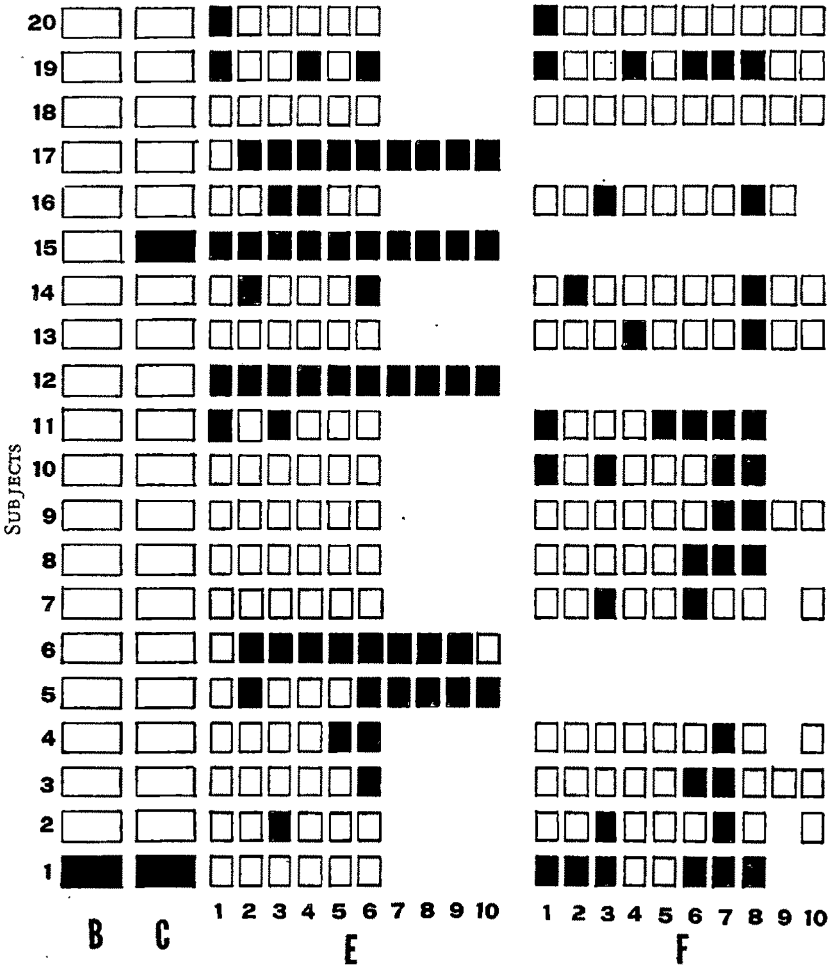
(3) Manner in which the relation was learned. All the *Ss* credited with learning the relation—E and F in Table I—learned it suddenly or insightfully. From associations whose determinations were unknown and incorrect within the meaning of our experiment, their answers shifted abruptly to associations determined by their comprehension of the causal relation. Their replies to one word were incorrect but to the next and to all subsequent words were correct. The learning curves dropped suddenly from 100% to 0% failures. From the moment that the children gained insight, their answers were correct and in general quickly and confidently given.

The *Ss* failing to learn divide into two distinct groups (see Table I). Some of them (5 in number, all from the lower age levels) failed completely. These failures are like those reported in the earlier studies: the replies to all the test-words were incorrect. The illustrative examples did not give those *Ss* insight into the problem and they did not learn. Other *Ss* (71 in all and distributed in decreasing numbers from the lowest to the highest age-groups) were marked as 'failures,' in accordance with our criteria of learning, because their correct and incorrect replies were interspersed. Their comprehension was apparently 'instable.' These *Ss* indicated that they had learned the relation at one test-word only to demonstrate at the next, by missing the relation entirely, that they had not. For example, *S* 8 (Age-Group VIII) replied to *drinking,* the seventh test-word, with "because you're thirsty"—an adequate cause-effect relation; his reply to *sickness,* the eighth test-word, was "you are ill"—a relation of similarity. Nothing comparable to this type of failure was obtained in any of the earlier studies. In the earlier studies, no *S* failed after a success; once a correct answer, always a correct answer. In the present study, however, a correct answer to one word was no guarantee that correct answers would be received to the following test-words.

Figs. 1 and 2 present graphically the results in Sections B, C, E, and F of our procedure for Age-Groups I and VIII—our extremes. The *Ss* are listed by number along the ordinates: the various sections of the procedure, and the test-words in Sections E and F, are arranged along the abscissæ. Black rectangles indicate success, open rectangles failure, and blank spaces omissions required by our procedure. These figures demonstrate clearly what happened, in varying proportions, in all the age-groups. The 'instable' type of performance is illustrated by the records of 15 *Ss* in Age-Group I and by *Ss* 2, 5, and 8 in Age-Group VIII.

The 'instable' records may be a function not of learning but of the test-words employed. Though we sought, by means of preliminary experiments with kindergarten and first-grade pupils, to select test-words that were comparable in difficulty, it may well be that all of our words did not demand the same degree of knowledge. If one word was 'easy' and the next 'hard,' the instable records are understandable. Since the experimental procedure did not permit us to go back of the *Ss'* replies, we have no way of knowing whether this explanation is valid. It is obvious, however, that *S* may fail in a particular test-word, not because he has failed to learn the causal relationship but because (1) he does not know an adequate or sufficient cause

(2) he lacks the necessary vocabulary,[9] (3) his intended meaning is not understood by *E*, and (4) he is distracted by remote causes. As an example of (3): *S* 1 (Age-Group I) replied to *bleeding* with "beets." By some context peculiar to the child,



SECTIONS

FIG. 1. COMPREHENSION OF CAUSE AND EFFECT IN AGE-GROUP I (5·0-5·6) IN DIFFERENT SECTIONS OF THE EXPERIMENT
(Black rectangles designate correct answers; open rectangles, incorrect answers.)

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[9] Victoria Hazlitt (Children's thinking, *Brit. J. Psychol.,* 20, 1930, 354-361) shows that children may understand a relationship that they are unable to verbalize.

that might be a correct answer, although it was considered wrong by *E.* Even more extreme instances of this sort may have occurred, but they of course could not, under the procedure of the present study, be detected by *E.* As an example of (4): thirst, as the cause of *drinking,* may be known by *S* but he may nevertheless reply, "I do not know," because he does not know what is the cause of thirst. Unless, therefore, test-words are chosen which do not yield these incorrect and ambiguous replies, or a procedure is devised which either avoids or detects them, we cannot be sure that the results are not in part at least, a function of the test-words employed.

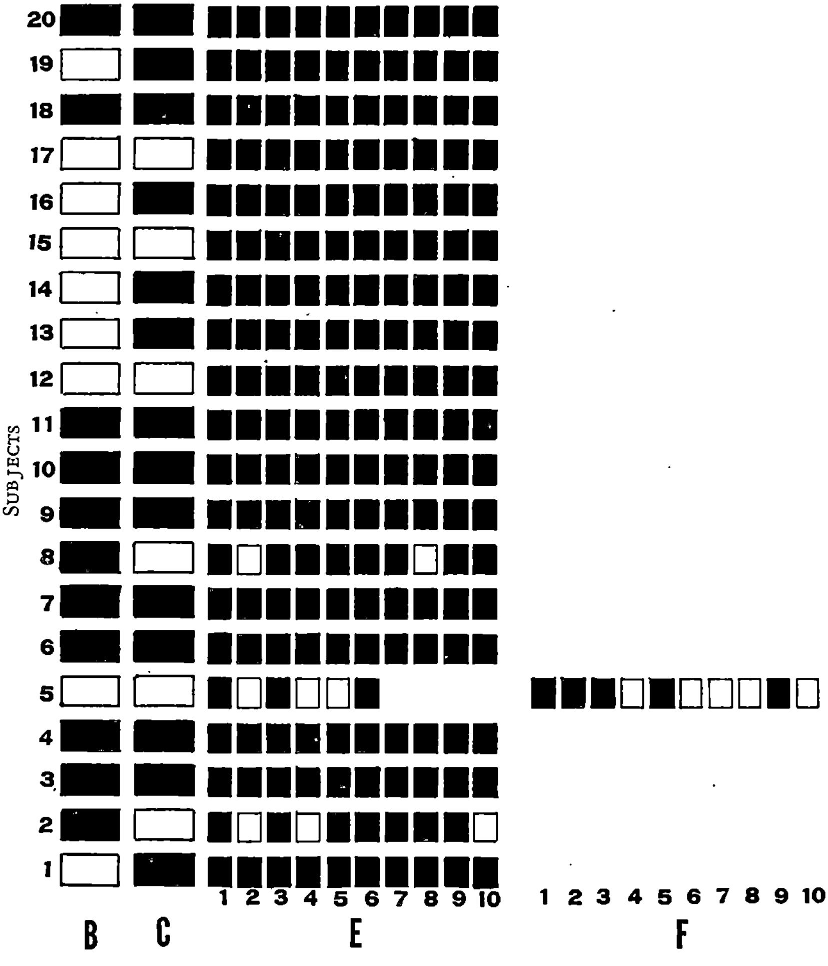

FIG. 2. RECORDS FOR AGE-GROUP VIII (8·7-8·12)

Which, then, of the 'instable' *Ss*, if any, should be credited with learning the relation? Or should all of their records be regarded as evidence of trial-and-error learning? It is true that in this study, though not in the earlier ones, the criterion of learning was 'loaded' in favor of insightful learning; all the *Ss* who failed after a success were classified as not having learned the relation. In other words, the *Ss* either learned insightfully or they were not credited with learning. The hypothesis of trial-and-error learning does not, however, resolve the difficulty. For what item in the various test-situations is repeated or exercised? The content and meaning of the various situations are entirely different. There are no identical or constant items among them to be exercised or 'stamped in.' Every correct reply is new and after it has been given, though 'stamped in' by the law of effect, it would be incorrect for the subsequent test-word. Frequency will not help in the explanation and recency, if effective, would inevitably lead to a continuous series of errors. These theories of learning cannot, therefore, explain even the 'instable' reports, which at first inspection seem to be particularly appropriate examples of trial-and-error.[10]

The 'instable' records, and their ambiguity, place the cause-effect relationship in a slightly different category from the other relations that have been studied. It is the most difficult relation from the point of view of experimental control and interpretation of results, and it also seems to be the most difficult for the *Ss* to learn. If, however, the 'instable' performances are to be regarded as instances of learning and the lapses explained as we have suggested above, then, since there were only 5 complete failures—a decidedly smaller number, relatively, than in any of the other relations studied—the causal relation is the easiest of all.

SUMMARY

(1) Children of the social status of our *Ss* learn the cause-effect relation without special instruction during middle childhood, *i.e.* by the end of the eighth year or the beginning of the ninth.

(2) With special instruction, such as we gave our *Ss*, children of the age-group of 6·7-6·12 have a probability of 0.5 of learning the relation.

(3) The age-limen of the cause-effect relation is the highest of the relations investigated.

(4) The cause-effect relation, when learned, is acquired suddenly and thereafter the answers are in general given more quickly and confidently.

(5) The children who fail to learn the relation divide into two distinct groups: the complete failures, whose replies to all the test-words are incorrect; and the 'instable,' who fail only part of the time and whose replies are haphazardly correct and incorrect.

(6) The failures of the 'instable' *Ss* may be due not to a lack of comprehension of the relation but (1) to a lack of specific knowledge, (2) to a lack of vocabulary, (3) to the experimenter's lack of understanding of *S*'s meaning, and (4) to distraction by remote causes.

(7) The trial-and-error theories of learning are inadequate, being unable to account for the instances of insightful learning nor for the performance of the 'instable' *Ss*.

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[10] For a discussion of the inadequacy of the trial-and-error theories to children's learning of relations, see Kreezer and Dallenbach, *op. cit.*, 436 ff.

APPARATUS

THE TYPEWRITER AS AN AID IN ITEM ANALYSIS

By Solomon Diamond, New York, N.Y.

There are many psychological investigations, particularly those in which questionaries are used or rankings made, in which the data for each subject consists of a series of readily symbolized responses, which must not only be scored for the individual but also treated statistically item by item. The best aid for such analysis is of course a card-sorting machine, but this is a luxury which few can afford. I have made very good use of the typewriter to compile and analyze such data. The data is recorded in the speediest possible manner in the most compact form possible, and one which permits considerable economy of effort in analysis. Others, who had been using much more cumbersome procedures, have found the method which I shall describe helpful to them as well.

Data sheet. The record of every individual *S* is transferred to a data sheet as a single line of type, without skipping any spaces between items. The way in which this is done will be readily apparent from an illustration, which I take from a data sheet used in a previously published study. The record shows the age and sex of every *S*, with answers to a 48-item questionary, in which the *Ss* indicated that statements were right or wrong as applied to themselves, or that they were uncertain.

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18f rrwwwwrrwrrwwwwr?rrwwrwrrwwrrwwrrwrrwwrwwwwrrwww
20m wwwrrrrwwwwwwwwrrwrwrr?rrr??ww?wrrrrwr??wwwrw?rw
20f rrwrw?rrwwrw?wrr?wrwrr?r?rrrr?wrrrwr?www?wwrrr?w
```

It is clear that any data which are subject to statistical analysis can be presented in this way. Marital status might have been shown in an additional column, or by using small and capital letters to indicate sex. Educational status might be shown in a column in which a numeral would indicate an elementary school grade, and the small letters f, i, j, s, would indicate the freshman, intermediate (sophomore), junior and senior years of high school, while the same letters capitalized would indicate these years in college, and other letters could be introduced for graduate and professional training. Occupational level could be shown in another column, and use of small and capital letters in this column might indicate employment and unemployment.

When the data for a group of *Ss* has thus been placed on one sheet of paper, it presents a mass of symbols which is utter chaos when it is all exposed to the eye. While scoring, or doing statistical analysis, only one or two rows or columns should be exposed at a time.

Scoring key. The scoring key used will vary in design according to whether separate totals for odd and even items are wanted, whether the items are to be given different weights, etc. The key used in connection with the records illustrated was made as follows. On a sheet of paper, the series of correct answers was typed, for odd items only, properly spaced. Two lines below, the correct answers for even items were typed, to fall under the intervening spaces in the first row. The sheet was taken from the typewriter, and a slot cut in it, between these two rows of letters (see Fig. 1). This key was then placed on the data sheet, to expose one row of type at a time. (The blank spaces correspond to items which were not scored, because they lacked differentiating power.)

If items are weighted differently, each row on the key may give the weights for a certain type of answer.

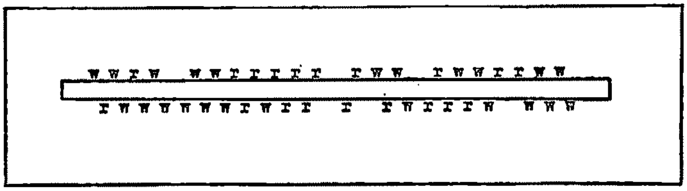
w w r w w w r r r r r r w w r w w r r w w
r w w w w w w r w r r r r w r r r w w w w

FIG. 1. SCORING KEY FOR TYPEWRITER ANALYSIS

Item analysis. For item analysis, columns are exposed one or two at a time. A device which assists in this is made from a celluloid strip 14/10 in. wide. (If élite type is used, it must be 14/12 in. wide.) Three slots, each 1/10 in. wide, and long enough to expose a full column, are cut in this strip. The first of these slots is cut 3/10 in. from one edge of the strip, the second is cut 2/10 in. from the first, and the third is cut 1/10 in. from the second, leaving 5/10 in. to the other edge of the strip. If the narrow strips of celluloid between the slots tend to twist or buckle, or if they have been broken in the cutting, the whole can be made quite strong by fastening some bands of transparent Scotch tape around the strip. With this device, one can expose any two columns which are separated by any number of intervening columns from one to eight. When the columns to be compared are farther apart than this, it is well to fold the sheet so as to bring them closer together, for ease in scanning. In this way, contingency tables may readily be constructed. (The one or two unused slots are covered by strips of heavy paper, whose ends are tucked into cuts made at the ends of the celluloid strip.)

Adaptations of this general method will suggest themselves to those working with slightly different types of data. In studying the relation of a vote on one question to a vote on another question, in a 10-item referendum, I found it expedient to record the votes on each ballot with a dot to represent 'yes,' and simply skipping a space for 'no.' The record of a few ballots appears as follows:

```
. . .         . . .
.   .     .       .
. . .     .   . . .
. . .   . . . . . .
  . . . . . . . . .
```

Again, let us assume that we are working with rankings made by a group of *S*s for 12 persons or things. To avoid the waste of time and effort involved in keeping columns in the usual way, we need one-place symbols for the numbers ten, eleven, and twelve. We decide that the zero shall represent 'ten,' and that "q" and "w," the first two letters in the bank of keys beneath the digits, shall represent 'eleven' and 'twelve.' The rankings made by several *S*s would then appear on our data sheet as follows:

```
4589037̇62wq1
029q746851w3
w541q6293078
6702813wq549
```

In treating the data, we expose one column at a time, and record the frequency of each ranking within it. The fact that letter-symbols have been used for some of them does not give rise to any inconvenience.

AN INEXPENSIVE, VARIABLE MULTIPLE-CHOICE MAZE

By ARTHUR BERMAN, University of California

Many mazes, adapted for human subjects, have been described in the psychological literature.[1] We add another to the long list because the maze we have devised is inexpensive and simple in construction and operation.

Our maze consists of a panel mounted on four uprights fitted together to make an inverted box. The panel, 12 x 12 in. square and 7/8 in. thick, is made of mahogany, hence holes may be drilled in it without splintering. The holes, 19 rows of 19 each (361 in all) are 1/8 in. in diam. and 3/8 in. apart (see Fig. 1). The panel is well sanded and shellacked so one hole cannot be distinguished from another; thus uncontrollable reaction-cues are eliminated. Beneath the panel, a metal plate is slid in slots like a drawer. Between the panel and this plate a reaction-pattern is placed. This pattern may be made of paper or of any non-conducting electrical material into which holes may easily be cut—we, however, used fiber board. The holes in this pattern may follow any design the experimenter wishes and as many different patterns may be made as desired.

At the center-head of the panel (see Fig. 1), a small 4–6 volt lamp is set which operates on three dry cells. One lead from the lamp goes to the positive pole of the battery and the other to the metal plate. A stylus, connected to the negative pole of the battery, is inserted in the panel-holes. When it touches the metal plate beneath, the electrical circuit is completed and the signal light flashes on. The flashes may be taken as indications of success or failure accordingly as the holes are cut in the reaction-pattern. A buzzer, an electrical shock, or other signal devices may quickly and easily be substituted for the electric light.

This maze is, as we believe, an improvement upon the earlier ones described. It has greater range of variability than any of them (361 holes and thus that many choices); it has no bolt heads to become scratched, and no complicated electrical circuits to wire; it is inexpensively made, and it is maximally variable as diagonal and rectilinear paths may be followed and new problem-patterns and goals quickly substituted. The last consideration is highly desirable in experiments which attempt to measure the comparative value of various rewards, the effect of an old goal on a new one, and the effect of changes in level of aspiration or satiation.

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[1] Shirley Morrison and T. W. Cook, An adjustable maze for visual and non-visual learning, this JOURNAL, 46, 1934, 141; R. G. Whisler, A universal flexible elevated finger maze, *J. Gen. Psychol.*, 10, 1934, 482-484; J. Buel, A simple, inexpensive finger maze for experimental and classroom use, *ibid.*, 17, 1937, 192; J. R. Knott and W. R. Miles, Notes on the history and contribution of the stylus maze, *Ped. Sem.*, 35, 1928, 415-427; Herbert Gurnee, A portable bolthead maze, this JOURNAL, 51, 1938, 405-406.

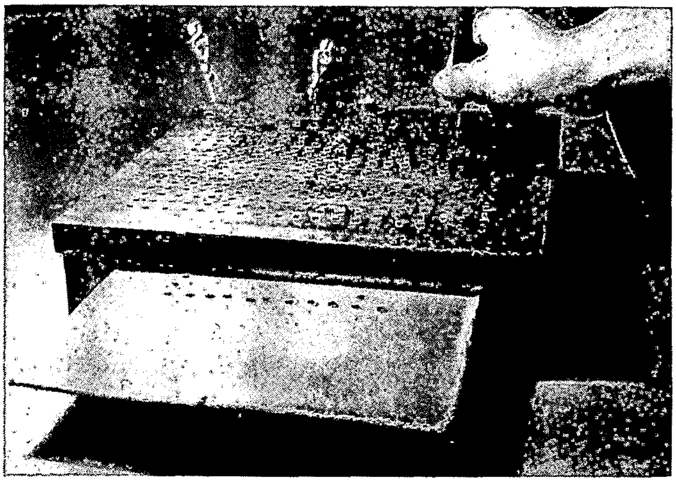

FIG. 1. THE VARIABLE MULTIPLE-CHOICE MAZE

The small cost and facility with which this maze may be built makes it an attractive apparatus where experimentation in small groups is desired. Throughout a period of three months it has been given a rigorous test in the laboratory of the University of Southern California.

We have found it desirable to use substances for the reaction-pattern as similar to the metal plate as possible. The subject is then unaware from secondary cues when he strikes the pattern or the metal plate. This, however, is of no great importance since the cue is not available until the stylus is inserted and is thus not retroactive. Once the stylus is inserted in a hole, it is counted a trial, and what follows is in this instance not of great significance.

In the final analysis, the chief advantage of the present maze lies in its inexpensiveness and great adaptibility.

A DEVICE FOR OBTAINING CONSTANT OLFACTORY STIMULATION

By A. L. WINSOR and B. KORCHIN, Cornell University

In our studies on the parotid secretory accompaniments of olfactory adaptation we found it necessary to devise some means of obtaining a stimulation which remained constant in intensity throughout the experimental period. For this purpose the apparatus described here served very satisfactorily. It can be constructed with relative ease and little expense.

An oval shaped opening (5 mm. x 20 mm.) is blown out of the wall of a piece of glass tubing (8 mm. bore; 15 cm. length). To prevent the liquids that are to pass through the tube from overflowing the edges of the opening are raised somewhat above the level of the rest of the tube. If it is desired to mix the stimulating vapors with air or other vapors, two small tubes can be attached by adhesive tape in the manner illustrated in Fig. 1. Fig. 1 gives in outline a diagram of the apparatus before the dental wax is molded around it. Two or three cakes of dental impression compound are sufficient to cover the tubes and be molded into the shape of the nose-piece shown in Fig. 2. By applying a heated cloth to the upper portion of the nose-piece it can be easily softened and molded to fit snugly around the *S*'s nose. The dental impression compound is a thermoplastic wax which is rigid at body temperature, but which becomes soft and pliable at about 40°C.[1] The wax should be heated in a waterbath. The nose-piece is clamped to a stand and adjusted to the convenience of *S*, whose head is held in place by means of a headrest.

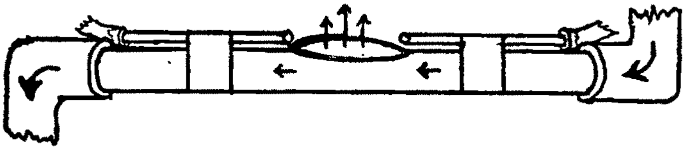

FIG. 1. DIAGRAM OF OLFACTORY STIMULATOR

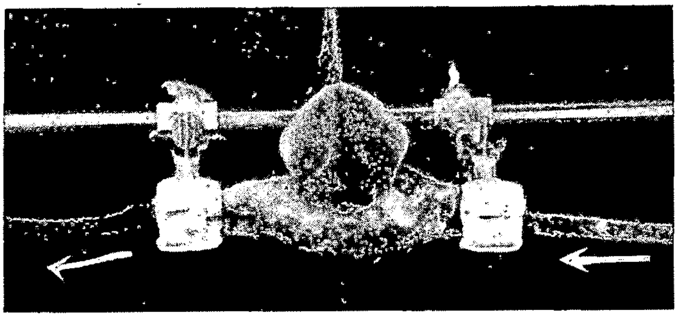

FIG. 2. THE OLFACTORY STIMULATOR
(Nasal shield in place.)

The liquids that are to flow through the tube are kept in bottles on a shelf above *S*'s head. The rate of flow can be adjusted by means of stop-cocks. Rate of flow can also be kept constant by means of Mariotte's bottles or other mechanical means.

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[1] For a similar use of the dental impression compound see H. Abrahams, D. Krakauer, and K. M. Dallenbach, Gustatory adaptation to salt, this JOURNAL, 49, 1937, 463.

THE MANNING PNEUMOGRAPH

By David Kirby Spelt, University of Mississippi

A simple pneumograph has been recently devised by Dr. I. H. Manning, Professor of Physiology, Emeritus, in the Medical School of the University of North Carolina. It is especially valuable because it is inexpensive to make and can be used satisfactorily with subjects of any size from rabbits up to human beings, without changes in dimensions.

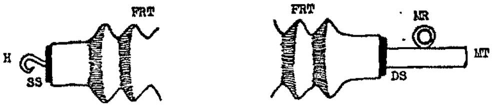

Fig. 1. The Manning Pneumograph

FRT = Flexible rubber tube
SS = Solid rubber stopper
DS = Drilled rubber stopper
MR = Metal ring
MT = Metal tube
H = Hook

It consists essentially of a flexible rubber tube, approximately 11 in. long and ⅞ in. in diameter.[1] Deeply corrugated transversely, it stretches longitudinally. One end is completely closed by a solid rubber stopper into which a small hook is securely fastened. In the other is a drilled rubber stopper, from which projects a piece of metal tubing of any convenient bore. The rubber stoppers are held in place by friction. A length of cloth tape or of chain is fastened permanently to a small ring soldered to the metal tubing, not too close to the free end. In use, this passes around the subject's back to the hook at the other end of the pneumograph, and a rubber hose, slipped over the metal tube, connects with a recording device.

The instrument is adequate both for research purposes and for general laboratory work. Because the rubber is not under tension its life is long if it is kept away from heat and strong light when not in use.

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[1] This tube can be purchased from Phipps & Bird, Inc., 915 E. Cary St., Richmond, Va.

NOTES AND DISCUSSIONS

BINOCULAR FUSION AND THE LOCUS OF 'YELLOW'

In a note (A binocular color phenomenon, this JOURNAL, 51, 1938, 769 f.), Dr. E. G. Bugg reported the observation of a violet after-image following the use of Hecht's Binocular Color Mixer to secure a 'fusion' yellow from monocular red and green. This violet after-image he assumes represents also a fusion product on a higher brain level. Since Dr. Bugg's discussion conveys an impression of the derivative character of 'yellow' and its cerebral rather than retinal locus, it seems desirable to amplify an earlier criticism of the technique employed[1] and the fallacies involved.[2] Binocular fusion or summation may be readily enough conceded, superseding in certain cases retinal rivalry. When, however, such fusion is invoked as generating so unique a quality as yellow, from two other as unique or primary ones—in this case red and green—caution demands a careful scrutiny of the conditions.

In 1928, Hecht (whose method was followed in the above study), after reporting the difficulties and hesitations of his predecessors of the last quarter-century in the field of binocular fusion, cannily sidestepped the pitfalls of earlier observers. First, as in many other Helmholtzian or tri-stimulus observations, the dice were weighted by passing over the standard 'red' and 'green' filters of the Wratten 'monochromat' series, and picking instead two wide-band yellow-green and yellowish red ones (no. 58 and 29), designed primarily for photography.[3] Secondly, a light source not of the low wattage for which the filters were designed but of 100 to 250 watts was chosen. Thirdly, the set-up was so arranged that the lens focus favors 'yellow' radiations, and the fusion area falls within the yellow-enhancing region of the retina, the macula lutea. Finally, as often happens in physical experiments, the rapidity of color adaptation or fatigue, especially for weak reds and greens, and the consequent need of brief exposures, was ignored, and no time limit set on an observation.

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[1] S. Hecht, On the binocular fusion of colors and its relation to theories of color vision, *Proc. Nat. Acad. Sci.*, 14, 1928, 237-241. For criticism of the same, see E. Murray, Color problems: the divergent outlook of physicist and psychologist, this JOURNAL, 32, 1930, 124, footnote 20.

[2] This critique is based upon experimental observations made in the Cornell Psychological Laboratory, in a special optical unit outfitted in part by a grant-in-aid of 1930-1931 from the National Research Council secured through the coöperation of Professor Madison Bentley.

[3] The three wave lengths of the I.C.I. tri-stimulus colorimetry tables of 1931, according to D. B. Judd of the Bureau of Standards, are 700, 546, and 435.8 mμ. In a spectrum of average intensity, these three stimuli yield a deep red verging on crimson, a yellow-green, and violet. Helmholtzian experimenters have a disconcerting way, however, of shifting the 'red' stimulus (700) about in the long-wave half of the spectrum, 'for convenience's sake.' Von Kries even pushed it in as far as 589 mμ, in order to line up his curves for dichromats satisfactorily; while Hecht uses 650 or 570 indifferently for the same purpose, substituting also 458.7 for 440 as the occasion demands.

As for the mid-spectrum stimulus (546), this is centered far from the 505 strip which is achromatic in red-green blinds, and which, owing to its invariability of hue with altered intensities, tends today to be picked by all theorists as the best excitant for the 'green' receptors. As a matter-of-fact, the mid-spectrum region of 525 to 545 mμ, favored by old-school Helmholtzians in complete disregard of peripheral and color-blindness data, is so yellow in tone that in 1922 Dunlap proposed designating it as *chlor* rather than green.

There is apparently a vague assumption that the optimal wave length may fail to yield a high-chroma primary owing to the overlapping of the curves of the other two primaries. Yet as early as 1892, König and Dieterici shifted their *Grundempfindungen* to purplish red, 505 and 470 mμ, to fit the mixture data of color-blind.

Checking over this set-up a few years ago, with a view to repeating Hecht's observations on a mixed group, the writer speedily reached the conclusion that no mystical fusion of red and green excitations in the higher centers above the chiasma need be invoked, to account for the binocular yellow square.[4] The summation of the yellow valences of the two monocular stimuli (mediated in all likelihood by 'yellow' receptors in the retinae), plus the selective action of macular pigment on the yellow component and its reënforcement by lens focus and other factors, would suffice to mask or swamp the red and green components. The latter are already weakened by the high intensity of the light source chosen; for red and green hues are notoriously keyed with low intensity mechanisms which pass out of function with increased intensities, these hues shifting into yellow or blue (notably in the bright light spectrum).[5] Further, both the red and the green components tend to adapt out quickly when there is no control of exposure-times.

Since, therefore, all the conditions point to enhancement of a yellow tone, excitations for which are probably already present monocularly, binocular summation appears to account fully for the phenomenon of the central yellow field, dispensing with the need of positing a brain locus for the creation of the quality yellow.

This solution automatically resolves the puzzle of the locus of Dr. Bugg's violet after-image. The latter becomes merely the summation of the two monocular complementary after-images of the yellow components of the yellow-red and yellow-green squares, of presumably retinal locus. The complementary after-images of the red and green components are too weak to count. Incidentally, the fact that the preceding sensory fusion is not yellow but yellow-green is indicated by the report of a *violet* rather than an indigo-blue after-image.

The status of yellow[6] as a distinctive sensory datum or quality, with photo-receptors in the retina in its own right (or in conjunction with its complementary blue), and its own unique excitation process in the neural tissues of the eye, is thus hardly affected either by Hecht's so-called demonstration of binocular color fusion, or by Bugg's repetition and extension of Hecht's observations into the domain of the after-image.

The first choice in 1801 of Thomas Young, originator of the tri-chromatic theory, for his mid-spectrum primary,[7] yellow is slowly but surely regaining its original lead in three- no less than in four-component theorizing. In view of the steadily accumulating evidence of the prevalence of this quality in the peripheral field, in the visual world of the color-blind, and in the bright-light spectrum this is not surprising.

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[4] The simulation of a new or unique quality by excitations fused above the receptor level is of course in itself no novelty in sense psychology. In 1907-1909 the writer used this principle to effect an economy in the diversity of receptors necessary to explain tactual and organic or somaesthetic perceptions. (Cf. this JOURNAL, 19, 1908, 289-344, esp. 337 ff.; and 20, 1909, 386-446, esp. 441 ff.) In the olfactory field also, some such synthetic principle plays apparently a significant and often baffling rôle.

[5] This hue shift may readily be demonstrated on the spectrometer by isolating a band centering as does Filter 58 on 545 mμ. Though 545 mμ appears yellowish green at a voltage of 60, at 105 it becomes greenish yellow. Filter 29 (orange) centers on 610-620 mμ, a band which in the spectrometer at 100 v. has its yellow tone heightened, its reddish component weakened.

[6] *I.e.* the hue located in the spectrum in the neighborhood of the sodium lines.

[7] Later abandoned for green (yellowish), through a misunderstanding of Wol-

Nor does the claim to sensory primacy of this hue need to lean at all on the antiquated doctrine of 'purity' or unanalyzability, and freedom from a discernible red or green component so vehemently reiterated by Ladd-Franklin in chorus with certain of the older Hering adherents.[8] To unprejudiced perception, the same criterion of uniqueness or singleness may be applied to any one of the 150 or more hue strips of the spectrum or continuum—violet, orange, purple—when viewed in isolation, as Hering himself frankly acknowledged.

The claim of yellow to genetic primacy,[9] in the opinion of experienced laboratory workers, familiar with the functioning of both normal and deficient color systems, rests on other grounds. The preëmption of the field of vision of the normal daylight eye by yellow, photo-receptors for which are distributed far out toward the margin of the retina,[10] the wide span within the spectrum of stimulus frequencies adequate to evoke it (5000-8000 ångstroms and beyond); its dominance in the color world of the commoner types of chromatic deficiency—the so-called red-green blind who match the bulk of their color impressions with varying saturations and shades of yellow, *i.e.* a hue whose most effective stimulus is located in the strip of wave-lengths just below sodium yellow in the spectrum, 590 to 570 mμ; the stability of the yellow mechanism under intensive stimulation or disease, all support its claims.[11] It seems indeed highly probable that yellow may represent the oldest visual response, possibly antedating white; occupying originally, with its comple-

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laston's results and their bearing. See R. A. Houstoun, *Vision and Color Vision,* 1932, 184.

[8] Cf. D. Katzin and E. Murray, Instability in color-naming in normal and anomalous observers, this JOURNAL, 46, 1934, 611.

[9] The popularization of this idea, borrowed from Donders, constitutes Ladd-Franklin's chief claim to rank among color theorists. Her elaboration of the genetic standpoint, in attempted reconciliation of Hering and Helmholtzian tenets, has never worked smoothly in detail as an explanatory principle. Black, color contrast and after-image, scoterythrous color-blindness, are among the phenomena left unaccounted for in her schematic hypotheses. Though accepted uncritically at face value by many psychologists, the whole structure falls to the ground with her shift of the red and green primaries (associated with the R and G molecules the evolution of which from the more primitive Y molecule she posits) from yellow-green and yellowish red to verdigris and crimson. The stimulus pair that yield the sickly mixture 'yellow' of the Helmholtzians are simply not the pair yoked in the complementary phenomena of color vision. See E. Murray's review of Ladd-Franklin's *Colour and Colour Theories,* this JOURNAL, 41, 1929, 653-656.

[10] It seems highly probable that the ambiguous responses to high intensity 'red' stimuli reported occasionally in the periphery, but apparently unguarded from a 'set' for red, are actually responses of these receptors, and liminal *yellow* rather than red in hue. Similarly the response to radiations between 800 and 856 mμ, in the so-called infra-red, appears to be yellowish rather than red in tone, even when stray light is excluded by filters.

[11] The occurrence of 'yellow' where primary 'red' is in abeyance or missing, as in the peripheral field or in red-green blindness, can be explained by the trichromatist only by complicated secondary mechanisms operative on the cerebral level. The different neural levels of the retina, along with slight structural variations in the cones, apparently suffice, however, for tetrachromatic theory—which, moreover, was never linked unalterably by its originator with *three Sehsubstanze.*

mentary blue, the entire span of the spectrum.[12] Certain groups of facts suggest that only as the eye developed protective pigments, or altered climatic conditions reduced light intensities did special red and green response-mechanisms (never very stably established) arise, cutting in on the curves for yellow and blue sensitivity, especially under moderate or reduced illumination.[13]

Luckily there are today many indications of a saner attitude toward color facts and color theory, a tendency to accept the eye as it is, rather than as it ought to be in the interests of a fancied 'economy.' If this permits the assembling of data unwarped by *a priori* mathematical formulas, it should bring both a better basis for the interpretation of color-blindness and its genetic correlates, and a better foundation for the integration of neural theory and the sense of vision.[14]

The recognition that every hue as experienced requires tri-dimensional definition, that it varies in depth of color or chroma as well as in intensity or brightness, signalized in Troland's Report of the Colorimetry Committee in 1922, could but have wide repercussions. Practically demonstrated in Munsell's widely accepted color system and color handbooks, it is now in use in the revision of standard color terms under way for the U. S. Pharmacopaeia at the hands of Inter-Society Color Council and Bureau of Standard experts. Not only has the confusion of saturation or chroma with intensity or 'luminosity' by physicists long muddled the use of the color triangle, and obscured the fact that dichromats ordinarily distinguish red and green as different *saturations* rather than brightnesses of yellow; it has also led to slurring over the important point that the mixture yellow of the Helmholtzians is far from rivalling in chromaticity the hue produced directly from wave-lengths 570-590. Recognition of this attribute should break down the stubborn insistence on the color-blend character of yellow,[15] and lead to the abandonment of the use of the Rayleigh equation and the Nagel anomaloscope as measures of the red or green sensitivity of an observer. A direct measure of the thresholds for red and green should give less ambiguous results. Variation in the yellow valence of either stimulus component is sufficient to obscure variation in red or green responsiveness *per se*.

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[12] The fact that the most effective stimuli for yellow are located precisely within the strip of radiations of greatest photo-effectiveness for achromatic response in the daylight eye is not without significance here. The statement that the most effective achromatic wave length lies somewhere near 545 mμ is based apparently upon scotopic vision. The brightest strip for Hecht's two deuteranopes lay near 570 for the photopic eye.

[13] This difference in the intensity levels of the two sets of thresholds may have inspired certain recent attempts to frame a 'quantitative theory' of color; by theorizers, however, too little familiar with the complexities of color phenomena to coördinate fact and hypothesis.

[14] Realization by progressive physicists that their color-mixing curves and formulæ, however ideally simple and compact, are not adequate to the physiological properties of the eye; and further, that the terms red-blind and green-blind are utterly misleading, since a blue-yellow spectrum linked with red-green deficiency characterizes both the commoner types, has lodged many of them in a dilemma. Like the scientific-minded schoolmen of the Middle Ages, forced to reconcile the contradictory tenets of science and theology, they appear to have taken refuge in the doctrine of the *Twofold Truth; i.e.* what is acceptable in color-mixing formulae is not necessarily theoretically true. If, however, they decide to retain Young's somewhat casually set up tri-receptor doctrine, they must assume in color-blindness not a reversion to a simpler type of vision but Fick's complicated perversion of the red and green receptors, greatly to the damage of the vaunted simplicity and economy of the trichromatic theory.

[15] Cf. Houstoun's position on this point, *op. cit.,* 224 f.

The significance of the outspoken abandonment of the three-color theory as mathematically unsound by so distinguished a physicist and authority on light as Houstoun should go far to turn speculation into more productive channels.[16] Economy of operation with three ordinates or stimuli, the lack of satisfactory dyes to produce a clear yellow glass or filter, the psychologist's usual poverty of physical resources and laboratory apparatus have long retarded the observations necessary to establish yellow as a primary. With the extended use of color in industry, however, has come the insistent demand for a more adequate basis for colorimetric technique and matching devices. Already the spectral curve for a given color sample turned out by the new spectrophotometers has been found to afford a more satisfactory specification for reproduction than the older tri-stimulus formulas. But interpretation of the significance of these curves for the eye demands an understanding of the latter, and its sensitivity curves for the various wave lengths. It demands a knowledge also of complementary colors and the fashion in which when reflected from a given surface they cancel out; of the conditions favorable to high saturations for each hue—for chroma is not a function of purity only as many physicists now assume—nor are the conditions favorable identical from hue to hue.

The *Eichung* of the spectrum in tetrachromatic terms is at present the urgent need, in both the theoretical and the practical fields. The requisite apparatus—two spectroscopes or spectrophotometers and accessories—is rarely available to a psychologist convinced of the need for matching each strip of the color circle with the proper mixture of the tetrachromatic primaries, Y, R, B, and G (the latter chosen near 505 mμ, as König, checking his mixture ordinates with dichromats, admitted as early as 1892). It is possible these curves might serve both as mixture or colorimetry curves, and also as indicators of the points of maximal sensitivity on the spectrum wave length abscissa for the four primaries. Such curves formed for twenty years the preoccupation of a Swedish scientist, Gösta Grönberg, but his untimely death in 1935 left us with only a meager and schematic account of his data and theory.[17]

As long as psychology delays equipping its personnel with the necessary exact knowledge of the intricacies of color phenomena and of the handling and control of light stimuli, and postpones attacking the problem of the quantitative testing and expression of the tetrachromatic theory, both accurate color observations in color-blindness and the development of adequate neural theories will be retarded. We shall be at the mercy of every distortion and elaboration of trichromatic theory (in defiance of the original argument of economy), every attempt such as the above to derive yellow from the operation of two distinct mechanisms.

Cornell University ELSIE MURRAY

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[16] Houstoun, *op. cit.*, 223 ff. For review of the same see E. Murray, this JOURNAL, 46, 1934, 346-348.

[17] En ny fargteori, *Svenska Läkar. Föhandl.*, 20, 1923, 577-582. Grönberg posited four different types of cones, for R, G, Y and B, differentiated for the corresponding wave lengths of light by slight variations in their lamellar or spiral structure, the pitch of which had been found by Hesse to be of the order of magnitude of a wave length of light. Assuming secondary maxima, or resonance to wave lengths standing in simple ratios as 3 to 2 or 4 to 5, Grönberg claimed his theory explained not only the reëmergence of red in the violet, and lavender gray at the end of the spectrum, but also the unexpected brightness of certain mixtures, and the Bidwell and Benham rings.

ON THE FALLACY OF IMPUTING THE CORRELATIONS FOUND FOR GROUPS TO THE INDIVIDUALS OR SMALLER GROUPS COMPOSING THEM

If the correlation between two traits, A and B (say, poverty and delinquency), in *n* groups (say, the residents of *w* districts) has a certain value, K, the correlation between A and B in the individuals or the families composing the groups need not be K and will not be, save in very special circumstances. It will usually be very much nearer zero. This is easily demonstrable, yet some able teachers have been guilty of thinking that the two correlations will be closely similar, or of leaving their students relatively unprotected against thinking so. And it is to be feared that many readers with little knowledge of correlation often misapply correlations between features of states, counties, cities, wards, classes, etc. to their constituent units.

Consider, for example, the following statements by Cyril Burt, an expert in psychology and education.[1] They are not downright guilty of the fallacy, but just what does he wish his readers to think? Can he justly complain if people quote him as asserting that certain social conditions correlate 0.60 or 0.70 with juvenile crime?

> "Table V [not shown here] shows the percentages of juvenile delinquency in each of the twenty-nine Metropolitan boroughs, treating for these purposes the City of London as itself an additional borough. Side by side, for every area I have placed such accessible statistics as might be held to reflect light on its social character. These in the main relate to features I have already commented upon in passing—the birth rate, the death rate, the amount of overcrowding, the amount of open space, the proportion of illegitimate children, the proportion of persons in receipt of relief, the percentage of the child population scheduled for purposes of public elementary education, and finally, the figures for poverty calculated by Charles Booth.
>
> "The correlations between the figures for juvenile delinquency and these several statistical assessments are . . . sufficient to show that the connection of childish offenses with poverty and its concomitants is significantly high, though not so high perhaps as previous inquirers, trusting mainly to experience and general impression, have as a rule implied. With the amount of poverty in each borough, as assessed by Charles Booth's survey, the amount of juvenile delinquency is correlated to the extent of 0.67, with the relative amount of poor relief to the extent of nearly 0.50; with the number of children scheduled for school purposes (probably the best single index of the present social character of each borough as a whole) to the extent of 0.63. The highest coefficient of all is that for the correlation between juvenile delinquency and overcrowding, namely 0.77. The correlation with the death rate (0.70) is nearly as high. Allowing for the gross shortcomings, inevitable in estimates so crude, so vague, and in some cases so largely out of date, these several figures are remarkably consistent one with another. They indicate plainly that it is in the poor, overcrowded, insanitary households, where families are huge, where the children are dependent solely on the State for their education and where the parents are largely dependent on charity and relief for their own maintenance, that juvenile delinquency is most rife."[2]

There is every reason to believe that the correlation between the overcrowding *of each family* and its proportion of delinquents per child would be much below 0.77; that the correlation between the poverty of each family and its proportion of delinquents per child would be much below 0.67; and similarly for the other correlations. Burt's statement elsewhere that 19% of his juvenile delinquents came from

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[1] If the fallacy was encouraged only by incompetent scientists, a note about it in this JOURNAL would not be needed.

[2] Cyril Burt, *The Young Delinquent*, 1935, 72-75.

TABLES I–XII

THE CORRELATION (ZERO) BETWEEN INTELLIGENCE QUOTIENT AND NUMBER OF ROOMS PER PERSON FOR EACH OF TWELVE DISTRICTS

The captions at the top of the Tables, −4, −3, −2, −1, 0, 1, 2, 3, 4 and 5, represent intelligence quotients of 64 to 71, 72 to 79, 80 to 87, etc. The stubs at the left of the Tables, −4, −3, −2, −1, 0, 1, 2, 3, 4 and 5, represent 0.20 to 0.39 rooms per person, 0.40 to 0.59 rooms per person, 0.60 to 0.79 rooms per person, etc.

Table I

	−4	−3	−2	−1	0	1	2	3	4
−4									
−3				1	1	1			
−2		1	1	4	6	4	1	1	
−1	1	2	4	7	15	7	4	2	1
0	2	2	6	8	20	8	6	2	2
1	1	2	4	7	15	7	4	2	1
2		1	1	4	6	4	1	1	
3				1	1	1			

Table II

	−4	−3	−2	−1	0	1	2
−4							
−3			1	1	1	1	
−2		1	2	4	4	2	1
−1		1	3	6	6	3	1
0		1	2	4	4	2	1
1			1	1	1	1	
2							
3							

Table III

	−4	−3	−2	−1	0	1	2	3	4
−4									
−3									
−2				1	1	1	1		
−1		1	1	2	5	5	2	1	
0		1	3	5	10	10	5	3	2
1		1	3	5	10	10	5	3	2
2		1	1	2	5	5	2	1	
3				1	1	1	1		

Table IV

	−4	−3	−2	−1	0	1	2
−4							
−3		1	1	1	1		
−2	1	2	4	4	2	1	
−1	1	3	6	6	3	1	
0	1	2	4	4	2	1	
1		1	1	1	1		

Table V

	−4	−3	−2	−1	0	1	2	3	4
−4		1	1	1					
−3	1	2	4	2	1				
−2	1	3	6	3	1				
−1	1	2	4	2	1				
0		1	1	1					

Table VI

	−4	−3	−2	−1	0	1	2
−4			1	1	1		
−3		1	2	4	2	1	
−2		1	3	6	3	1	
−1		1	2	4	2	1	
0			1	1	1		

Table VII

	−4	−3	−2	−1	0	1	2
−4							
−3			1	1	1		
−2		1	2	4	2	1	
−1		1	3	6	3	1	
0		1	2	4	2	1	
1			1	1	1		

Table VIII

	−3	−2	−1	0	1	2	3
−4			1	1	1		
−3		1	2	4	2	1	
−2		1	3	6	3	1	
−1		1	2	4	2	1	
0			1	1	1		

Table IX

	−4	−3	−2	−1	0	1	2
−4							
−3							
−2				1	1	1	
−1			1	2	4	2	1
−0			1	3	6	3	1
1			1	2	4	2	1
2				1	1	1	

Table X

	−3	−2	−1	0	1	2	3
−4							
−3							
−2							
−1				1	1	1	
0			1	2	4	2	1
1			1	3	6	3	1
2			1	2	4	2	1
3				1	1	1	

Table XI

	−4	−3	−2	−1	0	1	2	3	4
−1									
0						1	1	1	
1					1	2	4	2	1
2					1	3	6	3	1
3					1	2	4	2	1
4						1	1	1	
5									

Table XII

	−3	−2	−1	0	1	2	3	4	5
−1									
0									
1						1	1	1	
2					1	2	4	2	1
3					1	3	6	3	1
4					1	2	4	2	1
5						1	1	1	

homes of extreme poverty (classes A and B) and 53% from classes A, B, C and D, whereas only 8% of the general population are from classes A and B and only 30% from classes A, B, C and D, would give a correlation between delinquency and poverty not of 0.67, but of about 0.18.

I append an artificial illustration of the general fact, which will be of service especially in making the matter clear to students. A is supposed to be intelligence quotient and B is supposed to be the fraction of a room or number of rooms per person.

Table XIII

COMBINATION OF ALL THE PERSONS IN TABLES I-XII

	−4	−3	−2	−1	0	1	2	3	4	5
−4		1	2	3	2	1				
−3	1	4	10	12	11	5	1			
−2	2	8	19	30	26	14	4	1		
−1	3	11	25	38	44	23	9	3	1	
0	3	7	21	33	50	29	15	6	4	
1	1	3	12	20	39	26	16	8	5	
2		2	3	9	17	16	12	9	3	1
3				3	4	6	8	8	4	1
4						2	3	5	2	1
5							1	1	1	

Table XIV

AVERAGES OF THE TWELVE DISTRICTS

	−3	−2½	−2	−1½	−1	−½	0	½	1	1½	2	2½	3
−3													
−2½													
−2			1		1		1						
−1½													
−1				1	1	1							
−½													
0							2						
½								1					
1									1				
1½													
2											1		
2½													
3													1

Let the scores for sample persons in each of twelve districts into which a city is divided be as shown in Tables I-XII. Within each of the districts the correlation between A and B is zero. If all the persons in the sample are combined, the result is Table XIII, and the correlation is 0.45. If the averages for the twelve districts are used, the result is Table XIV, and the correlation is 0.90.

Columbia University E. L. THORNDIKE

The Stepwise Phenomenon

If an animal be trained to approach a gray of medium brightness (*M*) in preference to one of less brightness (*L*), it will tend, upon the substitution for *L* of a gray (*H*) of a brightness still greater than *M*, to approach *H* more frequently than *M*. This has been designated the 'stepwise phenomenon' by members of the Gestalt school, and it has been explained as the animal's response to 'relative' rather than to 'absolute' brightness of the grays (cf. Koffka, and Köhler).[1] Although the experimental results have in the main confirmed the facts outlined, four aspects require further consideration.

In the first place, in the substitution-series not all the test-runs are approaches to gray *H*. The writer has shown elsewhere that one may expect a range of relative approaches which extends from less than 50% to 95%.[2] The results reported by other investigators seem to confirm these figures.[3] May it be said that the animals responding 'relatively' 95% of the time are any more typical than those so responding only half the time? If not, any adequate explanation should fit equally well for both cases.

In the second place, there are now at least two negative bits of evidence to consider. Taylor has shown that when *L* is black, and *H* white, with *M* a medium gray, 'asbolute' and not 'relative' approaches are made by chickens in the substitution series.[4] Recently Ohtsuka has demonstrated that for monkeys a relatively great separation of the brightnesses of the grays will lead to 'absolute' approaches, whereas grays closer together in brightness will lead to 'relative' approaches.[5] Thus the relative intensities of the stimuli are of importance in determining what the animal will do in the substitution series.

The third aspect concerns the effects of 'overtraining' or experience in the situation after *S* has reached the criterion of learning. The writer failed to find any significant shift in the performance of rats when overtrained 100 trials beyond 'learning.'[6] Jackson and his colleagues,[7] using children as *S*s and different sizes of wooden forms, rather than grays, as the variables, have, however, found such a shift. In their experiment, overtraining resulted in an increase in the number of 'absolute' choices. Although the groups were small and later results may thus fail to confirm these findings, it seems not unlikely that the amount of experience in the training situation has an effect upon what the organism does subsequently in the substitution series.

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[1] K. Koffka, Perception: An introduction to *Gestalt-Theorie, Psychol. Bull.*, 19, 1922, 531-585; W. Köhler, Nachweis einfacher Struktur-funktionen beim Schimpansen und beim Haushuhn, *Abh. d. königl. Preuss. Akad. d. Wissensch., Phys.-Math. Klasse*, 1918, Nr. 2.

[2] The 'stepwise phenomenon' in rats, *J. Comp. Psychol.* (in press).

[3] A. H. Gayton, the discrimination of relative and absolute stimuli by albino rats, *ibid.*, 7, 1927, 93-105; C. J. Warden and B. J. Rowley, The Discrimination of absolute versus relative brightness in the ring dove, Turtur risorius, *ibid.*, 9, 1929, 317-337; D. O. Hebb, The innate organization of visual activity: II. Transfer of response in the discrimination of brightness and size by rats reared in total darkness, *ibid.*, 24, 1937, 277-299.

[4] H. Taylor, A study of configuration learning, *ibid.*, 13, 1932, 19-26.

[5] N. Ohtsuka, Über die absolute und relative Wahl beim Affen *Cercopithecus sp.*, *Acta. psychol. Keijo*, 3, 1937, 33-44 (Abstract seen).

[6] *Op. cit.*

[7] T. A. Jackson, E. Stonex, E. Lane, and K. Dominguez, Studies in the transposition of learning by children: I. Relative vs. absolute response as a function of amount of training, *J. Exper. Psychol.*, 23, 1938, 578-600.

In the last place, Jackson and his associates have also shown that the reaction-time of *S* is greater in the substitution than in the training trials.[8] It is to be regretted that more experimenters have not obtained measures of the reaction-times of their *Ss*, since such data are indicative of whether or not the two situations are perceptually identical. That is, if *S* does not perceive any change in the situation, we would expect his reaction-time to remain constant. On the basis of the results cited here, it would seem that the training and transfer stimuli are, at least under these conditions, perceived as different.

It would seem obvious that any explanation of the performance of the animals or children merely on the basis of an essentially identical perceptual structure in the training and substitution-situations is impossible. The Gestalt explanation fails to account for the wide individual differences among the *Ss*, the dependence of performance upon the relative differences among the stimuli, the effect of overtraining, or the increased reaction-times in the substitution-series. Jackson and his collaborators have proposed an explanation based upon the concept of 'generalization' of the 'correctness' or 'incorrectness' of a given stimulus.[9] Although this explanation is appealing because of its statement in mathematical form it is difficult to conceive of any process going on within an organism which would correspond to the theoretical curves.

In the consideration of a possible explanation for the behavior as outlined, the arguments of Gundlach and Herrington deserve attention.[10] Discrimination of the relative brightness of two grays is of course easier when the stimuli are presented simultaneously (cf. Bentley).[11] That is, the just noticeable difference for brightness attains its minimum value in the case of simultaneous stimulation, where a direct comparison is possible. If an *S* be required to discriminate between grays which are presented, not simultaneously, but a day or so apart, his difference limen will of necessity rise. The latter situation is, in effect, the one presented to the animals in the 'transfer' or *M-H* situation. Especially when we remember that we have no way of instructing the animals to look out for changes in the brightnesses of the grays it is not surprising that the change, when small, is overlooked. Indeed, we might well expect less than absolute accuracy even in the presence of suitable instruction, in the case of grays close together in brightness. The above discussion will apply equally as well to the experiments using children as *Ss*, since instructions of the sort mentioned must be avoided for fear of biasing the results. The discussion is also germane in the case of stimuli differing in some property other than brightness, as, for example, size. We may logically expect, then, that the greater the differences among *L*, *M*, and *H*, the more 'absolute' approaches will be made.

Since formal instruction is impossible in the animal experiment, the situation should be arranged so as to include the items to be 'learned' and the factors necessary to insure that the animal responds in terms of the problem set by *E*. An animal 'learning' experiment, then, is not strictly analogous to the human association of nonsense syllables, or other forms of rote learning. The consequence of this consideration is that the animal is performing in *its* psychological environment, not in that of the experimenter. Hence at least the early stages of the animal's experience in the situation must be given over to the discovery of just what variables in the environment will serve as reliable indicators or cues of things to come. Among the various ways of making it probable that the animal is responding in terms of those portions of the situation which *E* considers critical are 'tuning' (as by deprivations of food) and strict limitation of the possible cues. In the type of situation under consideration (*i.e.* differential response to brightnesses), the animal is 'tuned' to approach food, and the only reliable cues to the position of the food are the doors or papers of varying brightness. The animal ultimately comes to approach one brightness instead of the other. The cues utilized by the animal may be of two kinds. First, the 'correct' brightness may be selected by a comparison of one brightness with the other. Secondly, a given brightness may be perceived directly as 'correct' or 'incorrect' by the relation of the given brightness to its background. It may be that when the grays are relatively close together in brightness the animal is forced to the comparison, due to the difficulty of discrimination for stimuli not simultaneously present, as indicated earlier in this note. The situation is analogous to that involved in relative as against absolute judgment of pitch. If in this latter case, the *M-H* situation replaces the *L-M*, the change will be overlooked, as previously suggested, and what appears to be a 'relative' approach will occur. That is, the performance of a given animal in the transfer series will depend upon the manner in which it has come to perceive the situation. With any given set of brightnesses, individual differences will arise due to (1) the threshold of the given animal, and (2) the 'set' or 'hypothesis' which the animal brings to the situation (cf. Krechevsky).[12] In the case of the higher mammals 'set' is of extreme importance, and it is probably a shift in this factor which is at the basis of the results obtained by Jackson.

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[8] *Op. cit.*

[9] *Op. cit.*

[10] R. H. Gundlach and G. B. Herrington, Jr., The problem of relative and absolute transfer of discrimination, *J. Comp. Psychol.*, 16, 1933, 199-206.

[11] M. Bentley, The memory image and its qualitative fidelity, this JOURNAL, 11, 1899, 1-48.

Throughout this note the discussion has been confined largely to the case of brightness discrimination. This is for two reasons. First, nearly all the experimental work has been done with varying degrees of brightness as the variables. Secondly, the discussion can readily be extended to the cases involving similar gradations in any attribute of the situation. It is unfortunate that the experimental work has been so sharply limited in scope. There remains the problem of its extension into other sense fields. Also, owing apparently to a disregard of negative cases, there remains the problem of determining under just what experimental conditions the 'relative' or the 'absolute' response will predominate. Many more experiments, shrewdly planned, are necessary before we understand the problem of perceiving in animals.

Cornell University F. NOWELL JONES

THE FORTY-SIXTH ANNUAL MEETING OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION

The Forty-Sixth Annual Meeting of the American Psychological Association was held at Ohio State University, Columbus, Ohio, on Wednesday, Thursday, Friday, and Saturday, September 7, 8, 9, and 10, 1938. A total of 1153 persons registered, classified as follows: 239 Members, 495 Associate Members, 66 newly-elected Associates, and 353 persons not affiliated with the Association.

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[12] I. Krechevsky, 'Hypothesis' vs. 'chance' in the pre-solution period in sensory discrimination-learning, *Univ. Calif. Publ. Psychol.*, 6, 1932, 27-44.

At the Business Meeting one Member was elected, 18 Associates transferred to the status of Member, and 250 Associate Members were elected.

Newly-elected officers are: President, Gordon W. Allport; Directors, Frank N. Freeman and Henry E. Garrett; Division of Anthropology and Psychology of the National Research Council, Harold E. Burtt, Florence E. Goodenough, and Walter R. Miles; Social Science Research Council, Richard M. Elliott; American Association for the Advancement of Science, Walter R. Miles and John A. McGeoch.

Karl F. Muenzinger was appointed as a new member of the Committee on Precautions in Animal Experimentation. Henry E. Garrett and Walter R. Miles were continued as representatives on the Joint Committee with the Psychologists League. John E. Anderson was continued as representative to the American Documentation Institute. John G. Jenkins was named representative to the New York Management Council.

Calvin P. Stone, of Stanford University, and Warner Brown, of the University of California, were appointed as members of the Executive Committee for the year 1938-1939 in anticipation of the Forty-Seventh Annual Meeting to be held in California sometime during the week beginning September 3, 1939. The Program Committee for the 1939 meeting will consist of Elmer K. Culler, Chairman, Edward B. Greene, and Willard C. Olson, Secretary.

An important decision affecting the members was made when it was voted, upon recommendation of the Council of Directors, to send the Psychological Bulletin to all Members and Associates beginning with the issue of January, 1939. Under this plan future programs will be printed as the July issue of the Bulletin, the proceedings will appear in November as usual, and the presidential address will be placed in the following January issue. Economies introduced permitted this action without a change in dues.

The Council of Directors announced the completion of the dissolution of the Psychological Review Company under the date of August 13, 1938. All publication matters will hereafter be conducted in the name of the Association.

The American Association for Applied Psychology, the Midwestern Psychological Association, and the Western Psychological Association were affiliated with the American Psychological Association in accordance with the provisions of Article XI of the By-laws.

The Association voted to rename the "Honorary Member" classification "Life Member" because of the automatic nature of the requirements set up in the By-Laws at the 1937 meeting.

The Association created a new Committee on Scientific and Professional Ethics to consider the advisability of drafting an ethical code the purpose of which would be to serve as a guide to Members and Associates. Robert S. Woodworth was made chairman of the committee, with additional members to be appointed. The Association also created a Committee on Press Relations, with B. F. Skinner as chairman and Donald G. Paterson and Horace B. English as members. The Council created a Committee on Assistance in Employment of Refugee Psychologists, with Gordon W. Allport as chairman and members to be appointed.

The Association created a Committee on Investments, with Willard L. Valentine as chairman and Leonard Carmichael and Samuel W. Fernberger as members. An Advisory Committee to work with A. T. Poffenberger and his associates in the construction of an alphabetical cumulative index for the Psychological Index was appointed, with A. T. Poffenberger as chairman and Karl M. Dallenbach, C. M. Louttit, and R. R. Willoughby as members.

A survey on "The Teaching of Psychology in Teachers Colleges" was presented by Dr. Arthur I. Gates for the Committee to Study Problems Connected with the Teaching of Psychology in High Schools and Junior Colleges, of which Goodwin B. Watson was chairman and Calvin P. Stone an additional member. The survey of the teachers colleges was added to the committee's assignment at the 1937 meeting. The Association voted to have one thousand copies of the report printed and distributed to presidents and teachers of psychology in teachers colleges.

The Association voted to re-appoint its delegates to the Inter-Society Color Council and to act as sponsor, without financial obligation, to the 1939 meeting of the Inter-Society Color Council.

The Council of Directors had received copies of resolutions protesting the holding of the next International Congress of Psychology at Vienna from various individuals and organizations including the Chicago Psychological Club, the Illinois Society of Consulting Psychologists, the Midwestern Psychological Association, the Psychologists' League, the Rocky Mountain Branch of the American Psychological Association, and the Western Psychological Association. On the recommendation of the Council of Directors the Association voted to adopt the following resolution:

> *Be it resolved,* That the American Psychological Association request the Committee in charge of arrangements for the Twelfth International Congress of Psychology to terminate, if it has not already done so, the tentative plan to hold this congress in Vienna in 1940; and that it request the Committee to arrange for the Congress in some country where the progress of psychology as a branch of science is not hindered by a government hostile to the tradition of free and unimpeded scholarship.

The Psychometric Society arranged a scientific session on "Psychometrics" and a Round Table on "Factor Analysis" on Thursday in collaboration with the Program Committee of the American Psychological Association. Two hundred and one persons registered for the meetings. The Business Meeting of the Society was held on Wednesday. Officers continued for the coming year are: President, Joy Paul Guilford; Secretary, Jack W. Dunlap; and Treasurer, Albert K. Kurtz.

The National Institute of Psychology held sessions at 4:00 on Wednesday and Thursday. The Business Meeting was held at 5:00 P.M. on Wednesday. The officers for 1936-1941 are: President, Samuel W. Fernberger; Vice-President, F. L. Wells; Secretary-Treasurer, Karl M. Dallenbach; Directors, Franklin Fearing and Herbert Woodrow.

The Society for the Psychological Study of Social Issues arranged a scientific session on the "Psychology of Social Institutions" for Thursday in collaboration with the Program Committee of the American Psychological Association. Two hundred and fifteen persons registered for the meetings. At the Annual Business Meeting on Tuesday George W. Hartmann of Columbia University was elected Chairman for 1938-39 with G. W. Allport, F. H. Allport, Gardner Murphy, and E. C. Tolman as new members of the Council for the term 1938-40. I. Krechevsky of the University of Colorado was continued as Secretary-Treasurer. The same evening Gardner

Murphy delivered the presidential address on the subject "The Research Task of Social Psychology."

The American Association for Applied Psychology met September 5, 6, 7, and 8, 1938, in conjunction with the Association. Five hundred and thirty persons registered for the meetings. Besides the four research programs (listing 32 papers) arranged in collaboration with the Program Committee of the American Psychological Association, there were seven symposia and a round table, a public meeting with five addresses, a recessed organization meeting and the regular business meeting, and a large and varied array of conferences dealing with professional problems. Following the annual dinner the retiring President, Douglas Fryer, delivered an address on the subject "Applied and Professional Attitudes." Professor Donald G. Paterson was elected President, Dr. Horace B. English, Executive Secretary, and Dr. Jack W. Dunlap, Treasurer.

The program of the American Psychological Association consisted of twenty-one scientific sessions in which 172 papers were presented by Members and Associates. Six round tables were arranged for Thursday and Friday and fourteen research and instructional films were scheduled for Wednesday evening.

The Presidential Address was delivered by John Frederick Dashiell of the University of North Carolina at the general session in the Chapel of University Hall on Friday on the subject "Some Rapprochements in Contemporary Psychology." Members of the Department of Psychology entertained members and guests at the Faculty Club at the conclusion of the general session.

Excellent facilities for the meeting were provided by the University. The Faculty Club, Dormitories, Gymnasium, Swimming Pools, Tennis Courts, and Golf Course were opened to the Association. Professor Harold E. Burtt as Chairman, his colleagues, and many administrative officials earned the well-deserved thanks of the Association.

University of Michigan WILLARD C. OLSON

THE 1939 MEETING OF THE INTER-SOCIETY COLOR COUNCIL

The annual meeting of the Inter-Society Color Council will be held in New York, Feb. 23, in the auditorium of the Electrical and Gas Association in Grand Central Palace. The program of the afternoon session is sponsored by the American Psychological Association and will deal with the psychological, physical and practical aspects of the problem of "Color Tolerances." In the evening, each one of the member societies will present a short popular demonstration of some aspect of its work with color.

Members of affiliated societies are welcome to attend the programs but, since seating facilities are limited, they should secure admission cards in advance from the chairman of their delegation.

Hobart College FORREST LEE DIMMICK

BOOK REVIEWS

Experimental Psychology. By ROBERT S. WOODWORTH. New York, Henry Holt and Co., 1938. Pp. xi, 889.

With the publication of this book, Woodworth becomes the chief encyclopedist of American psychology, for there has not been, in psychology's central sector, a book of such wide scope, implemented by such multitudinous detail, since Wundt's *Physiologische Psychologie,* nor, I should think, a book of such meticulous wisdom since Titchener's *Manuals.* Covering as it does, without systematic bias, almost the whole range of what is loosely called *general psychology,* this volume should have a marked effect upon graduate instruction in psychology. It is, in short, *the* definitive book in general psychology, and its publication, so long awaited, is an event of the first importance.

The text could have been divided into four main parts as follows: I: Learning and memory (memory, retention, memory for form, conditioned response, maze learning, practice, skill, economy, interference): 229 pp. or 28%. II: Reaction and feeling (simple feeling, facial expression, bodily changes in emotion, psychogalvanic reflex, reaction-time, associative reaction, esthetic preference): 158 pp. or 19%. III: Sensation and perception (psychophysical methods, psychophysical data, skin senses, smell, taste, hearing, sight, eye-movements, color, perception of form, visual space perception, apprehension): 321 pp. or 39%. IV: Thought (reading, problem solving, thinking): 111 pp. or 14%.

Woodworth defines experimental psychology as the empirical field where there is control of the independent variable. On this ground he excludes from his book all the research where the 'correlational method' is used, an exclusion that makes his undertaking more nearly possible. He also omits, only partly for the same reason, the data of child development and the data of psychotherapy. Doubtless he will be accused of bigotry in applying the title *experimental psychology* to so limited a field, but he has a sanction of usage in spite of the persistent protests from psychology's less aged periphery.

It is a large book—823 pages of text—about 425,000 words.

Inevitably one asks whether such a book is up-to-date. Woodworth himself, in his preface (p. iii), examines the dates of the 1729 references that he gives. A third of them are for 1930 or later. A fourth of them are before 1900. The frequency function of citations is continuous: the older the fewer. These statistics, however, do not present the whole truth. Woodworth belongs to the school of historical thought that regards mutations of thought as abnormal. He readily sees the present in the past, and again and again he cites the recent studies as confirming or explicating some finding of 30 or 50 years ago. Of course antiquity varies with subject-matter. In the chapter on reaction-time, 82% of the references are dated before 1930, whereas in the chapter on the conditioned reflex, 83% are dated in 1930 or later. There are zealots who will find the chapters on learning and memory antiquated because they do not orient themselves with respect to the 'dynamic' concepts of motivation, but they will be unfair to Woodworth. He has set himself definitely to avoid speculative hypotheses and concepts that do not find ready meaning in terms of experimental operations. His chapter on the conditioned reflex, for instance, is really ultra-modern, for it is almost a special chapter on motivation, although Woodworth does not mention that word. He is sticking to 'facts.'

On the other hand, by 'sticking to facts' Woodworth does not fail to achieve scientific generalizations. He is less opposed to hypothesis than to pedantry. In the art of the neutral approach, bare of the pedantic substantialized hocus-pocus with which psychological thought is habitually encumbered, he is past master. The way in which he persuades the reader that the conditioned reflex depends on the wishes of the subject and not on the frequency of the concurrence of stimuli (pp. 92-123), and that the rate of reading depends not on the speed of the eyes but on the speed of comprehension (pp. 722-744), exhibits this method in perfection.

Neutral approach. Woodworth's use of the 'neutral approach' to achieve scientific generalization while avoiding controversy and pedantry is so unusual as to need description. Here are six instances in which I think Woodworth makes a positive contribution in avoiding the witchery of word-magic.

(1) The chapter about the transfer of training (pp. 175-207) is a straightforward account of all the ways in which prior learning of one material affects subsequent learning of another material. By keeping to such an 'operational' definition of transfer, Woodworth avoids both the question of what is transferred and the problem of identical elements.

(2) Similarly the chapter on PGR (pp. 276-297) is simply a chapter about PGR, the way in which differences of potential can be maintained at different points on the skin—another operational definition. Woodworth himself thinks of PGR as meaning *psychogalvanic reflex,* but, if someone else wants to think of it as "perspiratory galvanic reflex," Woodworth not only sees no objection but presents the evidence for its being perspiratory.

(3) In discussing reaction-times Woodworth distinguishes between the simple reaction, where the stimulus and response are always the same, and the disjunctive reaction, where the stimuli ('discrimination') or the responses ('choice') vary. Disjunctive reactions take longer, and Woodworth is free to discuss the problem as to why they take longer because he avoids all the implications of the 'subtractive procedure' (pp. 333-339).

(4) The section on PSE ('point of subjective equality') as it varies in time (pp. 438-449) is splendid because it is simply about PSE, which is exactly what varies in time. All the speculative problems disappear. Is the variation a 'time-error'? Is it the variation of a 'trace'? Is the 'trace' physiological? If so, what is it physiologically? Or is the psychologist in making such observations really a physiologist? That is the kind of word-magic in which Woodworth does not deal.

(5) Another example of Woodworth at his best is his chapter on attention (pp. 684-712). Here he reduces attention to the capacity for simultaneous quick apprehension, which is what most of the experiments measure. With that conception he rids himself of everything connected with the question as to whether attention is a dynamic force or an observable sensory attribute.

(6) In his chapter on solving problems (pp. 746-782) he enters into the question of theory, as he did not in these other instances. He asks: is a problem solved by trial-and-error or by insight? In such a situation Woodworth is not an eclectic; he does not choose the 'better' view. Instead he discusses both views, introduces the experimental evidence, and ends by convincing the reader—at last he convinced me—

that the supposed dilemma was merely fancied, that there is no difference between the views. All solving of problems, he might have said, is by insight-and-error.

This then is the 'neutral approach' which characterizes the book. Woodworth does not, however, always live up to his own genius in this regard. His chapter on thinking (pp. 783-800) is, I think, a notable failure on his part to rid himself of the limitations of classical pedantry, for he gets himself involved with the problem of consciousness (cf. also pp. 818-823), a concept for which he has ready no simple clarifying definition like his equation of the memory trace to PSE. Without such a definition one may not say whether thought that lacks adequate imagery is conscious or unconscious. When Woodworth pleads, as he seems to me to be doing, for some kind of substantialized non-sensory conscious content, I do not know what he has in mind (cf. also p. 43 f.). If he had seriously developed his own response theory of consciousness, he might have made this chapter, like the others, superior to controversy about fundamentals.

There are, of course, many psychologists who will reverse my praise and blame in this matter of the 'neutral approach.' They will say that, without speculative hypothesis, science loses its breath of life. That may be true of science as a whole; nevertheless we need also an occasional assessment of the accumulation of fact in psychology, and that, I take it, is what Woodworth is trying to do. Even those who can not stand facts unless they are hung on graceful hypotheses can applaud Woodworth for doing well what he sets out to do; and, if they do not like 'neutrality,' now at last they have a book in which to sink their teeth.

Essay style. For all his 'neutral approach' Woodworth has not written a handbook of experimental psychology. Besides being too small for a handbook, this text repeatedly adopts the essay style. Thus in places the exposition becomes unbalanced, occasionally overweighted. Woodworth, himself intrigued by the matter in hand, lets himself go, until the result, though often an excellent essay, is too long or discursive for the company that it keeps. Let me give examples.

The discussion of the Gestalt theory and G. E. Müller's theory for the memory of visual form (pp. 80-84) seems to me unnecessary in a book that in general eschews theories. Unduly long in view of the fact that it achieves no positive conclusion is the essay on the nature of simple feeling (pp. 235-241). Likewise the essay on facial expression (pp. 242-254) is too discursive for this book. The reader infers from the opening paragraphs that the Boring-Titchener-Piderit model shows what it was meant to show and then has to be undeceived—a method that involves the bad pedagogy of associative inhibition. Henning's smell prism does not merit 8 pages of discussion (pp. 483-490), nor should the essay on the localization of sound (pp. 518-534) receive 16 of the 38 pages allotted to the topic of hearing. The 4 pages devoted to the task of showing that the eye is not blind during eye-movement (pp. 591-594) are only 4 pages, but half a page would have done as well, except that Woodworth knows some "younger psychologists" who need convincing. The essay on visual space perception goes back to Leonardo and takes 12 pages to get past Panum (pp. 651-662). That is an historical luxury, although his concern with the remote past has some justification in the fact that he is devoting his chapter to showing that the visual perception of depth is purely visual and independent of accommodation and convergence. The essay on trial-and-error vs. insight (pp. 746-782), though pleasant enough reading, wastes precious space: the conclusion is clear long before it is reached. Nor does the psychology of deduction (pp. 807-817) merit 11 pages. Counsel of perfection *ex post facto* is easy. No author, I suppose, ever neglects his favorite topics. The point here, however, is that the very style of this text—a text which is not a handbook—enables its author to indulge himself with such ease.

Since the text is not a handbook, would it not be better if it never tried to function as a handbook? The details of the computative procedures in the psychophysical methods (pp. 396-421) seem to me to be out of place. They and the Müller-Urban table are there to be used, but not to be read. If Woodworth felt that Spearman's methods have been neglected and should at last be given their due, if he felt that a table of the normal ogive with the SD as the argument ought to be more readily available, then he could have put all this material into an appendix, along with the mathematics of the experimental methods of esthetics (pp. 370-381).

Physiology. One of the most remarkable features of this book is its unphysiological nature. It is my belief that the words *neuron, synapse,* and *thalamus* do not occur in the book, nor the names of any of the endocrine glands. The cortex gets mentioned casually in connection with theories of vision. The chapter on hearing finds no need to mention the basilar membrane, and little to say of the organ of Corti. On the other hand, the rods and cones of the retina get their due, and fully as much is said about cutaneous receptors as present knowledge could justify. Some of these elementary facts are presumably presupposed. Certainly Woodworth takes it for granted that the all-or-none nature of nerve-conduction is known in advance to his readers, that and the facts of refractory period. On the function of the autonomic nervous system and the endocrine glands in emotion, he specifically refers to texts of Cannon, Bard and Dunbar (p. 257), limiting his own discussion of bodily changes in emotion to respiration and circulation. Perhaps the most explicitly physiological section in the book is the two pages on the neurophysiology of the PGR (pp. 282-284).

Has Woodworth omitted something essential? Yes and No. No psychologist nowadays would dare to know as little physiology as Woodworth seems to recommend. On the other hand, the concern of recent psychology for physiological detail has tended to take attention away from the mass of psychological fact that no physiologist ever considers. Woodworth's text, too unphysiological perhaps for a psychologist's Bible, nevertheless shows how very far the physiologists are as yet from being able to substitute for the psychologists.

Omissions. Besides this premeditated omission of physiological fact, there are a number of other gaps which can hardly be oversights. Pressure from the publishers would, for instance, have squeezed out whole topics more readily than it would have condensed the fluency of some of the essays. I list here six serious omissions.

(1) The text is inadequate in its discussion of somesthesis, largely because it limits itself to cutaneous sensibility. There is nothing about subcutaneous sensibility, kinesthesis, proprioception, hunger, thirst, and the other organic sensibilities. There is nothing about the 'static' sense except sixteen lines in the middle of the chapter on eye-movements. Nor is the discussion of cutaneous sensibility complete. I find nothing about after-sensations or about the perceptual fusions (wetness, roughness, stickiness, etc.). Paradoxical cold is mentioned casually and the Alrutz-Von Frey theory of heat not at all. While the neglect of the hypotheses of epicritic and protopathic sensibility pleases me, I nevertheless believe that the facts of returning sensibility after nerve injury are illuminating, Woodworth ignores them. I should like him also to say

something about Von Frey's discovery that the pressure thresholds for small stimuli are constant only if expressed in grm./mm. and not grm./mm.^{2}, and then he ought not to give these thresholds in grm./mm.^{2} as he does, citing Von Frey's early data (p. 455).

(2) There ought to be more than three pages (pp. 680-683) on the perception of motion. The subject has an instructive history and a considerable body of accepted fact. Being a topic that the physiologists neglect, it could well have been emphasized.

(3) There ought to be something—ten or fifteen pages at least—about the perception of time. There is nothing. The *Zeitsinn* used to be important in experimental psychology.

(4) The discussion of psychophysical methods and results omits all mention of the problem of equivalents and of equal sense-distances. This is a serious omission for it allows Woodworth to assume the approximate validity of Fechner's law, whereas, if he had had to give data on the variation of the j.n.d. in subjective size, he would not have left the matter in such a simple state. On the other hand, it may be said that some of these data are new, and no book can be quite up to the minute. (See Stevens and Davis, *Hearing,* 1938, 76-84, 94-97, 112-123, and references there cited from 1933 to 1937.)

(5) Except two sentences tucked away in a paragraph about color blindness (p. 556), there is nothing about chromatic sensibility in indirect vision.

(6) Even admitting that physiological fact is left out on principle, an exception might have been made in the case of the structure and functioning of the inner ear. So many facts of hearing begin to make sense when the nature of the sense-organ is known.

Specific criticisms. We come now to the specific criticisms. I want first to argue two important ones at length, and then briefly to list the rest, leaving the reader to form his own opinion of their importance.

(1) First of all let me deplore the fact that Woodworth teaches our students that Weber's law holds approximately in the middle ranges of intensity (pp. 430-438). I had hoped that Holway and Pratt (whom Woodworth cites in his bibliography) had definitely settled that notion. (Cf. also the Boring-Langfeld-Weld textbook, 196-202.) In every known case the Weber fraction decreases at first steadily as intensity increases. The function may level out as for tone, or it may 'kick up' again at the high intensities, as for brightness. The typical form is a curve with a determinable minimum. It shows thus best in a semi-log plot where the fraction is plotted against log *I*. Woodworth says that the use of a log scale "assumes Weber's law as a basis for arguing against it" (p. 433), but that is not true. The constant-increment arithmetic scale is the simplest frame of reference. The constant-ratio geometric scale is the next simplest, and the one to which we turn for exhibiting trends that will not show on the other. One would use semi-log coördinates to plot size of cities against their frequency, and assume nothing about Weber's law. Moreover, if the Weber function were plotted against arithmetic scales so that the curve at the low intensities was practically crowded out of the picture, then the 'kick up' at the high intensities (cf., *e.g.* König's data) would be exaggerated. Or, if one plotted only the middle portion on arithmetic scales, then one would naturally increase the scale for the Weber fraction, so that what had seemed to Woodworth approximately constant, would now be revealed as significantly curved, for the semi-log plot straightens out the middle as well as curving up the ends.

It is the acceptance of this faulty approximation that enables Woodworth to say that decibels measure equal sensation distances (pp. 325, 437). Actually curves have been drawn showing loudness scales in directly judged sense-distances, summed j.n.d.'s, and decibels, and the relation between no pair of these three measures is linear. (See Stevens and Davis, 118, 138 f.). Woodworth also says that Miss Gamble found that "Weber's law holds" for smell (p. 479). That is what Miss Gamble thought too, because the Weber fraction varied between 1/3 and 1/4 in 62% of the cases, and between 1/2 and 1/6 in 90% of the cases. We should hardly call that constancy nowadays.

(2) Although my next criticism appears to be only verbal, it is directed toward the confusion of thought which a wrong word yields. Woodworth, in discussing perception, uses the word *cue,* when he should say, I think, *cause.* Here are the instances. The localization of sound depends, he says, upon the binaural cues present in dichotic stimulation (pp. 525, 532 f.). He explains the tendency toward constancy of hue and brightness in colors seen under different illuminations as dependent on cues that the observer gets for his interpretation of the objective situation (p. 595). So too he accounts for the visual perception of depth as entirely a matter of the available cues that give information about the third dimension (pp. 651-680). One perceives words, he says, in accordance with the cues that the print provides (pp. 737-745). Woodworth thus gives the student a picture of a perceiving observer who gets all the cues he can from the total situation and reconstructs (or interprets, or infers to) objective reality from the data. Almost identically Helmholtz gave us for the explanation of perception the *unbewusster Schluss,* a concept that was generally abandoned sixty years ago, for the reason that there is no discernible interpretative process. It is true that Woodworth says: "We are not to suppose that the subject *notices* time or intensity differences and *infers* from them the location of sound. The small fractions of a ms. which are found to be effective direction cues are imperceptible as time-intervals. In order to approach any theory of sound localization, we must think in physiological terms" (p. 533). But similarly Helmholtz said that the *Schluss* was unconscious, instantaneous and compulsory—a contradictory statement.

Take what Woodworth calls the "conflict of cues." He says that, if an experimenter "applies equal intensity to the two ears along with a time difference, the intensity cue indicates a source in the median plane, while the time difference locates it on the side of the earlier reception of the sound. Which of the conflicting cues will the listener follow in his localization?" (p. 525 f.). The fact is that there is no conflict in such a case. The evidence of conflict would be hesitation and fluctuation. Both the intensitive and the temporal dichotic differentials are conjointly effective in determining localization. Every binaurally determined localization is the resultant of these two causes. It is not a conflict of cues when the trajectory of a bullet is curved: it is a resultant of concurrent causes. And that is what happens in localization and perception in general.

It is not easy to decide what other specific criticisms should be advanced. Every careful reader of so large a book will have his list of 'errors,' many of which will turn out to be mere antipathies. Let me select a few from the field of sensation where I am most at home, and let the reader judge for himself how heinous are these departures of Woodworth's from my straight and narrow path.

First come two matters of definition. In spite of his desire to avoid systematic issues, Woodworth has attempted to state a distinction between *sensation* and *perception* (pp. 450 f.): "*Sensation* points to the sense organs with their nerves and nerve centers as the object of study; *perception* points to the objects of the world which we know through the senses." The conclusion that I draw from this formula is that perception becomes sensation as soon as you find out about its physiology, that the visual depth which comes from light and shade is perceptual because light and shade are in the object, but that stereoscopic depth is sensation because retinal disparity pertains to the sense-organ. For feeling Woodworth avoids a definition, but with the result that he can not take positive ground on the question of mixed feelings (p. 241). If he means by *feelings* either experiential processes or reactions, then he could have found good experimental ground for a dogmatic denial of simultaneous mixture; but, if he means 'meanings,' why then, of course, feelings mix.

In the field of psychophysics Woodworth gives valuable consideration to two insufficiently understood matters: the dependence of the size of the DL upon the width of the category of equality, and the relation of the method single stimuli to the principle of central tendency (pp. 425 f., 446-449). I applaud his treatment, and then add two complaints. The DL is not "more directly obtained" by the method of just noticeable difference than by the method of constant stimuli (p. 424), for the data from both methods have the same logical implications (if serial contradictions are ignored), and the size of the DL in both methods depends on the attitude of *O*, as to whether he uses a wide or a narrow category of equality. My other point is that the principle of central tendency, having been introduced so successfully with the method of single stimuli, ought also to be applied to the exposition of the relativistic nature of color constancy (pp. 599-801, 614, 616), for most of the magic disappears from the constancy-phenomenon when it is considered in this way.

The discussion of tone has elicited dissent from me a little oftener than has any other topic. Here are my complaints. Pitch difference and musical interval ought not to be equated (p. 504 f.), for they are different and the non-linear relation between them can be determined (cf. Stevens and Davis, 76-84). The exact ratio as the determiner of musical interval has gone, surely, into history, yet Woodworth says: "In music a slightly mistuned interval is not almost as pleasant as the true interval but quite unpleasant. In music the exact ratio counts heavily" (p. 386). Does it? when the musical *Os* who like the third best among the intervals still like it best in equal temperament where it is 16 cents off? when they like it best with a vibrato that varies it through 30 cents? The exact ratio does not count heavily for tonal fusion (Brues, 1927), and there is unpublished evidence (Newman) that it does not for pleasantness. Then Woodworth says: "According to Köhler's findings simple tones do have vowel quality, and vocality should be added to the list of tonal dimensions" (p. 514); but that, I think, is a non sequitur, for Külpe's rule that it takes independent variability to establish separate attributes still holds. Perhaps vocality *is* simply the pitch. Finally I have to say that beats do not establish difference tones (p. 515) unless there is distortion of the wave-form. When there is no distortion, Ohm's law works and the components are heard out without a difference tone, but distortion so changes the wave-form that a Fourier analysis or Ohm's law produces the beat frequency as a component. Perhaps sensation forms too technical a field for perfect adequacy in the general encyclopedist.

My chief regret about the chapter on color vision is that Woodworth has neglected the cortical theory of color vision. He mentions it for after-image (pp. 560 f.) and for binocular mixture (pp. 574 f.); and he mentions Helmholtz's *unbewusster Schluss* for contrast (pp. 569 f.), which is not the cortical theory I have in mind. Yet he needs such a theory for his discussion of color mixture and color blindness (pp. 555 f.). One of the best arguments for the cortical theory is that dichromats, who can match all hues with mixtures of two, nevertheless see yellows and blues. The thought is that the retina is trichromatic and that the brain is tetrachromatic, with the paired incompatibilities of the Hering theory (no reddish greens) holding, not at the retina, but in terms of central mutual inhibitions.

Of the skin Woodworth says conventionally: "While not conclusive, the evidence is rather in favor of localized receptors and punctate sensitivity" (p. 460). To that I reply: Of course, there must be localized receptors in the skin, just as there are in the retina and the organ of Corti. Anything else would be a neurological marvel. And, if the receptors are localized, sensitivity will have to be punctate if only you can circumscribe the effect of the stimulus enough, and not otherwise. Obviously strong stimulation could not be punctate, for pressure spreads and tissues conduct heat. Thus I think Woodworth takes the classical problem too seriously.

And that—as nearly as one can paint a miniature of an elephant—is Woodworth's *Experimental Psychology,* which we have all waited for so long. It will have a great influence, and its encyclopedist-author merits the gratitude of professional psychologists.

Harvard University EDWIN G. BORING

The Intelligent Individual and Society. By P. W. BRIDGMAN. New York, Macmillan Co., 1938. Pp. vii, 302.

This may be briefly characterized as the reaction of a distinguished physicist to the problems of the universe within the fields of philosophy, psychology, sociology, and political science. As is well known, Bridgman, in his *Logic of Modern Physics* (1928), took the position that concepts in physics and other experimental sciences have no validity beyond the observations and that the observations can be regarded as true only for the particular conditions under which the observations are made. This he called the operational interpretation of concepts and if applied universally would result in a rigid empiricism. It would closely limit the inferences that could be drawn from any observation, if it would permit inferences at all. This work extends the empiricism of the former book to make a destructive analysis of the generally accepted axioms of most of the sciences and then in the later chapters makes a mild retraction of the resulting complete scepticism.

The book begins with a suggestion from physics that even the most fundamental concepts of science are not unambiguous. The measurement of velocity depends upon the position of the observer. Time and space are merely convenient notions and have little claim to absolute existence. Of the space-time dimensions of modern physics, Bridgman says, "there has been a great deal of bunkum about their situation." Cause is said also not to stand critical analysis.

Meaning is considered at length to the conclusions that words never have completely consistent meaning to all people. One can seldom find an actual event or object that corresponds to a word or phrase. Nevertheless the thinking of the ordinary man goes no further than combinations of words. Verbalization is one of the ultimate substitutes for thought on the part of even the most critical thinkers, scientific as well as philosophical and popular. The fundamental verbalisms usually receive support from mysticism. God, law, nature when assumed in explanations are unknowable and while they give an emotional force to statements they add no real value to the conclusion. There is in the last analysis no complete justification for any statement that can be made except the operational one. That an idea works alone can be accepted. This test does not carry conviction as to the result of its application in any other instance or at any other time than the one in which it is found to work.

Social concepts are subjected to a similar destructive analysis. Ought and duty have the primary place in all social and ethical discussions. In usage they reduce to action in harmony with sublimated public opinion. Attempts to assign them to God's purposes does not stand analysis for no one knows God's purpose on any occasion. This is merely a verbalism to arouse emotions. Rights, property, morals of all sorts are verbalisms that can be given no rational justification. That they are of social origin is shown by the different values assigned to them in different societies, even in different civilized nations.

The state, law, justice are among the other social or political concepts that are reduced to mere verbalisms, that have grown up in society for the convenience of rulers or to justify social usages that at some time or another have made social coöperation or discipline easier. The sense of guilt is foolish as is any interest in the esteem one should have for one's self after one's death. Most of the fundamental values usually treated as the foundation of the social system are asserted to be the result of the extension of verbal combinations beyond the limit of applicability. They are the results of rationalization.

After all rational knowledge is questioned and all generalizations are rejected, Bridgman comes back to the emotional or instinctive life to justify decisions. The ultimate warrant for any action is the desire of the individual, just as the cause of any individual act is the mass of individual impulses. Even these can be generalized only on the unproven assumption that other members of society have the same emotions and impulses. One should expect to attain not absolute truth but a sense that a decision has been reached only after it has been scrutinized from as many points as possible. This attitude is favored by asking 'why' of each decision. The answer to the 'why' is not to be found in a logical formula or a rationalization but from an appreciation that the result will be pleasant to the individual, will fit into a broader scheme and will have desirable consequences even in remote applications. Ultimately the only justification is "I like it." To apply what the author would call a familiar verbalism "not reason but emotion or feeling must decide the course of action and in the last analysis truth and morality as well."

Bridgman's position has some resemblance to Kant's rule for ethical conduct so to act that the rule might be made a universal law. One "must not demand or allow for one's self as an individual any privileges which if assured for every one, would result in a kind of society that I cannot contemplate with satisfaction." Various present problems are tested by this rule. It is suggested that society might divide into those who like to use their brains and the great majority who do not. No ultimate way of convincing one group or the other can be expected. The man who enjoys thinking for its own sake will do it. The others will be content with verbalisms of ancient vintage. The latter will never accept the standards of the former. Bridgman also asserts that society must settle certain fundamental problems by physical force since not every problem can be solved by reason. He asserts that one must be ready to give over bitterness towards individuals who obtain their ends by force or guile. If they are successful, they must be accepted.

A judgment on the book is difficult. One cannot question the statements of the limitations of physical concepts or exact formulations in physics, nor can one question that there exists a general tendency to rationalize, to avoid the necessity for thought by verbalisms or by the use of mystic terms. One might apologize for laws and formulations as approximations and assert that with human limitations what they are, the formulations are necessary and not harmful, if they are not taken too seriously. Bridgman's objection to verbalisms may well be opposed to the Austrian logicians' assertion that words are the only mental reality in a point to point identity between external objects and thought. Bridgman may solve the problem by a denial of all reality.

Whether man will be satisfied to meet a scepticism more complete because more closely founded on fact than was the pre-socratic by the substitution for knowledge of personal feeling is a question. When subjected to Bridgman's criterion of rejecting all but concrete knowledge and making all acceptance dependent upon personal liking or dislike, the decision would lie with the personal reaction of each reader. Nothing more can be said.

University of Michigan W. B. PILLSBURY

Der Sinnbegriff als Kategorie der Geisteswissenschaften. By WILHELM KELLER, München, Ernst Reinhardt, 1937. Pp. 176.

This book, a prolegomenon to two forthcoming volumes, is an admirable, scholarly, and original study. Unfortunately, its ponderous, inflexible style, its interminably long and involved sentences, and its highly technical vocabulary—many of the terms being newly created—aggravate the task of the reader. To make himself understood, Dr. Keller explicates his thoughts by repeated definitions which have to be mastered before the points at issue can be comprehended. Exact for one slip (in one place he writes "A 'wooden iron' is an indeterminate being," and in another, "There are no indeterminate beings"), this book is a model of logical precision and integrative thinking.

The purpose of the book is "to take up the struggle for the logical and methodological bases of the *Geisteswissenschaften*" (p. 9), which are very different from those of the natural sciences as has been brought out by Hegel's classical antithesis between the philosophy of nature and of *Geist.* The *Geisteswissenschaften* aim at the comprehension of psychological and intellectual facts (real or imaginary, objective or hypothetical) and of ideally correlated situations. Psychology, sociology, and all branches of history belong within its scope. Philosophy, however, does not.

Within this book, Dr. Keller considers in separate chapters the following topics: "The primacy of method and its limitations;" "The relation of method and 'object' (*Gegenstand*);" "The 'object' of the *Geisteswissenschaften;*" "Meaning, understanding, and insight;" "A basic plan of a theory of the sciences."

The method of the *Geisteswissenschaften* has not only to determine its 'objects' but it is itself an intellectual phenomenon and, as such, it has to comply with the requirements that it sets up for its objects. In contrast to the natural sciences—which contradict themselves in applying themselves mechanistically to themselves—the *Geisteswissenschaften* are not only sciences of the mind but they are also sciences of themselves. They must accordingly, in their affirmations, justify their own existence. Thus psychology, for example, must continuously reëxamine and affirm its own foundations, being equally a science of facts and principles. The author attempts to escape the infertile discussion of method alone by treating all the *Geisteswissenschaften* together as an entirety.

In placing the concept of meaning (*Sinn*) in the focal point, the author intends to illustrate its fundamental character. Indeed, the analysis of the categories of 'object' and 'method' is undertaken not only for methodological clarification but also, and principally, for the reason that it gives the first indication of what 'meaning' and 'meaningfulness' (*Sinnhaftigkeit*) really are.

The valid conditions of the *Geisteswissenschaften* are in general determined through cognition. Therein lies the reason for the affinity with philosophy. The *Geisteswissenschaften* become philosophical as soon as they treat of the fundamental characters of the intellectual structure. In contrast to the nativist, as well as to the empiricist, Dr. Keller points to the dynamic processuality of cognition in its anthropological and ontological aspects.

Meaning is a fundamental category, not only in the actuality of its application but in its inner importance. It is not merely an internal affair of method, but it is initially constitutive of the objective being. It is the expression of a functional situation which holds together the 'being' and the 'being an object.'

Dr. Keller's thesis augurs well for the *Geisteswissenschaften.* In emphasizing the dynamic and progressive aspects of cognition, it deviates from traditional epistemology, and it challenges the scientific psychologies of today which are diversely ameliorated modifications of inherited British associationism.

The theories espoused are subject to criticism, but a critical review should await the forthcoming volumes in which the author promises to test and to substantiate his theories.

New York, N.Y. LUDWIG SCHOPP

A Biological Approach to the Problem of Abnormal Behavior. By MILTON HARRINGTON. Lancaster, Pa., Science Press Printing Co., 1938. Pp. 459.

In his *Wish-Hunting in the Unconscious* Dr. Harrington attempted to annihilate psychoanalysis; in the present book he attempts the praiseworthy task of providing "a new approach to the problem of abnormal behavior" which calls into account "the outstanding facts of anatomy, physiology, and psychology." In Part I (First Principles, 66 pages) he defines science as a system of conceptual constructs which provides prediction and accepts the requirements of a scientific theory as precision, simplicity, harmony with the facts, and comprehensiveness. All behavior is assumed to exist as the action of an anatomical mechanism which is governed by fixed laws. Conscious states are regarded as epiphenomena caused by the same bodily processes which cause behavior, and useful, therefore, as indicators of these processes.

In Part II (Psychophysiology, pp. 69-283) Dr. Harrington describes behavior as mediated by the nervous system which consists at once in a complicated reflex arc and a generator and reservoir of energy. Afferent impulses from stimulated sense organs (internal and external) produce active energy within the nervous system and a state of nervous tension. Psychological adjustment is finding relief from this state of tension and is achieved when the energy flowing out to the effectors has produced behavior which relieves the organism of the stimulus. This relief may be achieved in five ways, by "accomplishment," by "withdrawal from the stimulus," by "modification of postures," by "modification of thought," and by "disabling the machine." Four tension mechanisms (pain, pleasure, anger, laughter), the structure of the nervous system which is altered with every new adjustmental act (learning), and the *law of confluence* determine the form of action taken in achieving adjustment. The law of confluence states that when the organism is subjected to two or more stimuli at one and the same time, there is a tendency for the impulses aroused to coalesce. Justification for this law is found in the phenomena of conditioning, inhibition, and "reënforcement" (Sherringtonian summation), the same phenomena which the law is designed to explain (a beautiful example of circular logic). The James-Lange theory of emotion is adopted, and systematic but over-simplified conceptions of the physiological processes underlying sensation (*e.g.*, passage of an impulse originating in a single receptor to the brain), memory, thought, and anticipation are described.

In Part III (Psychopathology, pp. 287-431) psychopathology is defined as "the scientific study of why we misbehave and why we are unhappy and suffer pain." The requirements of the situation, the stimulus that calls forth the behavior, and the nature of the organism, determined in turn by heredity, education, and state of bodily health, are combined in causing behavior. Abnormalities are divided into two types: those of *impulsion* and those of *adjustment*. Every person suffers at some time from abnormal impulses, but they defend themselves by means of adjustment. Adjustment may fail, however, because relief from tension is obtained through unapproved accomplishments, withdrawals, or modifications of thought and posture (mal-adjustments), because relief from tension is not obtained at all (non-adjustment), or because the troubled person "disables the machine." A wide range of the phenomena of psychopathology are thus given systematic organization and explanation.

Except for an irritating interspersing of some form of "the answer is obvious," the style of the exposition is simple, clear, and should be interesting even to a lay reader. As Harrington's system serves well to classify and to account rationally for the phenomena of psychopathology, his book might serve well as an introductory text in mental hygiene, were it not for two defects: (1) His failure to take into account the facts uncovered in neuro-physiology, physiology, and psychology during the last 20 years would be obvious to students who had had courses in general biology and general psychology. (2) His hostility toward other contributors in the field of psychopathology, especially Freud, makes his book a bad example for inculcating an unemotional critical attitude in elementary students.

As a "new approach to the problem of abnormal behavior" which calls into account "the outstanding facts of anatomy, physiology, psychology" it falls far short of the mark. Psychopathology is in dire need of a major theoretical contribution which will marshal and integrate these facts, clarify the issues, and open new avenues of research. Harrington has not done this. He refers to the contributions of Sherrington and a few others when they fit into his system, but when the facts of research do not fit the reader hears nothing of them.

Harrington bitterly criticizes Freud for providing a metaphorical motivistic account of abnormal behavior, but his own constructs, "energy," "tension," "confluence," and his restatement and elaboration of McDougall's drainage theory are no more precise and have no more of an operational basis than have Freud's "libido," "cathexis," and "unconscious." Harrington has contributed an interesting rational system of physiological psychology which organizes the phenomena of psychopathology, but is mechanistic metaphor better than motivistic metaphor?

Brown University J. McV. HUNT

Die Technik der analytischen Psychotherapie. By WILHELM STEKEL. Bern, Switzerland, Medizinischer Verlag Hans Huber, 1938. Pp. 317.

After thirty years of active psychotherapeutic practice, and in celebration of his seventieth birthday, Wilhelm Stekel, author of numerous books on special branches of abnormal psychology, sets forth for beginners the fruits of his experience. There are many paternal passages: "What I am here reporting is an extract from several decades of experience. I hope that it will be some assistance to my younger colleagues (p. 40). One learns . . . only by experience; beginners will do well to hold closely to the rule at first (p. 70). Patients can, under certain circumstances, bring about consequences which may prove very distressing for the beginner (p. 78). Many of the matters touched upon in this book are presented as a warning for those who, without a proper calling or sufficient skill, would attempt to occupy themselves with psychoanalysis" (p. 311).

It is within this framework that the book is written. There is but little speculation and much in the way of practical counsel; almost nothing of what one usually expects from a psychoanalyst: no unconscious, no libido, no id, neither is wish-fulfillment mentioned, nor are there instances of the Oedipus-complex; the whole concept of the castration-complex is criticized rather than supported. Instead there is an abundance of advice about how to begin the analytic inquiry, when and how to bring up the question of fees, what to do about members of the patient's family, when to encourage and how to discourage emotional transference to the analyst; finally, how to break off the analysis without upsetting the patient's newly found equilibrium.

Stekel warns his readers against Freud and the "orthodox" psychoanalysis. His criticism rests chiefly in the contention that Freud is essentially unscientific in having ordained fixed rules and methods which it were heresy for his followers to disobey (pp. 7 f. and 312 f.). Occasionally hypnosis is useful, but Freud has banned hypnosis; by no means is the method of free association always best, but Freudians are constrained to employ it in every instance regardless. But the most serious indictment brought by Stekel has to do with the time required for a psychoanalytic treatment: a matter of years in the majority of cases! Stekel maintains that a successful analysis can usually be completed in from two to four months. This is possible, he argues, *only* if one is prepared to substitute for the Freudian method of passivity his own method of active participation in guiding the patient's thought processes. In a word the analyst must possess the special abilities of a detective and of a diplomat. Patients *invariably* seek (even when they disclaim it the loudest) to conceal their real difficulties: to get at the truth one must proceed as a detective would do in ferreting out a criminal and establishing evidence necessary for a conviction. But mere analysis is not enough if one's purpose is to cure the patient. Analysis must be followed by a creative, intuitively guided process of *synthesis,* and it is here that the talents of a diplomat are essential.

It would be impossible in a short review to refer specifically to the many techniques outlined in this book. Almost never should the analyst undertake to treat a patient without an agreement on both sides that a week's trial must come first. Fees must be specified at the very beginning and patients should agree to make weekly payments. Above all the analyst must discover at the earliest possible moment what form of *resistance* the patient is going to put up against a successful outcome of the treatments. Indeed, to this point the author returns again and again: if there is any exceptionless rule in psychotherapy it is, he believes, that one's patients are determined not to be cured. Usually the only way open to the analyst is to secure the emotional transference of his patient to an extent such that the latter will abandon his illness to please (or to spite!) the analyst. Thus transference is regarded by Stekel as of the highest possible importance, for it is only through its agency that resistance can be overcome, and it is only by overcoming resistance that positive results of any value can be achieved.

This is a good book. Clearly and vigorously written, it makes no pretense to being more than it is: a handbook of psychotherapeutic technique from the analytic point of view. Stekel admits that even 'orthodox' psychoanalysis has by now abandoned many fruitless notions; his own contribution is an argument for a revision of viewpoint such that psychoanalysis will disappear in favor of character analysis. This is not the principal theme of the present book, however. Most of its space is devoted to a straightforward and illuminating exposition of technique.

University of Arizona WILLIS D. ELLIS

The Brain and Its Environment. By JOSEPH BARCROFT. New Haven, Yale Univ. Press, 1938. Pp. vii, 117.

This book consists in three lectures given at Yale University on the Dwight Harrington Terry Foundation. The Foundation was established to further the "building of the truths of science and philosophy into the structure of a broadened and purified religion." In following out the details of this bequest, Sir Joseph Barcroft has summarized his own and other relevant studies on the chemical environment of the central nervous system and has amplified in this connection a true understanding of Claude Bernard's now famous phrase, "The constancy of the internal environment is the condition of the free life."

In the first lecture, Sir Joseph Barcroft describes the changes which take place during the fetal period of life in the chemistry of the blood of the developing organism. Here he emphasizes the preparatory nature of fetal development and brings much experimental evidence to bear against "a theory that is often paraded; namely, that the stimulus to growth is use." In connection with oxygen saturation in the blood which feeds the fetal brain, there is a drop in the fetal sheep from about 75% at 110 days to about 49% at 140 days. The former is an oxygen saturation of about that of an acclimatized person at an altitude of 20,000 feet; the latter saturation "brings you somewhere near the stratosphere." The author then well asks: "What can be the activity of the central nervous system under these circumstances?"

A consideration is further given of the type of behavior that can be elicited during various periods of fetal life. In general, the author suggests that the first receptor—central nervous system—effector actions are jerky movements. These then become more limited, and eventually inhibition develops, obscuring all such movements but allowing adaptive behavior. In the late fetus, however, if the chemical environment is changed by an even greater decrease in oxygen saturation, the fetus reverts progressively to earlier forms of fetal activity ending before death in the jerky movements which first characterized fetal response.

In the second lecture, the author points out that at birth many functions of the brain spring into action which before that time have remained latent. He says that this change is due mainly to two things: (1) the change in the stimuli which may act upon the now free organism's sense organs; and (2) the more effective aeration of the blood. The author's own words are "that the foetus of the sheep even up to birth presents few if any signs of consciousness. Within a few minutes after birth, . . . the lamb lifts its head and tries to get up. It is plainly awake, and may be sent to sleep once more either by cold or by reducing the amount of oxygen in the blood. These matters must await further investigation; but it is evident that the higher faculties need a higher tension oxygen supply than those of a more vegetative nature."

The reviewer's experience with fetal mammals of species other than those worked with by Sir Joseph Barcroft does not lead him to the view that in general prenatal life can in any profitable way be considered as "unconscious" as contrasted with a "conscious" postnatal life. This difference of opinion may be due to differences in the use of the word "consciousness" as well as differences in the effective oxygenation of the blood in the types studied by the author and the reviewer.

Finally, in his last lecture, the author turns to a consideration of the mental efficiency correlated with such properties of the blood stream of the brain as reduction or rise in temperature, oxygen want or oxygen excess, carbonic acid deficiency and excess, and finally, blood sugar.

In conclusion, Sir Joseph Barcroft says: "We have therefore arrived at the point where we can say not only that the form of life—man—which has developed an intellectual power far beyond any other, is that in which the constitution of the fluids that bathe his cells is most exactly regulated; but also that the most immediate effect of interference with the chemical or physical properties of the blood is impairment of the higher qualities of the brain."

In the reviewer's opinion, this small book deserves real consideration by psychologists.

Tufts College LEONARD CARMICHAEL

The Psychology of Speech. By JON EISENSON. New York, F. S. Crofts & Co., 1938. Pp. xiv, 280.

The Psychology of Speech is divided into five sections: (1) "The Nature and Origin of Speech;" (2) "Basic Psychological Aspects of Speech;" (3) "The Development of Speech and Language in the Child;" (4) "Personality and Speech;" and (5) "The Psychology of the Audience." The first three sections constitute an integrated whole, since they are based upon common 'principles.' The other two are more or less distinct essays upon their respective topics.

In considering the various theories of the origin of speech, Eisenson inclines toward the 'oral gesture' theory of Paget and earlier writers. The 'ding-dong' theory is regarded as similar to the divine origin theory and as mysterious and absurd. While there is a great deal to recommend the oral gesture notion, this summary treatment of the 'ding-dong' theory is scarcely fair. It seems to the reviewer that modern approaches to language by way of 'phonetic symbolism,' and the 'physiognomic' character of speech sounds are closely related to the 'ding-dong' theory. They have the additional advantage of suggesting *experimental* attacks upon speech. Yet these notions are not mentioned at all by Eisenson, and his only reference is Mark Twain.

In the second section, the relations of speech and emotion, the application of principles of learning to the development of speech, and the psychology of meaning and thought are discussed. In addition, there is a preliminary chapter upon the nervous system which seems out of place since so little of it has to do with mechanisms of speech.

The principles of learning which are believed to cover the development of meaning in words and the development of concepts in general are the familiar 'law of effect,' associative shifting (conditioned response), and associative spread. These, set in a context of stimulus-and-response, explain everything. When we add that the definition of thinking is the familiar 'talking to one's self,' the reader will note the familiar ring which all of this has. There are many objections which may be raised against the simplified picture of learning implied by these principles, and there are many ways of showing the inadequacy of such a superficial definition of thinking, but to present them here would exceed the limits of a review. We can only suggest that Eisenson's book would have been more useful if he had not accepted his 'principles' so uncritically.

The succeeding section on the development of speech in the child constitutes an excellent and brief summary of our present knowledge on the descriptive side of speech development. It is only when Eisenson attempts to *explain* the genesis on speech in terms of his principles of learning that the section becomes unsatisfactory. Again the explanation seems superficial and oversimplified.

We pointed out above that the last two sections are quite distinct from the first three and from one another. The disorders of speech are considered from the point of view of personality and its disorders rather than in terms of the principles of the earlier sections. Speech is here regarded as an expression of the total personality, and speech disorders as indicators of maladjustment of the personality. As a result of this logic, Eisenson considers not only the usual aphasic disorders and stammering, but also speech characteristics of the blind and of organisms suffering from more general psychological disorders such as schizophrenia and manic-depressive psychoses.

The final section on the "Psychology of the Audience" is a résumé of the well known facts and theories of the psychology of groups and audiences, and of the factors in gaining and holding the attention of listeners. The only difficulty which we may find here is that no experimental evidence concerning the *specific* application of the general facts to the speech situation is presented. For example, it is simple enough to say that change is one of the factors which is likely to secure the attention of an organism, but this does not tell us just what sorts of change or how much change is useful in addressing an audience. Eisenson does not see that these latter questions can be answered only by experimentation. Instead, he contents himself with casual observation and logical deduction from the general facts.

If called upon for an 'absolute judgment' of this book we should say that it is neither an adequate descriptive or 'theoretical' account of speech, nor a useful or practical handbook. It does, however, represent adequately the status of the field at the present time, although it contributes little that is new.

Cornell University T. A. RYAN

A Source Book of Gestalt Psychology. By W. D. ELLIS, with an introduction by Kurt Koffka. New York, Harcourt, Brace & Co., 1938, Pp. xiv, 413.

In preparing this source book Professor Ellis has answered a need which has been felt keenly by many teachers of experimental and systematic psychology, and in doing so he has set a standard of excellence which merits unqualified praise. For altogether too long the German language and the relative inaccessibility of many German publications have virtually excluded from consideration by American psychologists the results of some of the most important German research. The *Psychologische Forschung,* for instance, has a lamentably small circulation in this country; Bethe's *Handbuch der normalen und pathologischen Physiologie* is prohibitively expensive; and such a journal as *Melos,* in which Von Hornbostel's classic paper on the unity of the senses appeared, seldom finds its way into a psychological library. Yet publications such as these contain some of the finest fruits of German psychological research. Although the current abstracts of German articles are useful as guides to reading, they cannot because of their brevity serve as substitutes for the original material; and writers of textbooks, on the other hand, are bound to select and evaluate as they report. Professor Ellis has attempted to steer a middle course between abstracting and interpreting. The book contains digests of 35 significant contributions to German psychology, each reduced without change in topical order and without editorial interpretation to approximately one tenth of its original length. A system of marginal page references indicates the exact amount of condensation, and enables the reader to find without difficulty the corresponding discussion in the original text. In keeping with the experimental character of the selections, the figures and tables have for the most part been retained, and extended historical and theoretical discussions have been either greatly reduced in length or eliminated altogether.

As the title of the book indicates, Professor Ellis has drawn his selections from the literature of Gestalt psychology. The wisdom of such a plan is obvious, for it has served at the same time to give coherence to the whole work and to present Gestalt psychology as a developing experimental movement. To those who still harbor the delusion that Gestalt psychology is primarily speculative, the book should bring a flood of illumination. Of the 35 contributions summarized, 27 deal with special problems of perception, motivation and thinking, in which actual experiments are reported, and the remaining 8 include summaries of Köhler's book on physical gestalten and Wertheimer's lecture before the Kant Society in Berlin.

It is inevitable that some readers will disagree with the selection of articles and with the treatment of those which have been selected. It is regrettable, for instance, that nothing published after 1929 could be included, in consequence of which there is no discussion of the recent work on memory-traces; that Köhler's important article, "Zum Problem der Regulation," had to be omitted; that more of Lewin's experiments could not have been reported. To make such demands, however, would be to ask for a book of unwieldy size, and even then all readers would not be satisfied. The book should be evaluated on the basis of its contents rather than of its omissions. The reviewer is satisfied that every contribution included is of lasting significance. He has, furthermore, compared several of the summaries in detail with the German originals, and has been deeply impressed by the skill with which Professor Ellis has caught and reproduced the spirit of the original articles without permitting himself to slip into the cumbersome style of a literal translator. The book should prove of great value both to teacher and to student. It is to be hoped, too, that it will stimulate other scholars to undertake similar tasks in other fields of psychology.

Swarthmore College R. B. MacLeod

Psychology and Life. By FLOYD L. RUCH. Chicago, Scott, Foresman & Co., 1937. Pp. xiv, 679.

Working with Psychology; A guidebook for use with *Psychology and Life* and other introductory texts. By FLOYD L. RUCH and NEIL WARREN. Chicago, Scott, Foresman & Co., 1938. Pp. ix, 214.

The number of recent texts in elementary psychology suggests, among other things, that there is rather general discontent with the first course in psychology. *Psychology and Life* attempts to remove these sources of discontent. Early in the book personality and intelligence are rather fully treated, and then the author gradually leads up to the more traditional subject-matter.

It is the reviewer's experience that this sequence of topics serves to arouse the student's interest in the chapters on sensory phenomena, attending, perceiving, learning and thinking. This heightened interest, not commonly noticed when other texts following a more traditional pattern are used, may be attributed to the fact that the student comes to appreciate the ephemeral state of our knowledge concerning personality. Consequently he accepts, and even desires, more substantial subject-matter.

Ruch's book is almost completely psychological in content. It contains no chapter on the nervous system, although it does refer to neural and humoral mechanisms when such references are pertinent. The most significant virtue of this book is its predominating emphasis upon information which can function in the lives of those who study it. The following citations from the subject index are typical: automobile accidents, Communism, 'crushes,' friendship, homosexuality, strikes, and marriage. Important psychological principles emerge from homely example and laboratory experiment. The book, however, makes no concessions to scientific rigor and caution.

Some will question how well Ruch has executed his announced program. Although the reviewer believes he has succeeded reasonably well, he will be disappointed if future textbook writers building upon the foundation laid down by Ruch do not erect a more finished structure. Many improvements might be suggested. The reviewer is especially disturbed by the choice of pictures which introduce the several sections. Many of them are inconsequential, adding little to the value of the book. The chapter on individual differences should be omitted since this topic would have more meaning for the student if it emerged from a discussion of personality and intelligence. The chapter on the origin of individual differences might better follow that on intelligence and discuss the heredity-environment issue alone, leaving maturation for consideration with learning.

The Guidebook is a valuable pedagogical device. The group exercises are especially well worked out and most of them can be performed outside of class. The self-tests have the defect of all self-tests in that they encourage parrot learning.

Western Reserve University CALVIN S. HALL

The Pituitary Gland. Edited by WALTER TIMME, ANGUS M. FRANTZ, and CLARENCE C. HARE. Vol. 17 of a Series of Publications by the Association for Research in Nervous and Mental Disease. Baltimore, Williams & Wilkins Co., 1938. Pp. xxiv, 764.

This handbook summarizes and evaluates the present status of research on the pituitary gland. The first section, dealing with the anatomy of the gland, has contributions by Tilner; Wislocki; Severinghaus; Rasmussen; and Rioch. The second section, dealing with physiology, has contributions by Evans; Lee; Mortimer; Selye; Hisaw, Fevold and Greep; Thomson; Collip; Long; Riddle and Bates; Engle; Kurzrok; Kurzrok and Smith; Flohman; Bissonnette; Atwell; Richter; Ranson, Fisher and Ingram; Gersh; Geiling and Robbins; Lewis; Sittenfield; and Perla. The third section, entitled *General Considerations,* has chapter by Timme; Rubinstein; Goldzieher; Lurie; Richter and Eckert; Pardee; Herrick; Stockard; Beck; Hare; Horrax; Shelton; McKinney and Pardee; and Putnam and Davidoff.

The psychologist will be well rewarded by reading this whole book, but naturally he will be most interested professionally in certain chapters. Among these is Engle's treatment of the relation of the anterior pituitary gland to the physiological problems of puberty with special reference to the development of the male and female genital system at this period. The author emphasizes the importance of allowing the adolescent organism to achieve homeostatic stability by itself without the expression of anxiety or the use of premature therapy. Kurzrok's and Smith's treatment of the menopause will also be of interest to those who are concerned with the personality changes which occur at this time of life.

The chapter by Bissonnette on the influence of light upon pituitary activity is of direct concern to all students of behavior. This investigator here reviews in brief form the experimental work which shows that in certain animals, the sexual cycle is, in part at least, a function of the time and character of light stimuli present in the environment of the animal tested. Daily period, intensity, and even the wave length of the stimulating light are important. The eyes are the normal receptors for this stimulus, but in the duck and possibly some other animals, the pituitary gland itself may be stimulated directly by light falling upon it, thus directly modifying the sex cycle.

Richter's study of the pituitary gland in relation to water exchange is important in its own right and because of the ingenious experimental procedure used with his trial rats. Richter and Eckert also report that hypophysectomized rats are very inactive, show a reduced interest in food and water, and no interest in sex activity but that they build very large nests.

Rubinstein's interesting chapter on brain and body weight leads to the conclusion that the differences in brain weight between the sexes is independent of the influence of sex hormones.

Reading this book gives the psychologist many suggestions concerning some of the most complex and obscure factors which underlie human personality differences. The comparative psychologist will also be struck with the large amount of significant controlled behavior study which must sometime be undertaken in this field, especially in connection with the mechanisms of behavior activity which are rhythmic.

Tufts College LEONARD CARMICHAEL

Introductory Child Psychology. By WILLIAM A. KELLY and MARGARET REUTHER KELLY. Under the general editorship of Joseph Husslein, S.J., Milwaukee, The Bruce Publishing Co., 1938. Pp. xi, 413.

This book, according to the authors' foreword, is intended primarily for use in Catholic colleges and teacher-training institutions, as well as for Catholic parents. Thus we observe that "Catholic Philosophy has been appealed to throughout the volume in order to interpret the findings of modern psychology in the light of Catholic faith and tradition. The substantial foundation of the book is the principle that the child is a compound of body and soul, endowed with a free will, responsible for his actions, destined for eternal life. The spiritual, volitional, and moral training of the child has been emphasized thoroughly as the most important part of the parents' and teachers' task in guiding and directing the child."

The book is divided into four parts: Part I, "The Bases of Growth and Development," considers such topics as the nature and scope of child psychology, growth and development, the physical bases of growth and development, the factors of mental growth and development, the dynamic factors of growth and development, and the social bases of growth and development. Part II, "Periods of Growth and Development," offers a descriptive account of the different periods of child development: the period of infancy, the period of early childhood or the preschool age, the period of childhood proper, later childhood, the period of preadolescence or the gang age, and the period of adolescence. "The Exceptional Child" is treated in Part III, and the two chapters deal, respectively, with the physically handicapped and the mentally different and the socially maladjusted. The central viewpoint of the book comes to a focus especially in Part IV on "The Social and Moral Guidance of the Child." Three chapters here deal with mental hygiene of childhood and youth, training in purity, and the formation of moral character.

The descriptive portions of the book follow the lines of current psychological orthodoxy; the interpretative parts, Catholic orthodoxy. Psychology, in general, is considered as a branch of philosophy, which "treats of the soul, its nature, powers, and manifestations. The *soul* is the substantial form of the body, the principle of life and of thought. . . . The term *mind* is used to designate the vital principle as the subject of mental activity, the principle by which the human being knows, wills, and feels." The facultative conception of mind is implicit, if not explicit, throughout the book. For example, memory is regarded as a power of mind, capable of storing up representations of past experiences and reproducing them later. The discussion of the topic of instinct is uncritical, and gives no evidence of recent evaluations of this concept. Respect for law, for the rights of others, honest, duty, reverence for purity, truth, and right, are considered as instincts.

An appendix of thirty-one pages contains "The Children's Charter" and exercises and problems for special study. The exercises are well conceived and should serve as helpful aids to the student.

Franklin and Marshall College PAUL L. WHITELY

The Science of Human Behavior. By WALLACE T. WAIT. New York, Ronald Press Co., 1938. Pp. xv, 335.

This is another addition to the list of general textbooks. Designed for students at the freshman level, it incorporates many of the advantages which characterize texts of the sugar-coated variety. It is well-written, neatly primed with what might be termed "student interest," and employs many of the more fashionable educational devices such as chapter outlines and a really well designed series of "problems for further thought."

Unfortunately, however, the disadvantages are also present. The author, in what was probably an honest attempt to avoid indoctrination and to arrive at an "healthy eclecticism," has allowed his book to suffer from a lack of organization and internal consistency. Thus, after two chapters in which structuration is upheld as one of the major determinants of behavior and a definite attempt is made to convince the student of a psychophysical monism, the structure of the human organism, save for a lengthy chapter on the mechanisms of heredity, is almost completely ignored. The other major determinant is identified as the effect of environment upon the inherent structure but stimulation is never discussed and learning is relegated to a chapter of some seventeen pages. The topic of conditioning is exhausted in two pages while an admittedly unsatisfactory account of the Gestalt interpretation claims five. The student must therefore find himself in a somewhat unfortunate position. He is told what to call the principles of explanation to be employed but is forced to remain quite ignorant of their nature.

This major defect in the theoretical background leads to a number of minor inconsistencies, misinterpretations, and major omissions. Important questions which might be expected to give the student or the instructor some difficulty are sacrified for the more sensational, easy ones. Where difficult problems are discussed they are often disposed of by the device of "explanations" which are more facile than useful, more popular than thoughtful.

In short, the sugar coat while pleasant is too thick. It is so thick, indeed, that one suspects the thinking student will be inclined to wonder why the word science appears in the title at all.

Tulane University S. RAINS WALLACE

The Problems of Education: A First Course for the Orientation of Prospective Teachers. By C. C. CRAWFORD, L. P. THORPE, and FAY ADAMS. Los Angeles, Southern California School Book Depository, 1938. Pp. 239.

This is not an ordinary textbook. Rather it is an extended outline for a course of study. It consists of 207 topics, each occupying a page of the book, for which Issues, Activities, and Bibliographies are given. Under Bibliography the student is told under what headings the topic is listed in *Education Index* and *Educational Abstracts.* Periodicals dealing with the topic are next listed. Finally selections from the reserve shelf, consisting of twenty-nine carefully selected references, and two or three important books which deal with the topic are cited. The issues for each problem are clearly and succinctly stated. The activities comprise such things as debates, visits to schools, reports, and collecting data which bear upon the particular topic under discussion. Pedagogically the scheme is excellent. It follows a pragmatic philosophy faithfully. The reviewer was puzzled, however, as he read it. How could prospective teachers cover the course in a year or even in six years? Each topic with its wealth of reference material and suggestions for activities on the part of the students, seemed to contain sufficient material for at least a semester. Any student who was a member of a class that faithfully carried out all the instructions would find himself on the completion of the course at the M.A. or Ph.D. level of knowledge. Take, for example, topic 117, Connectionist (Thorndike) Psychology. The student besides looking up the appropriate material in *Education Index* and *Educational Abstracts* is requested to consult the *American Journal of Psychology,* the *Journal of Educational Psychology,* and *Psychological Review,* and to read specified sections from Caswell, Frasier, Myers, Strayer, Thomas, and Thorndike. In addition, he is expected to read or at least to consult Gates, *Psychology for Students of Education* and Thorndike's, *Fundamentals of Learning.* Now Thorndike's *Fundamentals of Learning* proves stiff reading for most graduate students, yet it is given as a reference for a first course of prospective teachers! Either a limited selection from these 207 topics must be made by the teacher, or the whole of them must be treated rather superficially.

As a mine of information for the teacher or the advanced student, the book is excellent, but in the opinion of the reviewer it will prove too difficult for most undergraduate groups unless the teacher is superbly good at his job.

For the psychologist the interesting feature of the book is the emphasis placed upon organismic and Gestalt psychology. For the teacher the stress upon the progressive point of view will prove to be the most interesting. Finally, it should be stated that the production is both workmanlike and consistent. It will probably be more useful for teachers and advanced students than for the group for which it has been specifically designed.

University of Toronto PETER SANDIFORD

Emotion and the Educative Process. By DANIEL A. PRESCOTT. Washington, D.C., American Council on Education, 1938. Pp. xviii, 323.

This is a report to the American Council on Education by its Committee on the Relation of Emotion to the Educative Process. It is a survey of research bearing directly and indirectly on the part played in education by all affective experiences, including feelings, emotions, and all attitudes having affective and emotional components. Prescott, chairman of the committee, wrote this report and selected the materials to be included supplied by seven members of the committee. The report is the outcome of four years of work, consisting for the most part of surveys of relevant literature and research, supplemented by Dr. Prescott's personal tour of various universities, clinics, and research centers. Plans for further research are to be made for areas of research which the present report shows to be undeveloped.

The findings of the survey are given in 10 successive chapters (II to XI), followed by a chapter of general conclusions. The chapter titles may serve as brief indications of the materials covered: II, Basic Affective Phenomena; III. Physiological Basis of Affective Experience and Behavior; IV, Patterning and Trainability of Affective Behavior; V, Affective Maturity; VI, Basic Personality Needs and Conditions which Frustrate Them; VII, Affective Behavior and Contemporary Social Institutions and Processes; VIII, The Influence of Affective Factors Upon Learning; IX, Affect and Education; X, Aspects of Education Needing Study; XI, Personnel Problems in Education.

Each chapter gives in survey fashion the basic theories, experimental methods and results of the references covered. Although these surveys are necessarily incomplete, a full citation of sources makes it possible for the reader to seek further information. The final summary chapter consists of a series of sections each one serving as a critical estimate of the present status of some special area of research, with an indication of the lines which further research should follow. The author has a clear realization of the complexity of the problems encountered, and emphasizes the need of pooling the knowledge and skills of a large group of experts in various fields of science and practice. Indeed the practical bearings of emotion on the educative process must wait for years of extensive research into fundamentals.

Wells College C. O. WEBER

An Introduction to the Fields of Psychology. By EMILY S. DEXTER and KATHARINE T. OMWAKE. New York, Prentice-Hall, 1938. Pp. 236.

The present volume is intended as a second book for elementary students. To introduce them into the various types of psychology, rather than the whole field of psychology in general, it should be used in an introductory course. The volume is simply written and easily read and it might well serve to be a text for elementary psychology provided a few introductory lectures are given. The chapter headings, with the exception of those dealing with medicine, law, efficiency, and advertising cover topics which easily constitute single courses in a good department of psychology; namely, abnormal psychology, social psychology, child, animal, measurement in psychology, and experiment in psychology. Since each of these chapters is seldom more than fifteen pages long, it is easy to conceive that the subject can scarcely be covered in much detail, in fact, so little of the vast amount of material that can be covered in individual courses dealing with these subjects is presented that it is somewhat questionable whether each chapter by itself would be stimulating to the student who might wish to go on into one of these more specialized courses. The information, as given, is conventional. There is little that is controversial but also little that is highly modern; for instance, in abnormal psychology a very brief list of disorders is given, each one being amplified by only a short paragraph. The idea of dynamisms should not be touched upon in such a brief space but is. Similar criticisms might be made of each individual chapter but if this book is to be used in a course, which appears early enough in the college student's curriculum, it should be with amplification by the instructor to provide a means of revelation to the student that there is more to psychology than one finds in the usual beginners' course. It is systematic, simple, and satisfactory so far as it goes.

Recorder's Court, Detroit, Michigan LOWELL S. SELLING

Shamanism in Western North America: A Study in Cultural Relationships. By W. Z. PARK, Evanston and Chicago, Northwestern University, 1938. Pp. viii, 166.

The author's problem and conclusions will be of interest chiefly to specialists in American Indian ethnology. His goal is to ascertain interrelationships of tribal cultures reflected in the geographical distribution of elements in the shamanism of the Paviotso, or Northern Paiute of western Nevada. After describing Paviotso shamanism in considerable detail, he traces the distribution of similar beliefs and practices among neighboring tribes of the Great Basin, the Plateau, California, etc. Raw data of interest to some psychologists may be found in detailed accounts of dreams reported by some of his Indian informants. Shamanistic powers among the Paviotso are always received through dreams. Illnesses are also thought to be caused by dreams. In curing such cases, the shaman requires his patient to recall the content of the dream and the time it occurred.

West Springfield, Mass. MABEL F. MARTIN

A Simplified Guide to Statistics for Students of Psychology and Education. By G. M. SMITH. New York, Farrar & Rinehart, 1938. Pp. ix, 70.

Satisfactory interpretation of much of the material in psychology and educational psychology texts requires a working knowledge of elementary statistics. In the laboratory courses in these fields such knowledge is indispensable. The general practice is to introduce into the texts, either as separate chapters or as part of the discussion at appropriate places, a minimum of statistical explanations. Frequently the limitations of space prevent such material from being adequate.

This simplified guide, while not pretending to serve as a text in statistics, "is intended to clarify and supplement the work in general or laboratory courses." The material covered includes a consideration of distributions, measures of central tendency and deviation, the use of norms and standard scores, reliability of the mean and of the differences between means, and correlation techniques and their uses.

At times there is an obvious tendency to talk down to the students. In a few instances the discussion is not complete enough to be clear to the elementary student. An example is the interpretation of correlation coefficients, especially the reliability of test scores. The book would be more complete if representative samples were described and tables for PE_Q and PE_r were included.

As a whole this simplified guide to statistics is excellent. The material is well chosen and clearly discussed. It will undoubtedly receive a hearty welcome from many instructors who have long felt a need for this sort of treatise.

University of Minnesota MILES A. TINKER

Progressive Relaxation. By EDMUND JACOBSON. 2nd. edition. Chicago, Univ. of Chicago Press, 1938. Pp. xvi, 494.

The first edition of this book was reviewed at length in this JOURNAL (42, 1930, 473). The new material in the second edition consists principally of three chapters. The first discusses the measurement, by means of string galvanometer and amplification, of minute potentials due to slight muscular contractions. These measurements were needed particularly to serve as an objective check upon the results of training in relaxation. The records lead the author to give up the traditional view "that healthy muscles, even in the resting state, are always in a state of slight sustained contraction, commonly called 'tonus'." They also show clearly that relaxation can be cultivated by training. In the second new chapter is presented the author's well known work on the connection between imaginal and other mental processes and the activity of specific muscle groups. The third new chapter, mainly of clinical interest, is on the cultivation of quick relaxation and of slow progressive relaxation. Dr. Jacobson's contributions are of both theoretical and practical importance; they have helped solve certain problems on the relation between mental and muscular activity, and in addition they appear to offer therapeutic aid in the treatment of certain disorders involving "neuromuscular hypertension," which include spasticity of parts of the alimentary tract, mucous colitis, certain psychoneurotic conditions, and other troubles.

The Rice Institute FRANK A. PATTIE, JR.

The Development of Linguistic Skill in Twins, Singletons, and Only Children from Five to Ten Years. By EDITH A. DAVIS. Minneapolis, Univ. Minnesota Press, 1937. Pp. ix, 165.

Like Piaget, Mrs. Davis has emphasized the symbolic content, rather than the mechanical aspects of speech development. In addition, however, she has contributed a more objective technique, a more controlled selection of subjects, and a statistical analysis of her protocol data. Her subjects, aged 5½ to 9½ yr., included groups from six socio-economic levels, carefully equated in sex, intelligence, birth order, etc. In a "free" play situation involving specific materials, each child was told to explain or describe to the experimenter what was going on in the game, and fifty remarks were analyzed as to articulation, vocabulary, sentence structure, function (*i.e.* monologue, conversation, questions, answers, etc.; naming, comparing, describing, etc.). Combining all phases, Mrs. Davis found that only children were consistently superior; singletons and siblings were next in order; and twins consistently last in language development. No relation to bilingual environment was discovered. Girls were superior to boys, especially at the earlier age-levels, but the greatest differences coincided with differences in socio-economic status.

In both structural and functional aspects her data confirm the 'rule' that normal speech development is toward better articulation, longer and more complex sentences, larger vocabulary, and precision of symbolic content.

University of Michigan MARTHA GUERNSEY COLBY

Philosophie und Pädagogik Paul Häberlin's in ihren Wandlungen. By PETER KAMM. München, Reinhardt, 1938. Pp. 479.

Häberlin is a contemporary German philosopher. As such he regards himself although he is a Swiss citizen, born in 1878 in Kanton Thurgau, and at present professor in the University of Basel. Kamm, a student of Häberlin, has undertaken to describe the development in four stages of his teacher's philosophical views. He does this to a large extent by quoting from the published works (94 in number) among them two dozen books, some published in revised editions years after their first publication. From the author's representation, Häberlin appears as a clear and modern thinker, devoted to all branches of philosophy, religion, ethics, esthetics, metaphysics, and pedagogical speculation. Those interested in acquainting themselves with the several aspects of his philosophy will be greatly aided by Kamm's elaborate index of nearly twenty pages in finding within the book the commentaries to the quotations and the references to Häberlin's works, which constitute the author's labor.

Coral Gables, Florida MAX F. MEYER

Fundamentals of Educational Psychology. By W. B. DOUGLAS and B. F. HOLLAND. New York, Macmillan Company, 1938. Pp. xv, 598.

This is but one of the many introductory texts on educational psychology that have appeared during the last five years. It is very elementary and therefore might prove of interest to the teacher having a class with a limited background in psychology.

The text is divided as follows: Part I, a brief introduction of definitions and a discussion of scientific method; Part II, Environment and Heredity (taking up about a sixth of the text); Part III, on learning makes up about half the text, thus appealing to the instructor wishing to place a large portion of the class time in the field of learning; and Part IV, on Measurements in Educational Psychology takes up a generous fourth of the book.

University of Pittsburgh W. T. ROOT

The Philosophy of the Act. By GEORGE HERBERT MEAD. Chicago, Univ. of Chicago Press, 1938. Pp. lxxxiv, 696.

This is the third and last volume embodying the unpublished writings of a great American teacher and philosopher. The previous volumes were *Mind, Self and Society* and *Movements of Thought in the Nineteenth Century.* The present volume contains only fragmentary notes, mainly on philosophical matters, and adds little to the elucidation of Mead's position which was developed in a more systematic fashion in the two other posthumous volumes. An excellent introduction by the editors gives a detailed and definitive analysis of Mead's point of view and his contributions.

Columbia University THEODORE ABEL

The Real Use of the Unconscious. By CHANDLER BENNITT. New York, Dial Press, 1937. Pp. vii, 380.

This potpourri of Psychoanalysis, Theosophy, Yoga and Christian Science has for its themes: (1) that the conscious mind is factual; (2) that the unconscious is symbolic; and (3) that man's ultimate salvation lies in a fusion of the factual and symbolic. The book is worthless from a scientific point of view except as an exhibit of irrational writing and conclusions that are *non sequitur.*

Occidental College GILBERT BRIGHOUSE

The Psychologist at Work: An introduction to Experimental Psychology. By M. R. HARROWER. Preface by K. Koffka. New York and London, Harper & Brothers, 1937. Pp. xi, 184.

This book was written with the avowed purpose of giving the layman some appreciation of what psychologists do—their problems, methods of attack, and modes of solution. It was evidently not written to entertain the reader, or to popularize the subject in the usual sense of the term. Rather it is addressed to the serious minded and intelligent layman who wishes to obtain some knowledge of what academic psychology is about. Naturally a short book best serves this purpose, and necessarily the number of topics selected for discussion is limited. The author recognizes that the selection of topics and their mode of treatment have been largely dictated by her predilection for the Gestalt point of view. Evaluated from this purpose and point of view, the book can be commended for its selection of topics, and especially for the directness, simplicity, and clarity of its exposition. Naturally the book is of no great interest to the professional psychologist, with the exception that it might well be used as supplementary reading in introductory courses.

University of Chicago HARVEY A. CARR

Statistics in Education and Psychology. By ELMER R. ENLOW. New York, Prentice-Hall, 1937. Pp. ix, 180.

In this compact little volume the author has essayed the difficult task of combining a handbook and a textbook. He has succeeded rather better in his selection of useful formulas and tables than he has in writing a teachable text for the novice. The usual fundamentals, the frequency distribution, measures of central tendency and dispersion, percentiles, correlation techniques (first order, partial, and multiple), and errors of sampling and measurement, are systematically treated, wherever possible, under the headings: Definition, Interpretation and Use, Formulas, Derivation of Formulas, Mathematical Properties, and Methods of Calculation.

The interpretations are not always helpful to the beginner; the derivations are frequently casual and the mathematical properties are often gratuitous. The style is not facile, and compactness has often been achieved at the expense of lucidity.

The appendix contains a selected bibliography, a glossary of symbols, and seven mathematical tables: tables of squares, square roots, reciprocals, and common logarithms for numbers from 1 to 1000; alienation coefficients and values of $1 - r^2$; and various combinations of areas, ordinates, and abscissas under the normal curve.

The tables are of distinct value to the statistical field worker, but are not adequately explained for the uninitiated or linked to the text.

College of the City of New York G. MILTON SMITH

Knowledge and Society: A Philosophical Approach to Modern Civilization. By the University of California Associates. Pp. ix, 417. New York, Appleton-Century Company, 1938.

The title of this work might have been somewhat more fittingly "Philosophy and Society." It is not an analysis of the social order, still less is it one of those studies to which the Germans give the name of *Wissenssoziologie.* It is an introduction to philosophy, an introduction in the proper sense—not a history of philosophy or a review of philosophical systems, but an exposition of the philosophical approach, bringing out its bearing on modern social and political issues. As such, it is lucidly and coherently written, remarkably unified for a work of composite authorship. It is the product of seven members of the Department of Philosophy of the University of California. It fulfills admirably its purpose as stated by the general editor of the Century Philosophy Series, as "an aid to any teacher whose major concern is to cultivate in his students the habit and enjoyment of reflection." But a book of this character might well serve another function. It can certainly be recommended to those sociologists and psychologists who without any real philosophical training commit themselves to philosophical positions, often without being aware that they are doing it.

Columbia University ROBERT M. MACIVER

The Handicap of Deafness. By IRENE R. EWING and ALEX. W. G. EWING. New York, Longmans, Green and Company, 1938. Pp. xi, 327.

This book deals with the scientific study of the social and educational effects of deafness; the measurement and assessment of any remaining power to hear, and the development, through training and skill, of its use; the value of lip-reading and methods of learning to lip-read; the efficiency, characteristics and use of hearing-aids of various types and their suitability for patients suffering from different forms and degrees of deafness; the principles which underlie the education of totally, severely and partially-deaf children.

The book is scientifically sound, pedagogically wise, well illustrated, and socially helpful. It is by all odds the best book now available for its purpose. And it comes at the psychological moment when the invention of audiometers and introduction of hearing aids has done for the ear what the fitting of glasses did for the eye ages ago. This book should be in the hands of every superintendent and principal of schools and parents who are concerned about the hearing of their children. It should be sold at the news stands because it contains news and aids for young and old who suffer from loss of hearing. I have only one criticism, and that is of the price and would suggest to the publishers that if they cut the price in two they would make an excellent investment.

University of Iowa C. E. SEASHORE

Le pithécanthrope était-il un pygmée? By PAUL BUYSSENS. Brussels, Purnal, 1937. Pp. 48.

As indicated by the subtitle, "Considérations sur la généalogie humaine, inspirées par les recherches recentes sur l'ancienneté at les caractères de certains hommes fossiles," Buyssens presents, in this little essay, the arguments in support of the thesis that the Pygmy was the first man. To the author there are two distinct lines of human evolution. Pithecanthropus is distinct from the Pygmies who were descended from him and who in the long course of evolutionary development produced all other human forms. Anatomical variation and wide dispersion of Pygmy populations are, in part, evidence cited in support of the main thesis.

The author weighs the chronological and filial relations of the Pithecanthrope, Sinanthrope, Solo (Java) man and the Australian and Rhodesian prehumans and decides that, despite cranial similarities and geological classifications as to antiquity, human phylogenesis is more complex and less certain than has been admitted.

This essay is intended to further clarify and extend the author's views on the problem of the Pygmy treated in his book *Les trois races de l'Europe et du monde* (1936).

Ohio State University SAMUEL RENSHAW

An Introduction to Child Study. By RUTH STRANG. Revised edition. New York, Macmillan Co., 1938. Pp. xv, 681.

Since its appearance in 1930, this book, which the author states is an "applied psychology of childhood . . . (with) emphasis . . . on the service aspect rather than on research methods," has been thoroughly revised and greatly enlarged. Its organization has not, however, been changed. It contains twenty-two chapters which are divided into six sections, as follows: Section I, infancy (including hereditary and prenatal influences); II, early preschool; III, late preschool; IV, primary school; V, preadolescence; and VI, adolescence. Every section includes a survey of growth and development, a study of how children learn, a discussion of special problems with suggestions for dealing with them, and an account of the techniques for observing and studying the child's behavior.

Dr. Strang's book is particularly well suited for parents and students of education. Her treatment of growth and intelligence testing are excellent, as are also the many suggestions for dealing with 'problem behavior' such as lying and cheating.

An appendix gives height and weight norms and lists of play materials. Its indices, subject matter and author, are excellent.

Sarah Lawrence College LAWRENCE JOSEPH STONE

The Psychology of Human Conflict. By EDWIN H. GUTHRIE. New York, Harper & Brothers, 1938. Pp. ix, 408.

Of the many volumes which have been published upon 'human conflict,' Dr. Guthrie's is, in the opinion of the reviewer, the best. By taking account of the results of recent researches in this field, the author avoids many of the pitfalls into which the earlier authors have fallen.

Dr. Guthrie's account proceeds from a homeostatic definition of life as 'the preservation of identity through the continued maintenance of essential constant states.' After a discussion of motives, desires, and interests, the principle of conflict, which is based on inhibition, is applied to the functional disorders. A notable aspect of the treatment is the author's insistence upon the rôle of the body in maladjustment and disorder.

The outstanding defect of the book is the imbalance between the first twenty-one chapters and the last seven. The tone of the book and the character of its contribution tails off in these closing chapters.

In addition to a clear and charming style of exposition, the author possesses a sound grasp of his subject matter. His verbal illustrations and analogies are well-chosen and his exposition is interesting throughout the book.

Cornell University ROBERT M. LINDNER

Psychology in Everyday Life. By WALTER C. VARNUM. New York, McGraw-Hill Book Co., 1938. Pp. x, 444.

This book is designed as a text for non-professional students of psychology. It is popularly written and is replete with illustrations many of which are not explained or referred to in the text (*e.g.,* p. 32). Varnum stresses the practical. Difficulties are glossed over and conflicting theories and opposing points of view are avoided.

The author attempts in this book to make psychology interesting and easy and he achieves some success in both goals but at the cost of a distorted and false view. The exposition depends largely upon secondary sources and the recommendations for further reading are chiefly to popular books.

It is unfortunate that such a text should be placed in the hands of elementary students.

University of Newark FREDERICK J. GAUDET

Physiological Optics. By VERNON W. GRANT. Chicago, The Professional Press, 1938. Pp. 240.

This is a summary from a popular point of view of some of the facts of vision and related fields. Two chapters are devoted to the problem of reaction and the anatomical and physiological substrate of responses and then one each to the visual reaction system, visual perception, visual perception of space, attention in vision, visual sensations and illusions of vision.

The material is abstracted from well-known current works. There is nothing about its content or method of presentation to make it noteworthy. The order is not satisfactory in many cases, and the statements are not always clear. It will probably interest the general reader but will find little place with university classes.

University of Michigan W. B. PILLSBURY

The Kelley Statistical Tables. By TRUMAN LEE KELLEY. New York, Macmillan Co., 1938. Pp. 136.

The author's apology for these very complete tables is that years of experience have convinced him of the universal usefulness of the normal distribution curve in psychology. Table I gives values of x (deviation score), z (ordinate), $(pq)^{1/2}$, $(1-p^2)^{1/2}$, and $(1-q^2)^{1/2}$, to eight decimal places corresponding to areas under the normal curve (p), for values of p varying from 0.5000 to 0.9999 in steps of 0.0001, and from 0.9999 to 0.9999 9999 9 in smaller steps. The total area is unity. The inclusion of the radical values yields constants useful in correlational problems; namely, $(1-r^2)^{1/2}$ and $(1-k^2)^{1/2}$, for various values of r.

Table II gives values for Pearson's coefficient P, for values of $\chi/n^{1/2}$ and χ^2/n, and for various degrees of freedom. Tables III, IV, and V give cubic, quintic, and septic interpolation coefficients, respectively, for values of p from 0.500 to 1.000. Table VI gives square roots of numbers from 1 to 1000, to six decimal places.

Table I comprises the bulk of the volume. In view of the insufficiency of all older tables of this type and the increasing call for the derivation of abscissa values from areal observations, statistical psychologists will welcome these new extensive tables. Apparently all possible precautions have been taken to insure accuracy in computing the tabular values.

University of Nebraska J. P. GUILFORD

Teachers and Behavior Problems. By E. K. WICKMAN, New York, The Commonwealth Fund, 1938. Pp. 40.

This is a digest and condensation of the well-known study of *Children's Behavior and Teacher's Attitudes* carried out by Wickman and published by the Commonwealth Fund ten years ago (1928).

The account of the methods of the original investigation and the detailed results are omitted. A condensation of the results and their implications for education comprise the essential portions of the pamphlet. It should be useful to teachers and educators who are not interested in the details of such studies but only their application to educational problems.

University of North Dakota C. W. TELFORD

A Preview to College and Life. By CARL E. SEASHORE. Iowa City, Iowa, University of Iowa Press, Studies on Aims and Progress of Research, No. 55, 1938. Pp. 78.

This monograph contains a series of open letters to undergraduate and graduate students setting forth personal questions referent to certain problems in educational orientation. The author does not attempt to offer exhaustive answers to the questions proposed; he limits his treatment to a discussion of some of the more significant factors. The student looking for inspirational advice may gain from the discussion, but it is of no direct interest to the psychologist.

University of Maryland LESTER P. GUEST

The Psychology of a Growing Personality. By F. M. GREGG. Lincoln, Nebraska, The Personality Press, 1938. Pp. xv, 489.

This book contains a mixture of uncritical eclecticism, strained physiologizing, sentimental pap, cheap moralizing, and practical common sense. It is intended for character educators and for the common man and woman.

The carelessness of the author in preparing the manuscript and proof reading is everywhere apparent. Psychology is spelled "physchology" in the very first sentence. The following names are misspelled: Gesell, Woolley, Kuo, Descartes, Thom, and Berkeley. R. S. Woodworth becomes S. R. Woodworth. Vineland appears repeatedly as Vinland. Although these are inconsequential errors, they indicate the general characteristics of the volume. Psychologists will not find it a useful book to have in their library.

Western Reserve University CALVIN S. HALL

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THE FORMULATION AND ELABORATION OF THOUGHT-PROBLEMS

By CLEO CHRISOF, American Youth Commission, Washington, D.C.

We commonly accept with uncritical complacence man's claim to superiority over the 'unthinking' animals. Psychologists themselves have long recognized this claim by the phrase 'higher mental processes' to be accorded to human beings. The phrase implies a hierarchy among man's abilities, with senses and instincts near the lowest level and thinking and reasoning at the top. Despite the attempt of certain behaviorists to reduce thinking to the stumbling processes of trial-and-error or to mere movements of mouth and throat, as well as the proposal of others to call 'reasoning' any kind of solution achieved through any means by any animal confronted by an experimenter's problem, there has remained the conviction that human accomplishment and human advancement have rested, in large part, upon certain specific—if not 'higher'—operations of real thinking.

Since thinking and its fruits are of great importance, it is surprising to discover that underlying activities of the organism have been consistently slighted in the laboratory. The difficulties of the topic are in part responsible; but less responsible, as it appears, than the success in human practice of easy substitutes for thinking. Most men avoid thinking, which requires labor and pains, readily accepting these easy substitutes. In his *Science et méthode*, Poincaré remarked thirty years ago that "la plupart des hommes n'aiment pas à penser et c'est peut-etre un bien, puisque l'instinct les guide, et le plus souvent mieux que la raison ne guiderait une pure intelligence . . . It faut donc penser pour ceux qui n'aiment pas à penser." Though we were forced to agree that thinking is not for all, we should not relinquish either the firm conviction that skilful and successful thinking by the capable is of primary import or the contention that the operations of thinking still present a challenge to the descriptive psychologist. The two main aspects of this complex operation; namely, formulation and elaboration, are named in our title.

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* Accepted for publication April 15, 1938. This study was directed by Professor Madison Bentley in the Research Laboratory of Psychology, Cornell University.

Most of the experimental investigations at hand have been limited to the securing of estimates, in quantitative form, of the *product* or *issue* of something called thinking but wanting a description or so much as a clear definition of the means and method of production; without, that is to say, first coming to an agreement or understanding upon the actual nature of the alleged performance itself. These studies have been concerned with the number of problems solved, promptness or delay in solution, the relation of failures to successes, the retention of completed and incomplete tasks, and the relation of outcome to such abstractions as 'intelligence' or 'drive.' The common method has been loosely to include under 'thinking' and 'reasoning' an animal's (including the hominid's) 'successful' behavior in a situation which seemed to the experimenter to be 'problematical.' Here the experimenter's double interest in the rat's behavior and in his own 'problem' tends to blind him to the logical confusion involved in relating the one to the other. For it should be clear that the significance of *the subject's solution* lies in its relation to *the subject's problem.* There is a touch of absurdity in an experiment upon problem-solving in which a rat, a dog, or an infant solves a man's problem.

The sparseness of descriptive material on the nature of thinking itself is reflected in current textbook definitions. Here are samples.

Thinking defined as 'association of ideas.' Warren and Carmichael may be selected as representative of those who identify thinking with association.[1]

"In man imagery, and more especially such compounds of imagery as make up thought, tend to become one of the most important phases of mental life. A perception arouses a thought, this thought excites another thought, this in turn a third, and so on. A long series of thoughts may arise in quick succession. . . . Such a train of thoughts is called thinking. . . ."

"For example, you hear a certain humming noise and think of an airplane. This suggests the thought of the airmail, then you think of crossing the Atlantic, then of a great Atlantic liner. . . ."

"The succession of mental images and thoughts is commonly called *association of ideas.*"

Thinking defined as 'subvocal behavior.' The strictly behavioristic treatment is best exemplified by Watson, who defines thinking in terms of subvocal behavior.[2]

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[1] H. C. Warren and L. Carmichael, *Elements of Human Psychology,* (rev. ed.), 1930, 298.

[2] J. B. Watson, *Psychology from the Standpoint of a Behaviorist,* (3d ed.), 1929, 356.

"Thinking would comprise . . . the subvocal use of any language or related material whatever, such as the implicit repetition of poetry, day dreaming, rephrasing word processes in logical terms, running over the day's events verbally, as well as implicit planning for the morrow and the verbal working out of difficult life situations. The term 'verbal' here must be made broad enough to cover processes substitutable for verbal activity, such as the shrug of the shoulder and the lifting of the brows. It must embrace the implicit movements demanded in the use of the deaf-and-dumb sign manual, which are, in essence, word activity. Thinking then might become our general term to cover all subvocal behavior."

Thinking defined as 'mental trial-and-error.' The weakness of the behavioristic view resulted in numerous modifications which sought to account for the formerly denied 'mental' factors in thinking. Of these modifications, the most tenaciously held defines thinking as 'mental trial-and-error.' As interpreted by Woodworth and by Murphy, for example, the view relates thinking to *remembering.*

"Reasoning may be described as mental exploration. Suppose you need the hammer, and go to the place where it is kept, only to find it gone. Now if you simply proceed to look here and there, ransacking the house without any plan, that would be motor exploration. But if, finding this trial and error procedure to be hopeless, you sit down and think, 'Where can that hammer be? Probably where I used it last!' You may recall using it in a certain place, go and find it there. You have substituted mental for motor exploration, and saved time and effort."

"The *process* of reasoning is illustrated very well in this simple case. In a way, it is a trial and error process. If you don't ransack the house, at least you ransack your memory. . . ."

"It differs from mere trial and error in that it follows clues by thinking them through, instead of by motor exploration. It follows them by recall of previously observed facts."[3]

"In its *simplest form,* thinking may be regarded as controlled association. . . . Thinking of this sort is involved in the effort to recall a name which is on the tip of one's tongue, the attempt to place a familiar face for which no name is found. . . . Thinking consists of searching for the right answer to a question. . . ."[4]

Thinking defined in terms of everyday usage. This convenient, though undiscriminating, usage can only serve to convince the beginning student that psychology is merely 'common sense.' The instructor who seeks to break down this false conviction will find the definition a hindrance rather than an aid.

"What do we mean, in psychology, by 'perceiving,' 'thinking,' and 'feeling?' We mean nothing more, and nothing less, than what is meant by them in the every-day conversation of men of average education. These are terms which, by the conventions of language, are employed to designate occurrences which are recognized by all. By majority usage, the general term which covers 'remembering,' 'imagining,' and 'anticipating,' is *thinking.*"[5]

A few experimental studies have yielded somewhat more pertinent results.

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[3] R. S. Woodworth, *Psychology,* (3d ed.), 1934, 464-465.
[4] G. Murphy, *A Briefer General Psychology,* 1935, 314-315, 341.
[5] K. Dunlap, *Civilized Life,* 1935, 15.

Heidbreder distinguishes the products of thinking from those of other psychological performance; although she discovers in thinking no distinctive operation. She concludes that thinking is merely a composite of other psychological activities, somehow yielding its own products. Thus "thinking . . . denotes any activity which, by some means other than overt motor reaction, produces responses which are adequate, novel, and invented."[6]

Duncker advanced toward a depiction of the thinking performance, but he was hampered by the doubtful and ambiguous concept of 'insight.'[7]

"Thinking is the process which by insight into a problematic situation leads to appropriate responses. . . . After the problematic situation as such has been fully comprehended, the thinking process sets in with the 'penetration into the conflicting circumstances of the problematical situation.' The 'penetration' is the first and principal phase in the thinking process. It consists in the insightful grasp of those traits . . . which cause the conflict."

Approaching the subject from a new direction, Ryan and Lemmon have given actual descriptions of certain phases of thinking operations, Ryan[8] dwelling upon the place of *the task* in these operations, and Lemmon[9] discovering the functional factors in the analogical varieties of thinking.

Further research on this topic is one of the greatest needs in psychology. As we see it, the problem is three-fold: to find the salient characteristics of thinking at large; to delimit and to describe the main stages in the total course of this operation; and to determine those means and methods of initiation and guidance by which a thinking performance progresses toward a conclusion. It is the aim of this study to discover some of the essential phases of this problem and to indicate the lines along which future research may profitably be directed. We shall try to avoid the implication of a 'mental process' which is *thinking,* and of a 'mental object' which is *thought.*

THE EXPERIMENTS

Three considerations were dominant in planning the experiments. (1) Materials were sought which would provoke a performance involving the integration of thinking with acting.[10] (2) The experimental occasion was relieved, so far as possible, from the restriction of those formal instructions which would have provided *O* with a problem designed by *E*. *O* was left free to formulate and direct the specific course of his own elaborative performances. It was thus possible to secure a description of the activities involved in formulation and also to determine whether these activities are a part of the thinking performance itself or merely an external and initiating factor. Finally (3) the experiments were devised with the purpose of avoiding so far as possible the confusions and complications arising from too great a dependence upon linguistic tools.

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[6] E. Heidbreder, An experimental study of thinking, *Arch. Psychol.,* 11, 1924, (no. 73), 43.

[7] K. Duncker, A qualitative (experimental and theoretical) study of productive thinking (solving of comprehensible problems), *Ped. Sem.,* 33, 1926, 702-703.

[8] T. A. Ryan, Mathematical objects and symbolizing, this JOURNAL, 51, 1938, 283-303.

[9] M. L. Lemmon, A psychological consideration of analogy, *ibid.,* 51, 1938, 304-356.

[10] The value of such an approach to the problem of thinking was first indicated by Ach in his pioneering experiments (*Ueber die Willenstätigkeit und das Denken,* 1905). Although preoccupation with the search for 'thought elements' and the ready

In brief, our procedure provided competent *O*s with an experimental occasion and preserved in uniform terminology a descriptive account of the resulting performance.[11] *E* supplemented the reports by the observed behavior of *O* during the experimental session. Since the emphasis of the study is upon a *description of functional course* rather than upon the degree of achievement, quantitative aspects of performance were of incidental interest.

Materials. There were three series of experimental occasions. The formal instruction in Series 1 and 2 was to *create and solve a problem.* When functioning took a persistently digressive course (*i.e.* away from the formulation of any problem appropriate to the occasion), a secondary instruction was interposed to create and solve a *different* problem. Before experimenting began, *O* was informally encouraged to discard the 'testing' attitude. Materials were either placed on a table before which he was seated or presented in some other visual way. To eliminate distraction, *E* was seated behind a screen. A mirror was so arranged that *E* might observe inconspicuously *O*'s behavior while at work.

Series 1. Each of the six sets of this series involved the presentation of a group of irregularly-cut pieces of cloth of various shapes, colors, and materials. The groups differed from one another in the total number and variety of pieces, and were similar in that each contained within it a sub-group distinguished by the common characteristic of thickness. The quality of thickness was indicative of a more fundamental distinction, by which the pieces of this sub-group might be set apart from the other pieces, for each of the thick pieces was part of a paper pennant disguised by a cloth cover.

All the pieces of a pennant were present in a single group. Upon discovery of this circumstance it was possible to fit them together into the whole pennant. The group presented to casual observation merely a heterogeneous collection of cloth pieces. We describe the six sets of this series.

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retreat to physiological explanation of obscure psychological performance clouded the interpretations of Ach's work, as well as that of his Würzburg colleagues, the discovery of the importance of the *Aufgabe* and of the central fact of *determination* was undeniably valuable.

[11] Observers were Dr. T. A. Ryan (*Ry*), Instructor in psychology, Dr. M. S. Ryan (*Sh*), R. M. Lindner (*Li*), and J. I. Lacey (*La*), assistants in psychology.

For the discussion of the nature of our experimental technique see M. Bentley, The nature and uses of experiment in psychology, this JOURNAL, 50, 1937, 452-469.

Emblem of	Total no.		Pennant pieces		Cloth pieces
(1) Brown Univ.	56	14	various shapes and sizes covered with figured silk	42	various shapes, sizes, colors and materials, some the same as the pennant covers
(2) Fordham	20	6	various shapes and sizes covered with the same brown cloth	14	various shapes, sizes, colors and materials
(3) West Point	36	9	various triangular shapes slightly different in size covered with figured silk	27	all of linen of three shapes and nine colors
(4) Pennsylvania	27	18	various shapes and sizes covered with material different in color and texture	9	all of silk taffeta of the same size and oblong shape but different in color
(5) Yale	12	6	various shapes of about the same size covered with plaid taffeta	6	all of the same size and oblong shape in white piqué differing in weave
(6) New York Univ.	36	12	various shapes and sizes covered with material different in color and texture	24	various shapes, sizes, colors and materials, some the same as the pennant covers and some backed with paper

Series 2. This series consisted of eight sentences in code, constructed by intermingling, in a specific pattern, the letters forming the sentence and 'irrelevant' letters. A simple example would be 'Time flies,' written 'Taibmce fdleifegs' by inserting the irrelevant series a, b, c, d, etc. The situation could, therefore, be grossly factored into two parts; the hidden sentence and the irrelevant letters. It was thus comparable to the situation in *Series 1.* There was in both a combination of the relevant and the irrelevant which could be disentangled only by close and critical scrutiny. Though greatly simplified, the situations were fundamentally similar to complicated problems presented to the investigator in the sciences who succeeds in ordering and classifying a heterogeneous mass of apparently unrelated items by the discovery of a principle through which items relevant to a problem are distinguished from irrelevant items.[12]

To replace the corrective secondary instruction given by *E* to halt digression in *Series 1,* there was substituted in the present series a succession of 'occasional' clues, hinting at the nature of the problem and of its solution. Clues became progressively more revealing and were presented at *E*'s discretion whenever performance seemed to have gone entirely astray.

The eight problem-sentences for the eight sections are given below, together with their clues, translation and mode of construction.

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[12] The writer plans to extend this investigation to include more and more complicated problems to match really difficult thought-tasks of the researcher and the scholar. It was considered desirable to begin the investigation with the simpler forms.

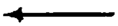
CODED FORM

(1) Cahbocodseef aglhtiejrknlamtnionpgq lrestttuevrwsx.
(2) Aalbtcedrenfagthei ljektltmenrosp fqorrsmt wuovrwdxsy.
(3) Dablutrezrmnwaqtae jlxepthtnecrbs vakrse liwrerieglnepvyaonft.
(4) Aaa mbboccdddieefffiggchhaiitjjikkollnmm onnfoo tpphqqerr
pssrttiuunvvcwwixxpyylzzeaa abbpccpddleeiffeggshh hiiejjrkkell.
(5) ravebwvcxedyrezsfaegbdhciidsjeskfelgnmhtnieojnpkcqlermtsnhtoeup
(6) ddeetstriemvoesrienconiestsneersgeohrtpfloascriettetbealhephltadenhat
(7) tcbaoserritbkr.dawcaowhtwdawofnoinitnimoeh
(8) gsrh ziizmtvnvmg rmeloevh hfyhgrgfgrlm

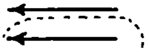
CLUES

(The numeral designates the sentence to which the clues apply, the letter the order in which the clues were presented. In each case the clue was typed below the problem sentence.)

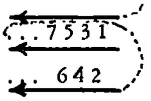
(1) a. abcde . . .
b. a b c d e f   g h i j k l m n o p q   r s t u v w x y
(2) a. same as 1a
b. a b c d e f g h i   j k l m n o p   q r s t   u v w x y
c. A*a*l*b*t*c*e*d*r*e*n*f*a*g*t*h*e*i* l*j*e*k*t*l*t*m*e*n*r*o*s*p* f*q*o*r*r*s*m*t* w*u*o*v*r*w*d*x*s*y*. (Italic letters were crossed out.)
(3) a. Dburz . . .
b. D b u r z m w q a   j x p h n c b   v k s   l w e i g n p y o f
c. *D*a*b*l*u*t*r*e*z*r*m*n*w*a*q*t*a*e *j*l*x*e*p*t*h*t*n*e*c*r*b*s *v*a*k*r*s*e *l*i*w*r*e*r*i*e*g*l*n*e*p*v*y*a*o*n*f*t. (Italic letters were crossed out.)
(4) a. aa bb cc dd ee ff . . . . . .
b. Aaa mbbocc (etc.)
(5) a. abcde . . .
vwxyz . . .
b. a b c d e f g h i j k l m n o p q r s t u v w x y z
(6) a.



b.



c.



d. dettimosinoissergorplacitebahplaeht
e. desreverecnetnesehtfosrettelehtdna
f. (d and e combined, one under the other)
(7) a. $\longrightarrow \quad \longleftarrow$
b. $\overset{1}{\longrightarrow} \quad \overset{2}{\longleftarrow}$
c. $\overset{t}{\longrightarrow} + \overset{eh}{\longleftarrow}$
(8) a. $a \longrightarrow z \longleftarrow$
b. a = z
c. a = z
b = y
c = x
d. (same as c except that the alphabets are completed)

TRANSLATION

(1) Choose alternating letters
(2) Alternate letters form words
(3) Alternate letters are irrelevant
(4) A modification of the principle applies here
(5) The sentence is reversed
(6) The alphabetical progression is omitted and the letters of the sentence reversed

(7) The combination is one forward with two backward
(8) This arrangement involves substitution

PLAN OF CONSTRUCTION

(1) The letters of the alphabet alternate with letters forming words of a sentence. The alphabetical sequence begins with the second letter. The group begins with a capital and ends with a period (thus indicating a sentence-structure).

(2) Same as 1.

(3) Letters forming words of a sentence alternate with 'nonsense' letters. The group begins with a capital and ends with a period.

(4) Every third letter represents the sentence-sequence, between which is interspersed an alphabetical sequence in pairs. The group begins with a capital and ends with a period.

(5) Every third letter, in reverse direction, represents the sentence-sequence. The first of the pairs of letters interspersed combines two alphabetical progressions. Beginning with the second letter, every third letter follows in alphabetical progression to the end of the group, *i.e.* to *u*. This progression is completed by reverting to the third letter at the beginning of the group, and continues as before with every third letter. The second alphabetical progression begins after completion of the first and follows the same order of every third letter, to the end of the group.

(6) Letters of the first half of a sentence are interspersed with letters of the last half of the sentence, in reverse order.

(7) Each letter, reading forward, is combined with two letters, reading backward, thus forming a sentence.

(8) The letters of the alphabet are paired with and substituted by letters of the reversed alphabet.

Series 3. These experiments were planned to emphasize a fundamental similarity of the actional and elaborative functions. In both the situation holds something-to-be-done, which is complicated by the necessity for discovering a means-of-doing. There were eight variations of this experimental situation. The first four consisted in the visual presentation of a perceptual event which implied an orderly arrangement of a set of plain white cards (2 x 3 in.). *O* was instructed to watch the procedure and later to reproduce the original order of the cards. The cards carried identifying letters or numbers on the back (to aid *E* in checking the accuracy of *O*'s performance). In the first sets used, these markings were included in the occasion. *O* was instructed:

> Each of these cards has a letter on the other side. In their present order, from top to bottom, the cards spell two words. I am going to change the order while you watch. You are then to discover the two wor s, not turning the cards over until you have formed a method of solution. Now watch.

As soon as *E* had completed the arrangement the set was handed to *O*. In later sets of the series the instruction was simply Watch! . . . Restore the cards to their original order.

The order of the cards before and after arrangement by *E* is given below, together with the method of arrangement.

(1) Number of cards = 17. Original order: cornelluniversity. Problem order: yirvnlerconluiest. Method of arrangement: *E* took the first card, placed the next under it and the next over it, continuing to place the cards alternately on the bottom and top of the new pack, until all the cards were rearranged. The order was thus one-under and one-over. The last card was on top. The first card was at the middle, with half of the remaining cards preceding it and half of the cards following it.

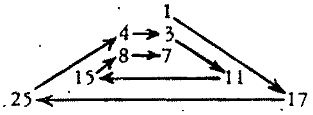
(2) Number of cards = 19. Original order: 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19. Problem order: 18 17 15 14 12 11 9 8 6 5 3 2 1 4 7 10 13 16 19. Method of arrangement: *E* took the first card, placed the next two cards over it and the next card under it. This alternation of two-over and one-under was maintained throughout. The last card was on the bottom of the new pack. The first card was preceded by two-thirds of the remaining cards and followed by one-third.

(3) Number of cards = 21. Original order: 21 20 19 18 17 16 15 14 13 12 11 10 9 8 7 6 5 4 3 2 1. Problem order: 17 16 15 14 10 9 8 5 4 2 1 3 6 7 11 12 13 18 19 20 21. Method of arrangement: *E* took the last card, placed the next to the last card over it and the second from the last under it. Continuing to take the cards from the bottom of the pack, *E* placed the next two cards on the top of the new pack and the next two cards on the bottom, the next three on the top, etc. The progression was, therefore, one-over, one-under, two-over, two-under, three-over, three-under, four-over, four-under. The first card was at the middle, with half of the remaining cards preceding it and half of the cards following it.

(4) Number of cards = 25. Original order (the cards were arranged on the table in the form shown below):

1
4 3 2
9 8 7 6 5
16 15 14 13 12 11 10
25 24 23 22 21 20 19 18 17

Problem order: 1 2 5 10 17 18 19 20 21 22 23 24 25 16 9 4 3 6 11 12 13 14 15 8 7. Method of arrangement: Beginning with card 1, *E* picked up the cards in the order indicated by the following diagram:



(5) In the fifth set of the series the instruction was merely, Watch! *E* arranged in a horizontal row on the table eleven cards bearing numbers in arithmetical progression. This was followed by three rows (each row being covered before the next was shown), in which the same progression was maintained but the members were uniformly increased by two. The four rows were then uncovered and shown together.

2	8	3	10	6	14	11	20	18	28	27
4	10	5	12	8	16	13	22	20	30	29
6	12	7	14	10	18	15	24	22	32	31
8	14	9	16	12	20	17	26	24	34	33

The arithmetical progression, typed on a card, was then presented.

+6 −5 +7 −4 +8 −3 +9 −2 +10 −1

Examination of this progression will reveal that it contains within it a further progression in alternation, *i.e.* a *minus* progression from right to left: −1 −2 −3 −4 −5 and a *plus* progression from left to right: +6 +7 +8 +9 +10.

(6) In the sixth set the following progression was presented on a single sheet:

5 5 1 8 16 11 19 57 51 60

Following this was a sequence of 'clues' similar to those of *Series 2.* These were discontinued as soon as *O* had fully grasped the progression.

CLUES

(6)	a.	5		5	1		8		16		11	19		57		51	60
	b.	5		5	1		8		16		11	19		57		51	60
			×1					×2					×3				
	c.	5		5	1		8		16		11	19		57		51	60
			×1					×2					×3				
					—4												
	d.	5		5	1		8		16		11	19		57		51	60
			×1					×2					×3				
					—4					—5					—6		
	e.	5		5	1		8		16		11	19		57		51	60
			×1					×2					×3				
					—4					—5					—6		
						+7											

(7, 8) Sets seven and eight reverted to the pack-arrangement of the earlier members of the series. In the 7th set were twelve cards. Taking the cards alternately from the top and bottom, *E* placed them successively in four piles so that the top card of the set was at the bottom of the first pile and the second card from the top at the bottom of the third pile. The total arrangement was, therefore, as follows:

Pile 1	Pile 2	Pile 3	Pile 4
5	8	6	7
3	10	4	9
1	12	2	11

Having placed the cards, *E* then picked up Pile 1 and placed it on Pile 2; then picked up the combined group and placed it on Pile 3, then picked up that group and placed it on Pile 4. *E* then picked up the entire pack and handed it to *O* to restore to the original order, *i.e.* from 1 to 12 consecutively. The sequence of the cards as given to *O* was, therefore,

5 3 1 8 10 12 6 4 2 7 9 11

The eighth set in the series followed the same procedure except that there were 24 cards which were placed in three piles.

I. CHARACTERIZATION OF THE MAIN STAGES OF THINKING

Our results suggest the following notion of thinking; as of a highly integrated performance involving a directive organization of various other psychological functions together with their products. It is further distinguished by its formulation of a problem which is advanced toward a solution. Examination of our data reveals three typical main stages in the total operation: (a) formulation of the thought-problem; (b) elaboration; (c) progress toward a solution (complete or partial solution or defeat).[13]

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[13] The technical terms and abbreviations in the reports which follow conform (with a few additions) to the list established in Cornell studies in dynasomatic psychology, this JOURNAL, 51, 1938, 224.

act (A) = acting
em (E) = emoving
per (P) = perceiving
TS = task-setting
SI = self-instruction
OI = occasional instruction

(a) Formulation of the problem. Developing the thought-problem was found to be an essential phase of thinking. This was demonstrated both where *O* carried out the instructions to create a problem and where he failed. The importance of this primary activity of formulation has been almost entirely overlooked by previous investigators, who have assumed that the problem exists ready-made and has only to be somehow appropriated by the prospective thinker. This assumption has led to the omission of an essential stage of the performance.

Let us examine the character of a problem which is of the *thinking* kind. What are its distinguishing marks? And how does it develop? We find first the *problematic occasion, i.e.* a situation which is essentially obscure and unclear and which resists clarification. We say that it is puzzling or baffling. Its want of precise meaning is indicated by such ejaculations as 'What *can* it mean!' and 'It doesn't make sense.' Furthermore, it is qualified by a restless dissatisfaction (unpleasant in cast); dissatisfaction which is strikingly similar to the emotive predicament.[14] In both cases there is set up an active, vigorous search for a way out, *i.e.* for a means of resolution. Here are instances.

S (there must be something to be done with this; I wonder what it is) *cum* com ("This is awful!") > SI (make something; something must be done) . . . insp (what can this be!) *cum* com ("wish this were over!"); S_g (carried by general bodily posture forward; biting cheek in puzzlement); insp (I don't see what I can do; I'm stuck) . . . *La* 1-1

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rem (R) = remembering
imag (Im) = imagining
insp (In) = inspecting
comp (C) = comprehending
T_e = elaborative thinking
S = searching
S_p = primitive searching
S_g = goal searching
som = somesthetic
kin = kinesthetic
vis = visual
aud = auditory
tac = tactual
com = comment (in words)
verb = verbal
fl = flash

FI = formal instruction
P_f = functional product
G_e = extra-organic government
G_o = organic government
G_h = historical government
[] encloses experimenter's or observer's remarks
() encloses functional products
. = complete break in function
: = closely related functions
cum = concurrent functions
> = successive functional phases

Functional moments included in compound performances are written adjectivally with a small initial letter placed as subscript to an abbreviated symbol for the main function. Thus, A_p = a perceptive moment in acting: E_m = a memorial moment in emoving. In this context, *imagining* and *inspecting* are distinguished by Im and In. Thus, In_s = inspecting qualified by search. Quotation marks enclose verbalized products.

[14] For a description of the emotive predicament see M. Bentley, *The New Field of Psychology,* 1934, 233-236.

S [to find a problem] *cum* organic strain and restlessness; insp (fruitless) > S [sighs heavily and looks at watch] . . . *Li* 1-4

S [for a problem] > fl (this is hopeless) > S [for a problem] > SI (damn it! there must be a problem here) . . . insp ("I can't do anything else with this"); S_{in} (I give up) > lapsing into idle per . . . *La* 1-5

This problematic occasion usually incites activity directed toward resolution. The incitement requires, however, that *O* see far enough into the situation to discover its puzzling nature, and our reports include many instances of failure to initiate performance necessary to the creation of a problem because of an inability to penetrate the occasion. Again, *O* may ignore the lack of clearness and occupy himself instead with whatever diversive substitutes may be at hand. Indeed, this resort to substitutive activity occurs at many points along the entire course of thinking. But when *O* grasps the situation, there results an active exploration which draws chiefly upon perceiving and inspecting moments. Note the following reports.

Per (whole field as haphazard) >SI (look for something to do) > insp (it's just a mess). per (this piece as having something glued to it) > insp (two pieces glued together) > OI (insp other similar pieces) > P_{abs} (material on part of several is the same) > TS (try to fit the pieces together) . . . insp (two different kinds of figured materials are attached to those composite pieces) > TS (do something with figured pieces) [dropped because of haphazard way in which figured materials were used] . . . *Ry* 1-4

Per (letters in special formation) > insp (a series of letters obviously spaced in word formation) > SI (what can it mean?) > fl (cryptogram); comp (cryptograms are solved on the basis of the natural frequency of letters) > OI > TS (determine frequency of letters) . . . insp (letters) *cum* com ("if these are words, they are very long; perhaps in a foreign language") . . . *La* 2-1

Substitutes for formulation. Thinking is especially difficult in this initiatory phase and a common device is to seek less demanding and more comfortable activities, thus avoiding bafflement and perplexity. Our common and everyday substitutes are prejudices, opinions, fixed beliefs, sheer guesses, and ready verbal tags. Since our experimental conditions did not favor these escapes, *O* was forced to invent some other means of resolving the difficulty at hand. At this invention, however, he was very apt.

The most frequent means was through imaginational construction. The essential distinction between this imaginational form of invention and that of real thinking is the lack in the former of any clarification of the baffling situation. The imaginational performance does not unravel; it embroiders. The situation remains obscure. The thinking performance, on the other hand, is directed toward clarification, and to some degree at least, attains it. We give below a few illustrative reports of problem-substitutes.

Per (pieces) > insp (good many round ones this time) > OI (I ought to be able to do something with that as a starting point); im_{vis} (a snowman) > in_{im} (these pieces might be a snowman); SI (that could represent winter; flowers could represent summer) > TS (make a snowman) > act (arranging) . . . TS (now make something to represent spring) > insp (spring is usually when people are falling in love); im_{vis} (students walking hand in hand here on spring nights) > OI (how to do that with these pieces?) > insp (this piece might be the moon) > TS (build a bench and have a pair sitting on it) > act (arranging) > TS_{sub} (find a piece to be his arm around her) > act (arranging) > insp (completed figure); com ("well, I certainly like my man and woman; they look most loving") > insp (I hate to disturb my precious couple; I hope I don't have to do something else with them). [After FI: Create a different problem] S [for some problem which would not disturb the couple] . . . *Sh* 1-6

SI (make something) > random arrangement > OI (this arrangement looks like the profile of Sherlock Holmes) > insp (yes, there is his hat; there his scarf; and this piece is the turned-up collar of his coat) > OI > TS (arrange the other pieces to represent the profile of Watson; thus we will have a conference between Holmes and Watson) . . . *Li* 1-4

The persistence of the imaginational trend is particularly striking in this *O*, who reports in a later series:

TS (make a couple of jingles based on the words formed) > insp_{im} (lines of a jingle); SI (count feet in the line to make it rhyme) > act (writing and reading the lines); insp (jingle: Let the cod and the perch Rest in the sea With you and me. But let us not fall In the same old rut Like that foolish old crut, The Llama!)

It is clear from the above reports that this form of imaginational activity does not clarify the problematical situation.

Another diversive performance which was also ineffective in formulating the problem was that of idle, goalless psychological flitting, governed only by fortuitous instruction from the occasion. This roaming seems characteristic of *O* when he finds he must do something but does not care to exert himself. It also occurred in our *O*s whenever performance was blocked by the fruitlessness of prolonged search and inadequacy of the psychological products uncovered. In *Series 1* it took the form of random handling and arranging of the cloth pieces, accompanied by a running commentary; in *Series 2* of random selection and combination of letters, accompanied by attempts at verbalization. The following excerpts illustrate the latter form of this activity.

S (random selection and combination of letters) *cum* verbal pronunciation > insp (groups of jumbled letters) > S [merely gazing at the letters in the hope that they would become reorganized into words] . . . *Ry* 2-6

S [juggling the letters] *cum* comp, bare (there's nothing here that resembles the mechanics of previous problems) > comp (there must be something here, some method by which words can be brought out) > S [juggling letters without a plan] . . . *Li* 2-7

Finally let us consider those instances where the activities set into operation during this first stage yield a formulation of the problem. We include here not only a grasp of perplexing factors in the situation but also the positing of means of resolving them. Any of the psychological functions may be here operative, either singly or in integrated forms. Furthermore, the products of these functions are so referred to the problematic occasion as that they are either discarded as useless or retained as significant to the problem and possibly to its solution. In the following examples we find perceiving and inspecting constantly employed to turn out useable material.

Insp (here is a series of letters with several possibilities: 1, letter sequence; 2, sentence sequence; 3, cryptogram; 4, isolated words hidden; 5, nonsense grouping; 6, some other grouping) > TS (determine whether any of these is correct) . . . *La* 2-1

Per (period at the end of the three groups) . . . TS (try to make a sentence) . . . *Sh* 2-1

Per (jumbled letters) > TS (unscramble) . . . per (two gaps between the groups) > TS (make the letters into three words) > TS (look for vowels) . . . *Ry* 2-1

TS (identify as many of the materials as possible) > insp (materials) > insp (this piece seems to be made of two layers; paper pasted on the back of cloth) > TS (inspect it carefully to determine whether the paper backing is there simply as a matter of chance) . . . per (this piece is more of the same material) > TS (look for other like pieces) . . . *Ry* 1-2

Thus when performance is clearly directed toward an anticipated solution we may say that the problem has been fully set and developed and that the initiatory stage in thinking is—at least temporarily—complete. As we have pointed out, the characteristics of this stage include an initial bafflement induced by an occasion which is really problematical to *O;* a search for means of discerning and segregating the perplexing factors, or an escape by some substitutive device; the positing of various methods of attack; and, finally, the setting of the operation in the direction of an anticipated solution.

(b) Elaboration. It must not be supposed that performance is always as clearly marked off into well-defined stages as may be suggested by our general plan. Many variant arrangements of moments and stages occur. Nevertheless, activity does take a new functional turn as soon as *O* has created a fresh problem and has thrown it into commission for solution. Then comes elaborating, sometimes tentative, halting and vague and sometimes direct, progressive and effective. In both cases methods of solution are proposed, tried out, amended, rejected, and followed up.

Within this elaborative stage are distinguishable three important phases, *i.e. (i)* the accumulation of material, *(ii)* valuation and selection, and *(iii)* fusion and synthesis of selected products.

(i) The accumulation of material. The first step in elaboration is assembling 'food for thought.' Any or all psychological functions may be in-

volved. The apprehensive functions and comprehending, however, are most active. We cite a few excerpts exemplifying the extensive collection of gross materials by perceiving, imagining, remembering, and comprehending. It will be seen that imagining and remembering serve to amplify the products of perceiving by extending the occasion into the past and future; while comprehending enriches and refashions the apprehensive products by yielding new significations.

Per (third card placed in the opposite direction from the second) > im_{ant} (the plan will be alternation) > per (cards placed in alternate arrangement as anticipated) . . . *Ry* 3-1

Per (abcde) > OI (what is the significance of that?) > TS (discover the relation between this and the top line) . . . *Li* 2-1

Im_{ant} (something that could be put together) > im_{ant} (some pieces will have paper glued on the back) > TS (look for pieces with paper glued on the back) > per (this one is stiff); com ("that's one") > TS (try to fit pieces together into a flag as before) . . . *Ry* 1-6

Rem (the two top cards were taken from the bottom of the original pack) . . . TS (determine how to reverse it) . . . *Ry* 3-2

Per (something different: a z) > SI (what does it mean?) > comp (alphabet); rem (previously there were alphabetical progressions and I tossed them out) > fl (this is right) > TS (find the progression and eliminate it) > insp (the progression is not here) > SI (now what to do?) . . . SI (what does a z mean?) > SI (why the space between a and z?) > comp (substitution: a is to be substituted for z, and z for a; b for y, and y for b; c for x, and x for c, etc.) . . . *La* 2-7

Per (figure) > insp (two arrows with connecting line) > OI (what does it mean?) > comp (it means start somewhere, go as far as you can, then turn back to a new beginning place and proceed until again stopped) > S (relation of this to the problem) . . . *La* 2-5

Per (placement of third card in the opposite direction from the second) > comp (the plan is alternation) > per (placement of cards in alternation); TS (how can I sort them out again?) > insp (first card will be in the middle) > S [order? was the second card placed above or below the middle card?] > TS (count the cards to find the middle one, then see which of the two before and after it best combine with it to form a word) > Im_{vis} (cards arranged on the table) > comp (I can work back, alternately taking cards from the top and bottom) *cum* OI (watch to see whether the last card is on the top or bottom) [as last card is placed] per (last card is on top) > OI > TS (start with the top card; then take the cards alternately from the top and bottom) > act (arranging) . . . *Ry* 3-1

The most important functional tool in elaboration, however, is inspecting. The critical scrutiny which is inherent in this function is essential to thinking, especially during the elaborative stage. It is by means of inspecting that the procedures suggested in the formulation of the problem are tried and tested.

SI (maybe there is an alphabetical sequence in reverse) > TS (try every third letter beginning at the end) > insp (no reverse sequence) . . . TS (try every third letter combined with the two letters following it) > insp (no such sequence; only three words, probably there by chance) . . . *La* 2-1

Per (something different here) > insp (two arrows pointing the same way) > S (what can that mean?) > fl (something backwards) > fl (of course, sentence backwards) > SI (why two arrows?) > insp (*the* at end of the sentence, letters separated by single letters) > TS (proceed by eliminating alternate letters) > insp (the alphabet) *cum* fl (this is right) . . . *La* 2-5

TS (take alternate letters beginning with the period) > insp (four vowels: unsatisfactory to form a word) . . . *Ry* 2-6

Per (word *task*) > TS (determine the pattern) > insp (t--a-s-----k) > comp (no regularity there; it must be wrong) . . . *Li* 2-6

Insp ("over under over") *cum* fl (same as before) > per (fourth card being placed in the first pile; not what I had anticipated) > com ("just three piles this time") [insp had lapsed because I thought I had the plan; with per of anticipated placement, there was a brief panic with self-reproach, "I've got it all mixed up now; I should have watched"] > SI (watch carefully now) > insp ("over under over, etc.") *cum* fl (relief); comp (It's really not different with three piles; the first pile contains cards which come alternately from the top and bottom; the plan is still alternation) . . . *Sh* 3-8

These reports reveal two extremely important aspects of thinking: first, the variety and the wealth of psychological material which is collected during elaboration and, second, the diverse and fruitful modifications of functional trend by way of antecedent performance, a type of progressive modification which is of the very essence of thinking in all its phases and stages from task-setting to final solution. It stamps thinking as of the same functional order as acting, where also the operation is continuous, forward-tending and cumulative.

(ii) Valuation and selection. The many products which accumulate during elaboration are often extremely heterogeneous and unrelated. By critical appraisal (well illustrated in the above reports), this raw material is refined and sifted to a more orderly mass of selected products, which bear more directly upon the problematical occasion. This valuing-and-selecting is usually carried on, in our experiments, through inspecting and comprehending. The obscurities of the occasion are thereby partly clarified and performance moves toward solution. Our facts indicate that the appraisal and the subsequent selection or rejection of products, involving the ability to distinguish serviceable and relevant from non-serviceable and irrelevant materials, is a basic factor in sound thinking. It is therefore obvious that the inculcation of this form of comprehending and of inspecting is of prime value in acquiring the art of thinking. This aspect of the really elaborative form of operation also serves to distinguish it clearly from all those behavioristic forms of 'problem-solving' which exploit trial-and-error, associative learning and conditioning.

We should note, however, that appraisal does not lead immediately to solution. There are many reversals, revivals, rehearsals and reconstructions in the course of thinking. Values are frequently misapplied and performance often misguided. There is an almost constant shifting in the occasion as newly-fashioned products throw into relief now one aspect of the problem and then another. It is easy to be lost among these changing forms and estimations, and correspondingly difficult to detect the essential aspect of the occasion and the valuable means of reaching a solution.

(iii) Fusion of selected products of elaboration. Although the three moments in elaboration are not always to be found as temporally distinct and of a fixed order, the fusional and integrational phase is logically last, for here occurs the final synthesis of selected materials whose product is the solution, although preliminary fusings and integratings may serve the thinker at any stage. The functional processes of integration are very subtle and often highly condensed. In fact the sturdy and ancient belief in what men like to regard as 'intuition' and 'insight' suggests the sudden discovery of great truths as a flash without any intermediating process in the mind of the thinker. While this belief rests almost wholly upon a legend of the 'divinity in man' and is wanting in experimental evidence, our own scanty results do not go very much beyond a definite and empirical statement of the problem of functional products with or without underlying and corresponding functional procedures of a psychological kind.[15] Until the problem has been carried through under a sound laboratory procedure very much more exacting than it has ever been, the existence, the nature, and the range of activity of an alleged 'intuitional' or 'insightful' faculty of thought should be regarded as a convenient, comforting and flattering hypothesis. Since the entire psychological development of man, especially from adolescence on, abounds in condensation, ellipsis, and functional short-cutting, nothing stands in principle against the production of large and significant issues from obscure and unobserved facts. More definite conclusions await a great increase in knowledge of the bodily conditions of elaborative thinking and of the psychological procedures which are involved. Until now the neurologist and the behaviorist have added very little in either direction.

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[15] As regards the actuality of 'insight' (as a psychological category), main references and an estimate of the evidence may be derived from M. E. Bulbrook, An experimental inquiry into the existence and nature of insight, this JOURNAL, 44, 1932, 409-453. Another way of approaching the problem is through the experimental study of the *flash* or *Bewusstseinslage.* See M. Bentley, *The New Field of Psychology,* 1934, 291, 303-306, 405-410.

The case for intuition in the sense of "sudden illumination" is cogently presented by H. Poincaré, *Science et méthode,* 1908, 43-63. The appeal is to various unconscious agents and to an esthetic sensibility.

The following excerpts, chiefly taken from the final stage of elaboration in our experiments, will at least show that, under our conditions, wherein *O* created and worked through his own problems of thinking, the functional phases of collecting and reassorting his materials in the elaborative stage led up to, and issued in, integrative procedures which passed beyond these intermediate products, by something like an inferential process, and suggested what was to the observer a new solution.

Per (a = z); comp, flash-like (a equals z; therefore substitute a for every z) > insp (if a equals z, then b equals y, etc.) > TS (translate the letters in that way) . . . *Li* 2-7

TS (identify arrangement) > insp ("first card out, second top, third bottom, etc.") > insp ("if there are an uneven number of cards, the middle card will be the first letter, the card above that will be the second letter, the card below, the third, above the fourth, below the fifth, etc.") > TS (count the cards to see if they are an uneven number) [counting: 17] insp ("uneven; *therefore* the ninth will be the middle card and the first letter") . . . *La* 3-1

Rem (last two cards on top were identified as cards from the bottom of the original pack) > comp (since I started with the bottom cards, they are now in reverse order) > TS (how to reverse?) > $\mathrm{im}_{\mathrm{vis}}$ (pack turned over) *cum* com ("I can't turn them over") > $\mathrm{R}_{\mathrm{gen}}$, vague (shuffling playing cards) > OI > TS (take them off the top in that way) . . . *Ry* 3-2

Insp ("it's down, around, reverse") *cum* comp (the scheme is a wavy line) > insp, relaxed (*E* is doing it as I anticipated) . . . rem (the top card is the last card) > comp ("*therefore* the first card I place will be the right-hand card of the second row") . . . *La* 3-5

Looking back upon the elaborative phase at large, we may say that the single word which seems best to express the entire procedure is 'manipulation,' *i.e.* herein the organism manipulates a great variety of psychological products. A fairly small fraction of these products are made under the guidance of the environment, of what the behaviorist calls *stimulus*. These are perceptive products. The larger fraction are turned out, in relative independence of the environment, from other resources of the organism under the guidance of a general *task* and of the specific thought-problem which the organism has created. The products once given, all along the course of elaboration, manipulation consists in extending, revising, condensing, and crystallizing, in preparation for a new product which is to emerge (where thinking is wholly successful) as the end-sought-under-the-problem.

In summary, the distinguishing contribution of the period of elaboration is (1) a collection of varied psychological materials; (2) an ordering of these into the relevant and the irrevelant materials through a critical appraisal based on their 'worth'; (3) a further scrutiny of 'accepted' products; (4) integration into a unity where products have been not only related to one another but to a general topic which is the anticipated solution.

(c) Solution or defeat. The final stage may emerge in solution, partial solution, or defeat. The following reports are examples:

. . . Com ("there's nothing more to this") [after clue x1-x2-x3 is presented] > per (startle) *cum* fl (this isn't over) > insp (5 × 1 is 5; 8 × 2 is 16; 19 × 3 is 57) > com ("now what?") > S [to continue this pattern] > insp (various patterns) . . . [with next clue] > S [what more?] > fl (if you drop the signs the whole thing is a progressive series from 1 to 9) > insp (within that progression there is also a multiplication progression of 1-2-3, a minus progression of 4-5-6, and a plus progression of 7-8-9) . . . *Li* 3-6

Per (same group as before) > rem (card previously shown) *cum* S [will those differences fit in here?] > rem (only plus and minus signs on card) *cum* comp, sudden (the card gave the composite plan of the series) > per (28 : plus six) [checked several more differences] > com ("that's what it is; queer that I didn't recognize it") . . . *Ry* 3-5

Per (*strive:* peculiar arrangement of the letters) > TS (determine the pattern of arrangement) > insp (pattern) > TS (find the next word by using this pattern); com ("I can't quite get the principle here") > SI (try to formulate the principle by a diagram) > insp (various diagrams) . . . S ("what can possibly be the principle here?") > insp (possible progressions are 1-2-1, 2-1-2, 3-1-3) > insp (those combinations won't work); com ("no, that's wrong") *cum* posture indicating defeat . . . *La* 2-5

Per (individual numbers) *cum* subvocal pronunciation of differences between pairs > comp (differences didn't follow any plan) > TS (look for pattern in alternate numbers) > insp (productless) > TS (try every third number to see if they form a pattern) > insp (5 8 19; 51 as not fitting in anywhere) > per (large numbers at the end; series going up very rapidly) *cum* (the plan is probably something like adding 8, subtracting 14, adding 25) > SI (there must be something easier than that) > per, roving (16 11 19, jump to 57) *cum* com ("simply can't be any plan in this thing") . . . *Ry* 3-6

Per (4 5) *cum* com ("maybe it is a progression after all") > per (12) *cum* fl (it is going to be a progression: 12 fits in: it's 4 10 5 12, one group of alternates progressing by one, the other by 2) > per (next numbers as going down) *cum* fl (it is not right after all: doesn't fit that plan) > insp (last card is 29; next to the last is 30) > OI (maybe they go backwards) > insp *cum* fl (dismissal of that) . . . *Sh* 3-5

It is not to be assumed that the products of thinking are confined to the final stage of solution. Numerous products result during the entire course of performance: discoveries, decisions, appraisals, acceptances, rejections, etc. These do not invariably lead to solution, but they are nevertheless thought-products. In the following excerpts, for example, we find a number of instances of such thought-products without solution of the primary problem.

Per, vis. roving (groups of letters) > TS (try some conventional pattern, *e.g.* take first letter of first group, second letter of second group, third of third group) > insp (*gie*) *cum* comp (probably wrong; *gie* not part of a word) > per (*h;* rapid pronunciation *gieh:* meaningless group of letters) . . . com ("simply can't make any words out of this because there aren't enough vowels") > comp (series have been getting more and more difficult) > comp ("this is probably insoluble"). [After being instructed to continue] TS (since I can't make words, look for some pattern of regularity) . . . insp > S ("this is really useless because it's impossible to make words and I can't find any regularity of arrangement") . . . *Ry* 2-7

Insp$_r$ (the others formed sentences) > OI (so will this: the word *the* or *this* is probably here) > insp (*t* in second group; *he* in third) > insp ("looks like one-forward-two-backward progression again") > TS (try that with the beginning) > insp (*gmr:* no word there) > insp (*sh* at beginning: could be a word) > OI (continue alternating) > insp (*shiz:* no word) > insp (there seem to be many consonants but a dearth of vowels: that's strange) > insp (in third group, *helm* is in reverse order by alternation) > OI (try that plan) > insp (*mrfrhf:* that's no good) > SI (maybe I should start someplace in the middle) > insp ("but there's no way of determining where: I can't make any sense of this: so few vowels") > insp (maybe there's some sort of substitution here) > rem (I did that once before and it didn't work; anyhow it's out of the running of things so far) > SI (there must be some progression here) > com ("if I can't get it, there'll be a clue; I'm stuck") . . . *La* 2-7

Insp (first group) > TS (try various combinations to get the word) > com ("it won't make a word because there are no vowels") > fl, fleeting (maybe I should substitute a for z, etc.) [dismissed] > insp (every other letter, backwards, in the third group gives *helm*) > insp (that doesn't work with the other group: dismissal) . . . com ("I don't believe it can be anything as it is because two groups have no vowels") > TS (put the groups together) > TS (trying various combinations) > insp (dismissal: emotional toning of unpleasantness) . . . *Sh* 2-7

TS (find the hidden words) > insp (*dd ee*) > TS (see if there are more paired letters) > insp (no). TS (find some pattern) > insp (dd-ee-t-jump-t: rejected as not representing a pattern) > In$_s$ (fruitless) > insp (various patterns tried and rejected: "this is not the pattern") > SI (maybe the *e*s have something to do with it: see how frequently they occur: see if there is any pattern in their arrangement) > insp (*e*, gap 5; *e*, gap 3; *e*, gap 3; *e*, gap 5; *e*, gap 4: gave up) *cum* fl (this isn't getting anywhere) . . . *Li* 2-5

Per (numbers) > TS (see if there is some arrangement like the last one) > insp (differences) [dismissal of that since there were no consecutive differences, like 0 1 2 3] > SI (maybe something is multiplied each time) > insp (dismissal) > SI (something divided?) > insp (dismissed) . . . *Sh* 3-6

In our discussion of halt or lapse of function, differences in *rate* or *tempo* of performance have been mentioned, differences due to the failure of products to provide for a functional advance toward the anticipated goal. In sharp contrast to this slowing down, there is also a quickening or sudden revival of performance due to the emergence of newly-significant products. This quickening may occur so suddenly that *O* is scarcely aware of the means of facilitation. The course of thinking thus progresses unevenly; in one phase it may advance 'by leaps and bounds,' in the next phase it may be completely stopped. Again, it may proceed at a steady but slow pace, or with alternations of advance and retrogression. Rarely, if ever, under our conditions, is thinking consistent and unimpeded in its advance toward solution.

II. THE GOVERNMENT AND GUIDANCE OF THINKING

Like acting, thinking is a directed operation, governed and directed from stage to stage by certain guiding devices both within and without the organism. Of all the psychological functions, the guidance of elaborative thinking is most complex and most delicate. In fact, so delicate are the means for keeping it homeostatically and effectively on its course toward effective conclusions that few men have mastered the art of hard thinking, most substituting for its arduous management—as we have observed—such easier devices as blundering trial and recovery, opinions, prejudices, imitative acting and believing, and verbal formulas. First among the means of direction and control of thinking stands *the task.*

The importance of the task is clearly demonstrated in our reports (where it constantly recurs), *O* sometimes discovering as many as a hundred tasks in a single observational session. These tasks exert an influence in two directions, the one *positive,* carrying the operation in a direction already established, and the other *negative* or away from the established course. Of positive tasks there are many varieties, depending both upon the demands of the problem and upon the specificity of a particular procedure. Tasks are also to be distinguished by the anticipative forecast of the thought-product presently to issue.[16]

The contrast between task-setting under a problem and task-setting independently of a problem is brought out in the two reports which follow. In the first, the tasks are all directed toward a solution, while, in the second, task-setting proceeds without reference to solution.

> [Problem: find the sentence hidden here, by eliminating letters] TS (try eliminating alternate letters) . . . TS (try eliminating every third letter) . . . TS (try every third letter backwards) . . . TS (try alternate letters beginning with the period) . . . TS (try alternate letters, beginning with the period, going backwards) . . . *Sh* 2-6

> Per (many pieces: like a quilt) > TS (do something with them) > insp (some pieces are cut in definite patterns) > TS (make a design of them) > insp (these are all alike) > TS (make a border of them) > insp (some fit together) > TS (collect them and see what can be made of them) . . . TS (fit them together) . . . TS (fix these leaves into a design of a growing flower with the long leaves at the bottom) > act (arranging) [during arrangement, several 'crucial,' *i.e.* pennant pieces are turned over revealing the paper backing, but *O* looks at them inattentively, carefully returns them to their original position, and puts them aside] . . . *Sh* 1-1

An essential characteristic of task-setting is its derivation from previous functional products. The task may emerge from the immediate or from a past occasion. In our experiments, it most frequently came out of a present occasion. Perceiving and inspecting are especially valuable in yielding materials for task-setting, as is evident in the following reports.

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[16] It is well here to clarify the distinction between the problem and the task. In the task, emphasis is on a way-of-doing, a kind of instruction which may be employed in any psychological operation. The problem, on the other hand, always predicts a possible solution and includes at least tentative instructions for attaining it.

Per (black piece) > TS (look for other black pieces) > P_{abs} (black pieces as a distinctive group); com ("they're in a group by themselves and have nothing to do with the others") . . . TS (inspect them further) > insp (paper backing) > OI (turn over) > act *cum* per (turning over: disclosing letters S and T) > OI (another banner to be put together) > TS (find the rest of the pieces in order to complete it) . . . *Ry* 1-3

Per (one piece backed with paper) > OI (is there any significance to that?) > TS (leave that until later; proceed with grouping the circles) [inadvertently turned over a backed piece] > insp (this must be the right side) > OI (there must be some significance to the backing) > In_{im} [interrupted by] SI (continue with grouping the circles) . . . *La* 1-6

Insp (some are backed with paper) > OI (what to do with them?) > TS (use to construct something different) [undirected movement arranging pieces at random] > fl (maybe these should make something; maybe they should all make something) > insp ("damned if I know what it would be; they're so different") . . . *Li* 1-1

It is interesting to note in those cases (which were paralleled by all *O*s who did not discover the significance of the 'backed' pieces) that, although perceiving or inspecting is directed to a part of the occasion (which might be expected to effect a turn in the direction of performance) the products turned out are not sufficiently meaningful to be effective.

This is a significant laboratory parallel to the situation in daily life where a clue to the solution of a problem is disregarded because of the inability to view it impartially. There is so great a 'set' toward a particular signification that the meaning which would be obtained through an unbiased envisagement is inhibited. For example, in a report cited earlier (*Sh* 1-1), *O* was so intent upon the 'artistic' placement of leaves that the backed (pennant) pieces were regarded merely as not-leaves and therefore cast aside.

When there are indications that the performance is not making for solution, *O* must be sufficiently alert to observe it and not so determined by a present functional inclination that he does not retrench. It is evident from our research that inflexibility of this sort is the source of much inadequate and wasteful thinking. It leads to an uneconomical use (or a total neglect) of resources at hand. Too great a fondness for one's performance is also a handicap in effective thinking because it prevents a critical appraisal of products and outcomes.

Apropos inflexibility, we have discovered a striking consistency of performance in each *O*. For example, one *O* inclines toward imaginational creation and will view any situation as potentially creative in that sense; another attacks it as he would a mathematical problem and is engrossed with plans for adopting suitable procedures. Still another constantly vacillates from one view to another, quickly proposing and as quickly abandoning suggested formulations and processes. He is commanded by, but does not command, the situation. In general, we have noted a certain skill of following through sufficiently but not too far; a certain discipline of sticking long enough but not too long, which develops the degree of flexibility sufficient to meet the requirements of the thought-problem.

As we have pointed out, our experiments included the presentation of a sequence of clues which would redirect performance toward solution when a digressive course was too persistently maintained. The following reports show how task-setting evolved from the comprehended clues.

Per (arrows pointing this way) *cum* comp (letters should be read backwards) > TS (read the letters backward to get the word) > insp (*that*) *cum* S (pattern in *that*) > insp (arrangement: t, skip a, return to a, then skip four places) . . . *Li* 2-5

Per (a z) > $\mathrm{C}_{\mathrm{bare}}$ (a z means something that I am to use) > S (what does it mean?) > per (there are no *a*s) > comp (perhaps it means the first and last) > TS (take first and last letters of each group to see if they make words) > insp (no, nothing there) *cum* fl (that's wrong). $\mathrm{insp}_{\mathrm{bare}}$ (manipulation of letters: rejection because the combinations do not form words) . . . *Li* 2-7

Per (something different: a z) > S (what does it mean?) > com ("a to z") *cum* $\mathrm{C}_{\mathrm{bare}}$ (alphabet); rem (previously there were alphabets and I tossed them out) > fl (this is right) > TS (find the progression and eliminate it) . . . *La* 2-7

[After presentation of clue sheet] Comp (solution of the problem is by substitution, a for z, etc.) *cum* self-reproach ("why didn't I do it, I thought of it but was too lazy! Why did I have to wait until I was told!") . . . *Sh* 2-7

Devices for maintaining the task. The devices by which *O* keeps the task clearly before him are varied. The most frequent is linguistic, *i.e.* he attempts to formulate in words what is to be done. The words serve as a shorthand reminder and taskmaster to keep the task alive. Such linguistic devices are numerous in our reports. We cite a few examples.

Insp (arrangement) *cum* $\mathrm{Im}_{\mathrm{ant}}$ (cards-about-to-be-picked-up) *cum* S (to phrase verbally *E's* procedure) > com ("the arrangement is circular") > insp continued (the circular order is maintained) > SI (remember the order) . . . *Li* 3-4

Insp *cum* act ("top, bottom; top, bottom; top, bottom; top, bottom.") [placing each card in the appropriate piles, the word 'top' or 'bottom' served as OI for the placement of the next card.] . . . *La* 3-7

Again, the task may be embodied in a non-verbal form for guidance. An interesting example is the *emphasis-posture,* which serves to extract, through the task, appropriate products. One service of the emphasis-posture is that it ignores unuseable parts of the occasion. For example, in the excerpt given below, this device enables *O* to disregard the irrelevant letters.

TS (try to disregard all letters which do not form a word); rem (*Gestalt* theories of perception) > insp [by moving eyes over the whole group] (*alternate:* standing out from the other letters). insp continued [by the same method] (*letters are irrelevant*) *cum* fl (this is how it can be done) . . . *Li* 2-2

Rivalry and guidance. Another similarity to acting appears in the 'conflicting' or 'rivalrous' tasks of thinking, one of which becomes predominant and thrusts out another. At times rivalry attaches directly to inconsistent or conflicting products. A perceived object, *e.g.* may so predominate in a problematical setting that it leads to the abandonment of other similar objects. This rise-and-fall of rival factors of guidance is also similar to the attempts at emotive resolution. Much remains to be done to clarify both the similarity and the difference of elaborative thinking and the rivalrous and predicamentive aspects of emoving. In fact, the whole problem of initiation and guidance in psychological activity has been veiled in a complacent acceptance of such non-explanatory blanket terms as motive, purpose, drive, choice, and the like. It is abundantly clear, however, that governing factors are of great and especial significance in thinking, a performance which may, under adequate government, go steadily forward along a more or less fixed course toward an appropriate final term.

SUMMARY AND CONCLUSIONS

The study was prompted by the need for a descriptive account, at once methodical and controlled, of the nature of thinking. Our primary aim has been to determine and to depict all the phases of this characteristic operation and to discover the means employed by man for its initiation and guidance. The experiments provided the observer with a variety of occasions designed to provoke thinking and to carry the elaborative processes through to a conclusion. Only laboratory psychologists who were expert in observation and report were used as observers.

We derive from the results the following facts:

(1) Thinking is a functionally distinct psychological performance in which any combination of the simpler psychological functions may be operative.

(2) Three distinct phases in the total course of thinking are formulation of the problem, elaboration, and solution (or defeat).

(3) The period of formulation of the problem is marked by bafflement and search for the significance of an obscure and puzzling situation. When formulation has been effected, a general procedure for reaching a solution is developed.

(4) Elaboration involves accumulation of psychological products, valuation, critical selection and integration.

(5) Solution of the problem derives from elaboration. The final stage in

thinking may, however, yield only partial solutions or it may end in defeat.

(6) The entire course is directed toward the solution of a problem.

(7) Thinking progresses by the sustained guidance of frequently revised tasks.

(8) Tasks are constantly refashioned by assimilation of previous functional products.

(9) Thinking is difficult and is frequently avoided by resort to less demanding activity.

(10) Numerous products (such as discoveries, decisions, doubts, deductions) issue throughout the course of thinking.

(11) The final stage in thinking may not yield a solution of the primary problem.

(12) Thinking is tentatively described as a distinctive, highly integrated performance involving a directive organization of various other psychological functions together with their products.

A FUNCTIONAL AND DYNASOMATIC STUDY OF EMOVING

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No adequate descriptive account of emoving has ever been published. In spite of the fact that 'the emotions' fill one of the longest chapters in psychology and that emotion is now a favorite subject of research and of speculation, a coherent and first-hand description of what the living organism is actually doing when emoved is still wanting.

The general subject has been approached from four main directions; physiological, pathological, expressive or indicatory, and theoretical.

The *physiological* approach has led, according to a recent study of feeling and emotion, to nine topics or methods of research, all of them aiming at a quantitative account of bodily processes incited by 'emotional stimuli.'[1] The general tendency here has been to resolve emotion into bodily process and state; although some physiological writers have assumed (without defining) their readers' awareness that emotions are conscious things with bodily causes, accompaniments, and modes of 'expression.' The logical slip in 'emotional stimuli' is that they are not energetic agents exciting neural organs but homespun 'situations' from the physiologist's experience which have made *him* 'fall into an emotion.' As turbid a use of the causal concept as one is likely to find in science!

The *pathological* approach has yielded little of value for a descriptive account. The clinician usually borrows his notions of the subject either from the physiologist or (more often) from speculative theories which make the emotions dynamic agents in the causation of mental disease and disorder. Since the clinician's observations are, as a rule, based upon the translation of gross behavior into terms of 'expression' (a set of indicators found by experiment to be almost wholly unreliable, so far as actual emoving is concerned), no sound psychology is likely to receive benefit from the pathological side. Both because of his training and because he is immediately responsible for the patient, the pathologist depends heavily upon the psychological vernacular of the sick-room and easily falls into the folk-belief in mental causes and spiritual agencies. This belief reflects a doctrine of emotion not credited in the laboratories of the careful experimentalist.

A conjunction of the physiological and the pathological approaches has led to the current doctrine of cortical, subcortical and autonomic centers for emotion. The theory would promise more if it were based upon a reliable account of emoving itself, or else were content to be a plain account of bodily areas and processes devoid of 'emotive' pretensions.[2] Again, it is as impossible to know emoving and emotions

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[1] C. A. Ruckmick, *The Psychology of Feeling and Emotion,* 1936, 283-285.

[2] *Cf.* M. Bentley, The psychologist's uses of neurology, this JOURNAL, 49, 1937, 233-264.

without a first-hand command of other psychological functions (especially the other problem-positing functions of thinking and deliberative acting) as to know the reptiles without a cross-reference to the amphibia and the birds.

The *expressive* side of emotive functioning rests upon the observation, as impressive as it is common, that face, mien, posture, voice and movement offer rough but useful indicators of human intent, feeling and conviction. Men communicate by dexterous use of these bodily signs. Darwin's fascinating study of 'the expression of the emotions in man and animals,' as well as the biological theories which have derived from it, have tended to narrow the indicators to those of the face and the indications to the emotive variety. Where they have not known the forms and varieties of emoving itself, physiologists and psychologists have made this rough and ambiguous theory of signs into a substitute for the processes signified. Thus the growl of the dog lashed to the operating table has been read as 'anger' and the trembling of the mouse as 'fear.' That is as if the discovery of red pigments in a wilderness were to be taken as a sure indication that 'red men' were in the region. In the second place, the term 'expression' suggests that it is of the nature of emoving *to express itself,* an assumption quite gratuitous and one which wholly confuses the socialized onlooker with the emoved organism. Had it not been for this confusion, it is doubtful whether we should have had three decades of footless discussion upon the argument whether 'we cry because we are sorry' or 'are sorry because we cry.'[3]

Theories of emotion have been established upon the generalizations of biology. The presupposition that bodily organs and actions are useful was long ago extended to this subject. Emotions have accordingly been found to be designed for defense, warning, display, augmentation of energy, and so on. The more recent theories have dwelt upon the emotive response and its relation to special devices in the brain, endocrines, and elsewhere. Nothing like a classification of bodily resources for the various classes of emotion is to be expected until the emotive modes have been themselves differentially described and definitely related to the psychological functions of perceiving, imagining, comprehending, acting and thinking. Although the psychological theories of Lehmann, Wundt, and some others, which have sought to relate the specificities of emoving to circulatory, respiratory, and muscular processes have moved in the right direction, the facts taken into account, both psychological and physiological, have been too limited for an adequate construction.

We have urged a careful description of the processes of emoving as the central need at present. All four main approaches seem to us to have been blocked at many points by the fact that expositor, theorist and researcher have alike been embarrassed by the want of specific and reliable knowledge upon the nature, the temper, the variety, and the time-course of emoving, as well as of the relation of the emotive operations of the organism to the other psychological functions. It is as if the physiology of organic respiration were to be resolved without knowledge of the course of the respiratory acts and of the chemical participation therein of oxygen and carbonic acid, or the racial history of the hominids without a comparative anatomy of primates.

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[3] *Cf. Feelings and Emotions: the Wittenberg Symposium,* 1928, 17-23; M. Bentley, *The New Field of Psychology,* 1934, 235, 243; A. M. Feleky, The expression of the emotions, *Psychol. Rev.,* 21, 1914, 33-41.

Emotive description shares with the experimental accounts of thinking very great and unusual difficulties of technique and of training. We cannot hope to make more than a bare beginning, a beginning which should, however, be enriched by parallel studies, especially studies in this series of researches, which bear upon comprehending, perceiving, remembering, searching, acting and thinking.[4]

Our experiments have been directed toward conditions simple enough to admit of report and record and complex enough to induce real emoving of definable sorts. The high similarity of emoving to certain forms of the better-known function of acting has suggested that we provoke occasions where the relations to acting were first remote and then intimate. At the end it appears that we might well have reversed the order and so had the support, from the beginning, of the relatively full knowledge which psychologists have acquired over several decades of the variant courses of the several sub-modes—simple, selective, abbreviated, elaborative, etc.—of the actional forms of executive functioning.

So widespread is the claim which emoving makes upon the organism that men have always found it difficult to guarantee a fair sample of this operation and, at the same time, to reserve enough resource to note and to record. The experimenter has had to resort to every kind of device and control. The most popular (possibly because it is the most spectacular) device has been to subjugate and then to bully the subject. Darkness, isolation, strapping, effrontery, paining and terrifying have all been tried. Like a major earthquake, an overloaded battery, or the judicial electric chair, these means have produced results. The kymographic pointer has left the smoked paper in a dizzy swing; sweat has oozed; the pneumograph has snapped; the galvanic needle has swung beyond its range; and profanity has eased the tantrum of a perspiring sufferer. Plenty of results; but what of them? Well; the advocate of expression and of response has found enough of both and he has been at pains to devise cunning experimental means of registration; but what his records 'express' or what the organism has 'responded to' he cannot say further than to refer to an 'emotional stimulus' or to declare that he has been working with one of the textbook forms of 'affect,' usually anger, pain or fear, a trinity of unknown gods for twenty-five years esteemed alike by behaviorist, physiologist, neurologist and clinician.

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[4] For these studies see this JOURNAL, 49, 1937, 343-418; 51, 1938, 253-356; 52, 1939, 161-185. The present writers had the advantage of observing in Dr. Chrisof's concurrent study of thinking.

In our own preliminary and exploring trials we inadvertently fell far enough into the heroic method of capture and torture to discern the extent to which results may be thereby invalidated and also to warn us against its more obscure and subtle forms. In these exploratory trials a predicament was induced by a slight, variable, but presumably innocuous, electric shock delivered on *O*'s forearm. First observations made by the director of the laboratory were carried through without incident; but it appeared after some time that the wide variability of the shock and of the *Os*' limens for pain permitted extremely unpleasant moments which were laid resentfully but without comment against *E*. Unluckily the determination to carry on without complaint submerged the resentment and blocked it from the reports. Only an outside inquiry brought out the fact that an experimenter's slip had delivered into our hands a beautiful emotion of increasing violence, but an emotion which wholly escaped the record. Although these early trials were thus damaged by the enemy of work of this kind upon emotion, they rewarded us by contributing considerable new information upon the rise and character of predicament and the nature and temporal placing of the stage of seizure. They also informed us upon the defects and inadequacies of report in an extremely difficult situation and so led toward great improvement in training and in experimental procedure. A timely shift in method and a frank understanding between *E* and *O* wholly eliminated this error from our main experiments.

SET I. EMOVING WITH ACTIONAL RESOLUTION DENIED

The predicament was induced by slight electric shock which *O* was led to expect when a continuously sounding tone should end. The emotive occasion was built upon the expectation of a slightly disagreeable instant which *E* was unable to escape by such primitive devices as flight, request, or the use of force. When once 'the malicious, capricious and designing experimenter' had been removed from the occasion, the general setting of the experiment served fairly well as a first approach to the problem. It promised to meet the requirements of constancy, control, repetition and report, the most exacting of all requirements in the experimental study of emoving.

Experimental setting. E was seated in an anteroom before a control-board which governed a timing-device (consisting of brush contact on revolving drum, driven by constant speed motor and provided with properly timed, uninsulated sectors), a Harvard inductorium, an audio-oscillator, and a microphone-and-pick-up system.[5] The observation began when a tone of 1000 ~ was sent into *O*'s (soundproof) room. The tone continued for 1 min. Concurrent with the ending of the tone, a weakly induced (shocking) current was delivered through electrodes to *O*'s left arm for 1 sec. A periscope with one-way screen allowed *E* to observe *O*.

O was seated, facing the (unseen) periscope, in a comfortable chair in the soundproof room. To his left arm an ordinary dog-collar electrode was attached. On a stand at *O*'s left were a red light and a microphone, with a loudspeaker beyond.

When *O*[6] was seated and the armband attached the following instructions were read:

> You will hear a tone. When it ends, you will receive an electric shock. When the red light flashes, report (into the microphone) in terms of function and of functional product. Report all functioning in order of occurrence, beginning with the tonal signal. Make your reports complete. Do not fail to report all self- and occasional instructions. If the light flashes while you are making your report, repeat.

The preliminary trials acquainted *O* with the method of experimentation and afforded practice in the art of accurate and adequate report. Before the regular experiments began, the following additional instructions, designed to improve the report, were informally placed before *O* for frequent reference.

(1) Are you sure that you are distinguishing comment and flash?

(2) Are all the perceivings which you report actually separate functional performances or are they moments or adjectival phases in something else? . . . If so, what else?

(3) Be sure to indicate perceptive trains when they occur.

(4) Further define and describe what you have called products of inspecting.

(5) Describe what you mean when you report 'relaxation' or 'relief.' Are these body-objects? Are they products of something other than perceiving?

(6) Describe more fully the 'perceiving' of the tone.

(7) Describe further the perceiving of 'shock' and of 'startle.' What do *you* mean by 'startle?'

(8) Are you sure that you are distinguishing perceiving and inspecting?

(9) Are you sure that you are distinguishing inspecting and comprehending?

(10) You report imaginative anticipating. . . . How is it done? Can you describe it further?

(11) Make a further attempt to give sequence in your reports by saying 'leading to' when that is what you mean, reporting parallel modes when they occur, and indicating independence of one function from another by saying 'period' before going on.

(12) Localize tendinous strains, muscular pressures and the like where they are body-objects.

(13) Make your reports fuller and more exact.

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[5] The apparatus was designed and arranged with the assistance of J. D. Coakley. The standardization of the shocking current was difficult, since resistance varied not only from day to day and with the location of the electrode on the arm but also from observation to observation and from *O* to *O*. Thus a given current reported as a slight shock in one observation might be painful in another. To resolve this difficulty the inductorium was so set for a given period of observation that *O* preliminarily reported a slight, barely painful sting. Many preliminary trials fixed this setting at 6.25-6.50 (on the inductorium scale) for all *O*s.

[6] The observers were Dr. T. A. Ryan (*Ry*), Dr. M. S. Ryan (*Sh*), C. Chrisof (*Ch*), J. K. Macmillan (*Mc*), and I. J. Lacey (*La*).

Results

In the present set, 150 regular observations were made. The reports made it evident that the main emotive patterns evoked were two, each presenting variant sub-forms. The first pattern shows the general sequence,

Predicamentive occasion > seizure > decline.

The second sequence reverses two of these stages, thus,

Seizure > predicamentive occasion > decline.

Although the two sequences appeared irregularly and under the same formal instructions, we shall separately consider samples of each.

The first pattern: PO > S > D. The following reports exemplify the sequence where the predicament precedes and leads into seizure.[7]

P$_{ant}$ (tone) > OI (stop what doing) > Im$_{ant}$ (vaguely of something to happen) . . . *cum* E$_{ins}$ (pressures in chest; tone; arm as above chair; arms cold; ears ringing) . . . per (slight prick in arm) . . . E$_{per}$ (body relaxing; disappearance of tensions) *Sh*-5a

P$_{ant}$ (tone) > OI (stop what doing) > Im$_{ant}$ (something to come) . . . *cum* E$_{per}$ (tone in ear; pressure in chest; visceral tightening) . . . insp (tightened stomach muscles) > per (arm tingling). . . . E$_{per}$ (tightening in stomach; muscles all relaxing) *Sh*-2b

. . . P$_{ant}$ (tone clearer and louder) *cum* S$_{g}$ (straining forward to end of tone) > Im$_{ant}$ (end of tone; shock) > P$_{ant}$ (having lasted long; end in sight) . . . per (body-objects; visceral tightening; tensions in arm; rapid heartbeat) . . . per (relaxing) > SI (get report ready) *Ry*-1c

P$_{ant}$ (tone) *cum* fl ('there it is') . . . insp (same tone) > Im$_{ant}$ (shock coming) . . . *cum* E$_{per}$ (visceral state upset) . . . E$_{per}$ (right hand; arm; heartbeat; abdominal contractions) > . . . imag (arm being intensively shocked) > rem (L's work) *Mc*-2a

P$_{ant}$ (whole experimental sit'n; room; equipment) > rem (previous experimental sit'n) > Im$_{ant}$ (shock to come) . . . per (body-objects; tensing of whole musculature; difficult and heavy breathing). S$_{p}$ (straining forward without goal) *cum* com ("When will it end? Must come soon.") > per (body bracing to resist) > E$_{per}$ (relief) *Ch*-4a

A functional analysis of these reports can best be undertaken by dividing the performance into its stages and carefully describing each stage in its turn.

The predicamentive occasion is devised by the organism through an anticipative cast thrown upon the functional trends in course. In this first emotive stage, perceiving, imagining, searching, comprehending or remembering may take this forward-looking cast, enabling the organism to transcend the actual 'present.' Just as in acting, where *the task* falls within an anticipatory phrase, so in emotive predicamenting is there a futural reaching out, which is, however, anticipative in a peculiar and unique way. Let us see whether we can define more closely.

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[7] A list of the abbreviations used will be found on p. 170 f. above (C. Chrisof, The formulation and elaboration of thought-problems). The use of *ant* for 'anticipation' or 'anticipative,' should be added. The term denotes both a separate function and a functional coloring, as of imagination (*i.e.* Ant and Im$_{ant}$).

The apprehending of the tone provided for in the formal instruction was more than the mere perceiving of a sound. There was signalized either the beginning of an uncomfortable episode, or the opening of a period to end with a specific body-object (pricking arm), or, finally, an ominous and unescapable occasion. Like the first element of a known melody (which is really apprehended with a coefficient placed in time with respect to subsequent elements), the aural occasion was here sharply defined as marking the initiation of a highly specialized phase of psychological activity and leading on anticipatively to an undesired outcome.

The first specific means which we here discovered were the *flash* and the *comment*. Here are instances taken from the first stage:

P_{ant} (tone) *cum* com ("I must listen") > fl (it is coming) > . . . *Sb*-1e

P_{ant} (tone) > fl (it is crowding on) > P (body-state: chest presses) . . . *Cb*-6d

P_{ant} (tone) > fl (it is soon to end) . . . *Ry*-2d

Both flash and comment are ready and compact functional resources helping to pack the awaiting-period with predicamentive foreboding. It appeared at times that condensation went still farther to a simple perceiving of 'this is the going-on-to-the-inevitable-prick tone.'[8]

Remembered objects and occasions themselves—strange as it may seem—sometimes acquired futurity stamped by a foreboding expectancy and thus set the stage for an oncoming seizure. A daily instance is the remembered insult which contributes to an onrushing resentment. Under our conditions, remembering was always directed upon situations analogous to the present, which it significantly supported. It entered, as an integral moment, into the predicament.[9] Here are experimental instances:

Rem (previous shock) > Im_{ant} (dread of coming shock) . . . *Cb*-2b

Rem (shock in last obs'n) > Im_{ant} (shock to come) . . . *La*-1g

Rem (ref. to previous obs'n) > Im_{ant} (shock going to come soon) . . . *Ry*-2a

R_{in} (of shock last obs'n) > Im_{ant} (shock to come) . . . *Sb*-3c

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[8] W. S. Neff found (this JOURNAL, 49, 1937, 376-418) several stages or degrees of perceiving leading toward a quick and bare abstractive form which was quite remote from the more primitive, concrete and 'pictorial' kind. Looking in another direction, M. L. Lemmon (*ibid.*, 51, 1938, 304-356) noted perceiving thinning out, without accessory processes, into a symbolizing or figurative variety.

[9] Most of our revised and 'prospecting' memories fall under Erb's Type i and Type ii (M. B. Erb, The memorial forms of apprehending, this JOURNAL, 49, 1937, 343-375). Type i: "An occasion or event which bears a particular and explicit temporal and autobiographical reference"; Type ii: "A specific event or scene which bears a vague and indefinite temporal and autobiographical reference." We also found a memorial *flash* resembling Erb's Type iv.

The anticipative form of imagining plays a very important rôle in emoving of the present pattern. It is a sort of matrix into which plain perceiving (or remembering) of the present (or of the past) occasion is imbedded. While remembering by itself may advert to like former occasions and perceiving impart a central significance to the immediate occasion, anticipative imagining frequently incorporates the entire train of functional events and yields a prophetic product.[10]

One further moment, the most difficult of all for reporting, must be considered before we are done with the first, predicamentive stage. It concerns means used by the organism to produce the 'foreboding' aspect of anticipation. Over and above remembering, imagining and perceiving of the sound and its sequels, body-objects, body-states and body-events play their important part. We consider them as well-defined or obscure perceptive products. In the following examples, we indicate them by P_{e}, where the subscript suggests the peculiar 'emotive' smart, ache, or dread which qualifies perception.

$\mathrm{Im}_{\mathrm{ant}}$ (twinge to come) *cum* P_{e} (heavy chest: aching arm) . . . *Sh*-2e
$\mathrm{P}_{\mathrm{ant}}$ (approaching twinge) *cum* P_{e} (diffuse body-strains and tensions . . . *Ry*-4f
$\mathrm{Im}_{\mathrm{ant}}$ (arm-twinge) *cum* P_{e} (visceral and respiratory events . . . *Mc*-5f

At times, this moment was qualified by a SI which intended the abolition of the 'emotive' body-objects, etc.:

$\mathrm{P}_{\mathrm{ant}}$ (arm-twinge) *cum* SI (relax and cut out the dreading anticipation) . . . *Ry*-2e

At other times, the mere product 'rising dread' appears in the report without the somatic components.

And still again, this moment in the predicament appears to have been prepared for by a bare antecedent searching (S) set up by warning signs before ever the tonal sound comes:

S (restless foreboding, unattached to any per or im) > P (the tone) *cum* S (as earlier) > $\mathrm{Im}_{\mathrm{ant}}$ (it is coming) . . . *Ch*-1b

Or the antecedent searching may be supported by a bit of free imagining or remembering. Thus:

Im (looks of E as he sits in the outer room preparing the shock) *cum* S (foreboding) > P (the tone) > $\mathrm{Im}_{\mathrm{ant}}$ (this will be painful) . . . *Mc*-3b
and

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[10] This anticipative moment has been studied by one of the present writers (this JOURNAL, 51, 1938 253-260) as it appears in the actional forms of executive function. In the present shorthand, the moment is set down both as $\mathrm{P}_{\mathrm{ant}}$ and $\mathrm{Im}_{\mathrm{ant}}$ according as it inclines more to the perceptive or the imaginational side: In the imaginational form, the presentness of the occasion is nearly or quite suppressed. In extreme cases, the perceptive form, on the other hand, may bring back to the apprehended present the shock-prick before the shocking circuit has actually been closed.

Rem (the last was severe) *cum* S (restless foreboding) > P (the tone) > P_{ant} (the twinging) . . . *Sh*-4c

Finally, the predicamentive stage sometimes led to the first part of an action, which was designed for an escape but was followed by a SI to suffer the inevitable.

. . . Im_{ant} (it is coming) > act (pull away the arm) > SI (keep quiet) . . . *Mc*-4d and *Sh*-3i

The seizure. The outcome and consequent of all that has preceded in the formation of the predicament may be grossly described as a baffled agitation. In many common forms of emoving this second period runs over again into the proposing of means for removing the predicamentive occasion or the seeking of an escape from it. Under our conditions, which tended to exclude the third (actional) stage, the baffled agitation or 'seizure' is nevertheless left. We may not conclude, however, that seizure here would be the same as a seizure which led quickly into an actional attempt at resolution. Let us see what character our seizure possessed and how it came out of the preceding stage which we have just now tried to member and depict.

There are two general characteristics of the predicamentive phase which should throw light upon seizure; the first is its *occasional quale* and the second is its *anticipative trend.* It is primarily a confronting occasion, an occasion to be feared, resented (as by angry rejoinder or reproof), rebuffed, suffered (as by embarrassment, humiliation, self-condemnation or confession), contended against, and so on.

Many occasions are problematical but not emotive or predicamentive. Some carry within them the promise of resolution by thinking (thought-problems), and others resolution by doing (action-problems). As all three sorts of problems are related through many intermediate forms, they may be regarded as forming the great psychological family of problematical functions.

With respect to the second characteristic of emoving, we may observe again that all the great problematical functions are anticipative; all look ahead and all alike advance toward solution, remodeling, or defeat; but each in its own way.

Now 'seizure' also stands related both to an occasion and to anticipation. The ardent thinker knows the frenzied state of an exciting problem, and he has likewise his prophetic moments when the end is in sight and the thought-answer lies on the tip of his tongue. So also with the actional occasion and the actional advance which runs ahead of accomplishment and forecasts the completed deed. Our main interest, however, concerns the active relation of *emotive* seizure to the anticipative occasion which precedes it.

Since it is peculiar to the emotive occasion that it is a bafflement, a situation which offers no immediate release and still cannot be immediately dropped, we might well expect a further demand to be made by the organism upon its general resources and reserves. Again, this demand is by way of being met as emoving proceeds by virtue of the anticipative trend of physiological and psychological function. That which is forecast but not completed in the predicament is bound to color the succeeding stage of seizure.

We shall find, as we might well expect, that the demanding occasion and the forward-running, but unfulfilled, anticipation lead on to a general somatic extension and intensification, precisely to those factors which would create a wide variety of body-objects and body-events.[11] Here are evidences—abundant in our reports—from the stage of seizure.

. . . P_e (shallow and rapid breathing, heavy heart-throbs, abdominal strains) . . . *Sb*-2b

. . . P_e (tenseness of arms, clammy palms, hot wet forehead) . . . *La*-2h

. . . P_e (pulling up in visceral regions of abdomen) . . . *Mc*-3c

The many and diverse body-products recovered by report doubtless owe their somatic origins to processes of respiration, circulation, digestion, secretion, transpiration, contraction, and so on, which are under neural incitement, but which are, outside emotive seizure, either too weak or too much employed otherwise to enter into the psychological functions here considered and reported.[12] Since kinesthesis and coenesthesis are both far commoner than our emotions, it looks as if their emotive contribution depended primarily upon a particular physiological and psychological setting. We should therefore carefully consider them as moments in a total time-course of quite specific construction.[13]

Our experiments strongly suggest that—to put the problem in more concrete terms—the seizure-phase of emoving (in the type of emotive sequence which we have thus far considered) is an inevitable sequel to the baffling and forward-tending predicament upon which the emotive product is based. The phase is greatly enriched and widely extended in specificity by the somatic components which we have cited and which appear both in vulgar recitals of emotional seizure (the straitened chest, the dry mouth, etc.) and in trained report. To extract these components one by one, to set them down as discrete sensations and feelings making up the state, or to regard them as creatures of the hypothalamus and the autonomic, is to make a caricature of the type of function which we here inspect. Even in forming as careful a functional report upon this difficult matter-of-fact as the trained psychologist can command, there remains the danger that such an item as 'catching the breath' or 'clenching the fists' will be over-interpreted as a separate or separable moment in a panicky-dread-of-being-pained.[14] Such items are undoubtedly actual, but they must be set into the context and regarded as a part of the integrated functional flow.

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[11] Here a physiologist's account of events which did not confuse bodily description with a gross depiction of psychological products would greatly clarify our problem. Nothing satisfactory is at hand. The theory, recently popular among physiologists, of adaptive secretions which augment bodily energies has never been successfully applied to emotions at large. The choice of anger as a typical emotion is not impressive since the alleged 'angry' states fall much closer to a special class of *actional* functions calling for excessive strength and agility than to anything which can sensibly be called emotive.

[12] For a fuller account of body-objects and the like, see H. C. Werner, A descriptive study of somatic objects, this JOURNAL, 51, 1938, 225-234.

[13] This view stands of course against the neurologist's view of emotion as fixed and determined by certain cortical and subcortical centers and peripheral organs and contrivances.

In divining the real part which these moments play in emoving at the seizure-stage, we must make allowance for the analytical effect upon the function from inspecting, whether inspecting results from an instruction (formal or self-initiated) to 'note critically' or from the sheer impressiveness of such symptoms as the trembling of terror or the aching set jaw of impotent rage. At times these body-objects are powerful enough in their own right to cut across and even to dissolve the predicament. The case is just the same as the noting of the headlights of a following automobile when the light shed on the road ahead grows sufficiently strong. The notion that bodily postures, muscular turns, and 'vital twists' serve as 'gauges' and 'indicators' to inject flashes of doubt, denial, query, certitude, encouragement, and the like, into elaborative thinking will serve us also in emoving. In so far as these somatic products are compacted into the bodily resources of the seizure, so far do they tend to disappear as inspected products of perceiving and to contribute to the entire functional trend of the moment and to its single psychological product. To them we may then extend the term 'indicatory transfers.'[15]

Decline. In our experiments, the pungent twinge in the arm from the electric current is the end toward which the searching, anticipating, and all other functional events are aimed, but it by no means marks the end of emotive functioning, which does not immediately vanish with this perception. Actually this body-event is no more a 'mere perception' to *O* than the sounding tone was. It is something like a 'there-it-has-come-at-last: now-it-is-over.' In other words, this event marks a new turn in the emotive course. It begins the stage of decline. Seizure now fades toward a memorial echo and new turns of bodily function give coloring to 'relief.' Inspection at this point reveals the body-products that contribute to decline. Among the most prominent items are the feel of muscular and tendinous relaxation, with constant changes in respiration, heart-beat and general posture. The same 'transfer' or 'translation' out of perceived body-objects and events and into 'decline' that we found in seizure occurs again here. The great concurrent change in psychological functioning is the disappearance of the anticipatory reference and all that it contains.

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[14] The danger is still greater in neural physiology where the naïve notion flourishes that a block of these somatic processes, taken together with their central inciters and sequels, *is* the emotion.

[15] For the primary references to such indicative or gauging moments in elaborative thinking, see M. Bentley, *The Field of Psychology,* 1927, 355-357; *The New Field of Psychology,* 1934, 305. Further experimental study of these subtle but important moments in the emotive course is urgently called for.

For this stage we have the following excerpts from reports:

. . . Per (relaxed trunk and arms) > fl (there it is) *cum* com ("it is ended") . . . *Sb*-2d

. . . Rem (the tone *was*) > per (dropping in visceral region: arms relax) . . . *Mc*-3b

. . . Insp (tingling in arm) > comp (tingling was akin to tickle); com ("I must report") . . . *Ry*-2b

It is obvious that such reports as these are inadequate to the functional significance of the declining phase of the emoving. The main psychological product ('predicament is no more: this tingling sharpness ends it and absolves me') tends to disappear behind the inspected details.

Modal variations. In a fairly large number of cases the fundamental mode $po > s > d$ suffered internal changes. The simplest change was the reinstatement of a second predicamentive occasion just after seizure. At times this reinstatement led to a second seizure, and at other times to further repetitions of the double phase $po > s$. These variants suggest, concordantly with daily experience, that the disturbance in functional course easily leads to a baffled alternation of phases in which the unsettling occasion may arise again and again, thus augmenting and prolonging the seizure.

The second emotive pattern: $S > PO > D$. All men know this form of emoving, which frequently begins with a violent automatized movement of the body and of its members, as from a sudden loud noise for which the organism is not immediately prepared. Seizure strikes; to be followed by a perception or understanding of a predicamentive, usually a dangerous or annoying, occasion. This mode of reversed phases appeared in our experiments when the tonal sound acquired a 'startling' character which immediately evoked the seizure before any corresponding occasion was fabricated.

The following extracts will serve to show the order:

P_e (tone: start of body forward; pressures in chest and ears) > Im_{ant} (something coming) . . . *Sb*-1d

P_e (tone: muscular contractions in abdomen; eye twitching): P_{ant} (tone) > Im_{ant} (shock coming) . . . *La*-2c

Modal variations. Again the swing between first and second phases, sometimes with a second seizure following the tardy occasion and sometimes with two or more alternations in sequence before the final decline. Another variant entirely omitted the predicamentive occasion, decline immediately setting in after the initial seizure. Possibly this should be regarded as the simple startle, though everyone who suffers from this hot and unpleasant sort of upset-without-warning will agree that it usually possesses a decidedly emotive coloring. Wherever a human perpetrator of the 'startle' is suspected, the sufferer is inclined to fill-in an intermediate phase of predicamentive occasion charged with helpless resentment against the annoyer.

SET II. EMOVING WITH ACTIONAL RESOLUTION ENCOURAGED

The experiments next to be described were designed to provide a predicamentive occasion which *O* might resolve by way of an appropriate actional train. The procedure accordingly arranged for the insertion of a penultimate stage, *attempt at resolution,* in the functional performance. Our central observational task was to be the careful description of this penultimate stage as it grew out of the preceding predicament and seizure. We were to discover, however, that the experimental setting and character of the task assumed by *O* greatly changed the entire performance, both in complexity and in functional temper.

The task set by *O* for solution was the picking out on the musical keyboard of a certain melodic fragment which would prevent the sharp body-tingle event from the electric shock, otherwise to be administered to the arm.

Experimental setting. The general arrangement was the same as before, save that *O* was seated before the modified piano-keyboard of a reed harmonical. The same tone as before was sounded for 1 min. Thereafter, at a variable interval, the shock might (or might not) be administered. The playing of a familiar melody required no technical ability beyond the picking out of a few initial notes. The melodies to be used were independently submitted by seven persons not related to the experiments. They were to be the titles of songs 'known to everybody.' Those finally selected by *E* had appeared in six of the seven lists. When seated before the harmonical, with the electrode in place on the left arm, several of the song-titles, clearly typed on separate cards, were placed face-downward in a pack near *O*'s right hand. The formal instructions were given orally:

> When the tone begins, you are to play (on the instrument before you) the melody indicated on the topmost card in the pack. You have until the tone ends to play the tune correctly. If by the time the tone ends you have correctly worked out the pattern of the melody, the shock will not be delivered.
>
> When the red light flashes after the tone has ended, make your report in terms of function and of functional product. Your task is to make a complete report, item by item, omitting nothing that falls between the beginning of the tone and the flashing of the red light.
>
> When your report has been received, the red light will flash three times. You are then to remove the top card and to go on to the next in the pack.

E was the judge of the correctness of the performance. He was thoroughly acquainted with the melodies to be played and had had enough musical training to pass upon the value of the playing. *O* was required only to work out the general pattern of the melody to avoid shock. *E* placed a liberal interpretation upon the performance. The shock used was within the range of intensity determined by the standardization of Set I. When the playing was unsatisfactory to *E*, the shock was delivered shortly after the tone had ceased.

In the second half of the set the formal instructions were supplemented as before in order to improve the reports. The supplement ran:

> (1) When you report, be sure to title correctly your functions.
> (2) Be careful that you do not confuse acting with movement and its issues.
> (3) Do not blanket products of other functions under the term 'perceiving.'
> (4) Make sure you sharply distinguish an independent function from adjectival coloring.
> (5) Describe the stage after the tone has ended more fully than you have done.
> (6) Describe fully the nature and composition of 'predicament.'
> (7) Describe the bodily aspects of your performing.

The set includes 160 observations, preceded by a training period to acquaint *O* with the harmonical and the formal instructions and also to permit *E* to determine *O*'s skill in manipulation. The intent was to administer the slight shock with reserve and discretion, although it might at any time be anticipated by *O*.[16]

The possibility that the ensuing functions might be actional in character makes a comment on the varieties of action in order. Where the *actional task* was incorporated in our reports, it usually corresponded to Longwell's 'specific' sub-form, referring prospectively to *O*'s manual use of the keyboard.[17] Less frequently appeared Longwell's 'explicit' kind (this particular melody is to be played), and from time to time various 'secondary' tasks (indefinite, definite and elaborate).

First we shall review, as independently as the facts will permit, that aspect of the entire course which introduces the actional task, always remembering that this task is built up and governed quite as importantly by the occasional and self-initiated forms of instruction as from the formal variety contributed verbally and otherwise by the experimenter. In our setting, the main occasional instructions came from the apprehensional grasp of room, cards, song-title, harmonical, flashing light, and so on.

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[16] Illness prevented observer *Ch* from completing the last half of the set.

[17] See S. G. Longwell, Progressive change in simple action; a study of learning, this JOURNAL, 51, 1938, 261-282. The reader who is not acquainted with the executive functions at large will find comprehensive accounts in M. Bentley, *The New Field of Psychology*, 1934, 209-265, 396-400.

Here are samples:

. . . Per (light) > OI (turn card and look) > insp (card title) *cum* fl (familiar > imag (vocal rehearsal) . . . *La*-4a

. . . Per (title) > imag (tune: schematic group of words; further notes; entire phrase): per (tone) > OI (play the imagined product) . . . *Ry*-2c

. . . Per title) > imag (one phase of tune; first note as d'): per (tone) > act (determination to play; playing d') . . . *Ry*-3b

. . . Per (light) > OI (turn card > per (title) *cum* com ("I know it") > rem (occasion this was sung) . . . *Mc*-1c

This phase of the actional mode sets into commission the determination, *i.e.* the carrying forward of the task. In our reports it is opened by imagining, remembering, and other functional moments which point toward resolution, after occasional and self-instruction have set the task. The forward-running determination provides solidity and continuity. Longwell has stressed its anticipatory cast, but it is also necessary to emphasize the manner in which it marshals the products of previous performing and incorporates them into the activity. This is well exemplified in the reports below.

. . . (carrying through OI to play) *cum* imag (of tune running ahead) > SI (find first part of tune) > imag (notes accompanying first words) > SI (play them) > act (playing from d' under SI) . . . *Ry*-2c

. . . imag (of tune) > S (complete the phrase): insp (imagined tune; first note about a' > SI (start on note a'; play without sharps and flats) > (playing of phrase) *cum* insp (almost correct) > SI (insert half-steps) . . . *Ry*-1a

The final phase of the actional moments is resolution of the task through movement. But in our actional trains, while a single task remained, the entire performance was divided into segments, each of which held determinational and resolutional phases. Determination is constantly recast and is followed by new motor activities in each segment. Released and directed by perceiving, inspecting, and searching, each determinational phase runs into a motor activity which is specific.[18]

It is the rule for an underlying determination to be present straight through, representing a permanent general task, but to be repeatedly specified and reënforced.

There were instances where nothing more than a very slight predicamentive shading (anticipative perceiving or imagining before or when the one-minute tone began to sound) was added to the action, which then ran a characteristic course to the successful rendition of the melodic fragment. In these cases, the final resolution of the action did, to be sure, hold a kind of relieved ejaculation which brought the products 'fine'; 'that's right'; 'I've escaped the penalty.' This might be called a triumphal resolution, a triumph really sweetened by the initial foreboding that 'this may turn out badly' or a surrogative remembrance of earlier trials where the melody failed and the twinge fell.

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[18] Bentley has portrayed this variety of acting as Formulation of task > Progress of determination > Resolution and new apprehension > Progress of determination > Resolution and new apprehension > Progress of determination (*New Field,* 217).

These instructive instances, where only the shadow of an emoving crossed the functional course, require only one more comment, a comment to explain how and when "the slight predicamentive shading" passed into plain and unimpeded acting. It appears from the *Os*' reports that the striking of the first note or notes of the melody suddenly brought the change, and that this change wore a double aspect. On the one side, it characterized the actional determination ('this is the way it goes'; 'I have it'; 'I can do it'; 'aha! I am safe'; 'how exciting!'), and on the other side, it was the fading out of a foreboding uncertainty which nevertheless so lingered as to lend a positive coloring to the first phase of the free action. The whole change in tone of the performance was often sustained by bodily changes which brought (under inspection) such reports as 'quickened breathing,' 'springy thrills through the shoulders,' 'a relaxed throat,' and the like. Those who number *relief* among the emotions would call these bodily objects and events evidences of this emotion; but if we hold to the predicamentive occasion as the core of emoving, we shall have to regard this moment as a sequel to, rather than a constituent of, emoving, *i.e.* as a notice that emoving has come to an end.

Now we turn to those reports which disclose *a really predicamentive foundation.* We here find the activity turning decidedly emotive under three conditions; (1) failure to set the actional task; (2) failure to establish a suitable actional determination; and (3) miscarriage of the manual performance upon the keyboard. The conditions fall thus into the given temporal course, *i.e.* the task is not set; it is set but not translated into a prophetic determination; when set and determined, it miscarries in the stage of resolution.

While we go on to examine these cases, we must keep before us the well-attested fact that an actional performance *may* fall short in all these ways without turning emotive. Take ordinary piano-practice itself. The learner may stumble in a difficult passage (1) through neglect of the teacher's directions for fingering and so miss the task; (2) in his anticipative attack upon the approaching passage, or (3) through fumbling as he strikes the successive notes. All lead to bad and balked performance, but not one of them necessarily leads to emoving; although any may, indeed, when circumstances (*e.g.* great fatigue, discouragement, or the teacher's reproach) favor. What is the difference? Unless we mean to beg the question by falling back upon the 'emotional situation' of the behaviorist as a causal agent, we shall have to be content for the present to say simply *context* and try later to define what that 'context' is which distinguishes an actional emoving from a disturbed acting.

(1) Failure in tasking. The task, as *O* attempts it, is impossible either by virtue of its difficulty or because it is unfamiliar. The course of events then takes the following form:

Per (light as OI to begin > [card turned] > comp (title unfamiliar) > fl (not going to get this) > Im_{ant} (will get shock) > P_e (visceral stirring; shallow breathing) > R_{tn} (for title) > rem (hearing song) > SI (remember the tune) > S (to imagine) *cum* com ("I don't know this") > Im_{ant} (shock) > P_e (highly agitated body-object) . . . *Sh*-4e

Per (title) > com ("don't know this") > Im_{ant} (of shock) > P_e (body-strain objects; difficult breathing) . . . *Mc*-2e

[card exposed] > per (title) > comp (don't know this) > insp (title unfamiliar) > S (for melody) > Im_{ant} (shock to come) > P_e (trembling legs; straining arm-muscles; visceral excitement; time passing; tone-end) . . . *La*-3f

We note that the observation-period contains attempts to formulate the task, the organism calling upon various functional aids. The period is abortive; nothing is accomplished. A tense searching for the melody is set up. *O* is helpless; he awaits the impending shock. The predicament consists here in *O*'s inability, at a certain juncture, to formulate correctly the task and to provide a basis for determination and resolution. We may represent the general procedure as Attempted tasking > Predicamentive occasion > Seizure > Decline.

A variant of this course occurs when brief, ill-founded, and unavailing actional determinations or movements appear in spite of inadequate formulation of task. Such determination and movements invariably lead to a renewal of the predicamentive stage and a more intense seizure. The course is extremely difficult to diagram, since the stages intermingle and overlap.

To exemplify:

Per (card) > comp (title) *cum* com ("My God, I don't know this!") > rem (many tunes) > SI (do something) > Im_{ant} (shock coming) > (random beating at instrument) > P_e (tone end; gripping strain in trunk; heart quickening; intense general strain) *cum* Im_{ant} (of shock) . . . *Mc*-1b

Per (tone) > per (kinetic contraction; straining forward) *cum* [exposing card] > comp (title) > imag (vocal rehearsal) > insp (melody wrong) > S (for rest of melody) *cum* com ("don't know it") *cum* Im$_{ant}$ (of shock) > P$_e$ (general bodily seizure) > SI (take it easy) > S (for rest of melody) *cum* com ("don't know it") *cum* Im$_{ant}$ (shock to come) *cum* P$_e$ (bodily tenseness; trunk excitement; trembling legs) . . . *La*-1c

We all recall incidents where emotive performing has resulted from an insufficiently clarified or formulated task. The actional determination rarely gets under way, and time is consumed in performance directed toward a clearer exposition of the task. Help is sought in remembering and imagining; unrelieved searching strains for the solution, and the sum is a thwarting occasion accompanied by an organic crisis.

(2) Failure in determination. Here an imperfect actional forecast fails to provide for resolution of the task through movement. Instead it leads to an anticipative imagining of shock and to the agitation of seizure. Proposals for movement are considered and rejected. Movement never comes about. The main rôle is played by searching. Imagining is also called upon for aid, often emerging from search. SI is prominent, and the entire course consists of a series of attempts to move forward to the stage of resolution. Out of the unrelieved searching arises a predicament, and seizure follows.

Per (title) > comp (melody familiar) > S$_{im}$ (what *is* the tune?) > SI (get going): > S$_{im}$ (the tune is?) > imag (briefly of first phrase) > S (to confirm imagined product) > insp (imagined product) *cum* com ("Is that right?") > SI (start playing) > S (for place to begin) > Im$_{ant}$ (shock to come) > P$_e$ (general bodily upset; end of tone; body-objects) . . . *Ry*-4f

Per (light) > OI (turn card and begin) > comp (title familiar) *cum* OI (to sing it) > S (for tune): per (tone) > S (for tune) > S (to imagine V singing it) > imag (part of tune) > S (get it placed) > imag (briefly of tune) > S (get melody) *cum* Im$_{ant}$ (of shock coming) *cum* P$_e$ (tenseness throughout body; pressures in chest) . . . *Sh*-3a

The outstanding fact here is that movement never comes off because determination is defective. The formula should stand, then: Task>Abortive determination>Predicamentive occasion> Seizure>Decline.

In a modified form, predicament and seizure are followed by a brief new attempt at resolving, which again gives way to predicament and another seizure. To exemplify:

. . . S (for melody) > Im$_a$ (vocal rehearsal) *cum* fl (not right) > S (again for melody) > Im$_a$ (vocal rehearsal) *cum* insp (right tune) > OI (play it) > S (for beginning) > Im$_{ant}$ (shock coming) > fl (won't get it) *cum* P$_e$ (bodily agitation; palms sweating; heart faster; excited body-object): SI (try it) > [attempts at playing] > Im$_{ant}$ (self-receiving shock) *cum* P$_e$ (heart fast; pulse fast; sweating) . . . *La*-3e

. . . S (to imagine tune) > SI (I get it right) > SI (imagine) > S (to imagine) > Im$_{ant}$ (shock at end) > P$_e$ (shallow breathing; pressures in stomach; heaviness of limbs; tensing muscles): > SI (try to play) > (random striking at keys) > Im$_{\text{ant}}$ (shock is coming) . . . *Sh*-2e

. . . S (to identify first note) > SI (imagine first note) > S (for note) > Im$_{\text{ant}}$ (shock coming) *cum* com ("I won't get this") > P$_{\text{e}}$ (muscular tension: trunk-objects) > SI (play) > S (for first note) > SI (hurry and play) > S (for first note) > Im$_{\text{ant}}$ (going to get shock) *cum* (excited body-object; muscular bracing for shock) . . . *Ry*-3e

Note the use here of powerful self-instruction to destroy the predicament and to restore the actional course.

(3) Failure in resolution. This is the commonest variety met in our melodic set. Task and determination are both present, but the movements of resolution are ineffectual. Let us examine examples:

Per (title) > imag (melody) > S (to imagine clearly): per (tone) > SI (play intervals) > [playing intervals] *cum* fl (wrong) > [playing other intervals] *cum* fl (wrong) . . . [playing] . . . comp (can't play tune) > [striking out at random]: > per (end of tone) > Im$_{\text{ant}}$ (coming shock) *cum* rem (poor performance) *cum* P$_{\text{e}}$ (train of body-objects) . . . *Ry*-4b

. . . [picking at keys] *cum* fl (not right) > SI (go on) > S (to imagine) > SI (pick it out) > [playing] > fl (wrong) > [random picking at keys] *cum* P$_{\text{e}}$ (intense bodily agitation; body-objects) . . . *Ry*-4c

. . . [pressing keys] *cum* OI (lower) > [pressing lower key] > insp (right) > [playing] *cum* insp (wrong) > [playing]. per (end of tone) *cum* com ("that's over.") > Im$_{\text{ant}}$ (shock coming) . . . *La*-3g

In the first report the task is correctly formulated and unhesitatingly accepted. Task enters by way of formal and occasional instruction and is given aid and clarification by the products of imagining, searching, and inspecting. The actional determination is consistent and persevering. Frequent self-instructions (to continue, to get it correctly, to make other attempts) color all the reports, while forward-tending searching assumes a prominent place. The movement resolution, however, is at fault. *O* discovers (fl), by way of perceiving and inspecting, that his playing is incorrect, and this leads him imaginationally to anticipate shock and to suffer seizure. In many cases the breakdown of the performance is so complete (as in the first two reports above) that *O* hits out at random, indicating a complete disintegration of the coördinated actional train. Because these attempts at resolution prove inadequate and deficient, therefore, the emotive outcome follows. The crucial point—and this holds generally for actional emoving—lies in the functional setting. The apprehensional type of anticipation found in our non-actional emotions of Set I is here wanting; nevertheless there is about the present performance an aura of futurity common to all incomplete acting, an aura which derives from the determinational provision for resolution through movement. The resolution may fail, in our experiments, either because *O* lacks skill and musical knowledge necessary to pick out the notes or because the occasion suggests an impending shock-object at the arm. In the first instance, the actional problem simply ends; in the second, the occasion sets up its own futurity which cuts across and disorganizes the action. Acting then disintegrates by virtue of the competitive train of bodily and psychological events leading toward the impending twinge.

The key to the emotive functions. Before we conclude and summarize our studies, we should relate them to the general field of research in which they lie.

There can be no doubt that seizure is the most impressive aspect of our emotive experiences. Impressive it is whether viewed as the spectacle of a hominid or other animal exhibiting violent, agitated, or unrestrained behavior; as the trembling, throbbing, muttering and perspiring of one's own body out of control, or as a shorthand means of indicating or expressing to another one's undone and agitated states. Nevertheless, our attempt to regard emotion as one of the great psychological modes of bodily performance, arising under assignable conditions, taking a specific course, and ending with inevitable issues, has led to experimental results which make *the predicamentive phase basal and seizure secondary and derivative.*

The history of the subject, from Darwin, Lehmann, Lange, and James to recent times, points unfortunately to seizure as the characteristic of emoving that has almost wholly determined theory, doctrine, description, classification, and research. Given this natural, if thoroughly naïve, absorption in seizure, it is easy to discern two major interests of the last seventy-five years which have been mainly responsible for our emotive history. The one interest derives from the biological theory of adaptation (what *is* in behavior exists because it has usefully conformed the living being to life's necessary conditions) ; the other interest derives from morphological theory (life depends, individually and racially, upon the organism's structure and configuration). The first interest interprets the processes of life, the second its physical constitution.

Darwin emphasized the first in his contention that the outward evidences of seizure give the clue to many behavioral resources of the body. Thus his 'laws of expression.' Those in search of body-seats of emotion, on the other hand, have delved—like ardent phrenologists—into thalamus, hypothalamus, adrenals, and autonomic, to discover the secret place of seizure. Lehmann and hundreds of his followers have tried to catch on graphic record and oscillographic display, the operational drift of seizure in muscular, respiratory, circulatory, cerebral and glandular processes.

Lange stood especially for circulatory and respiratory causes, while James emphasized the skeletal muscles. With his interests and sympathies divided between physiology and folk-beliefs about the mysteries of life, James capitalized the dramatic possibilities and shocked the psychic causalists by supposing that the physical states might underlie seizure and so *cause* the emotion as felt or suffered, instead of symptomatizing or 'expressing' it. The effects of this resounding casuistry would never have echoed through Europe and America for a half-century had anyone ever discovered what emoving actually is; discovered, that is to say, as taxonomist, astronomer, physiologist, embryologist, and the comparative student of languages discover the kind, character, and course of the objects and processes which they examine.

Summary and Conclusions

Our pioneering studies of emoving have sought an experimental approach to one of the main psychological modes of operation of the living body. Always we have taken advantage of prior descriptions of those forms of psychological activity which are better known, especially perceiving, remembering, imagining, acting, and searching. Furthermore, we have constantly found indispensable aid in a comparative examination of the three great 'problem-solving' functions of acting, emoving, and elaborative thinking, for underlying all these are pronounced and significant similarities.

Finally, the present chaotic state of our subject, in whatever context it is examined, has admonished us to keep in mind the general historical setting. The main facts of this setting we again review for the sake of our summary and interpretation.

The historical approaches to the study of emotion, distinguished in our introductory paragraphs as physiological, pathological, theoretical and expressive, show the very great influence exerted in each and all by the powerful doctrine of biological utility and the ancient assumption of inherited local bodily seats. Physiology and pathology have generally stood for specific processes, normal or diseased, running their singular courses in fixed and local parts and organs of the head and trunk; the more speculative theories have held that the emotions are either sheer functional breakdowns or else veiled devices like instincts, desires and habits bestowed for purposes of adaptation; and the expressionists, setting seizure in the midst, have made the pantomimic display of posture, pose, mien, and facial cast the 'expressive' indicators of this central feature.

That no one of the four main approaches leads toward a really psychological description of the emotions should be obvious from the direction of their incidence as well as from the issues of many decades of research undertaken since Darwin wrote his *Expression of the emotions in man and animals.*

The study has shown, as we think, that it is possible to secure a descriptive (we should not say a 'photographic') account of at least certain of the emotive functions of the body with precisely the same empirical methodology as has been successfully used in all of the other primary operations. The method implies special training, a technical vocabulary of the operational or functional sort designed for events in course rather than for fixed objects, and a disposition to avoid doctrines and explanatory conceits derived from other disciplines, notably from speculative neurology and biology. While its own description is not made in terms of anatomy or of the physiologist's processes of the body, the method finds a place for all *factual* accounts of the living organism which bear upon the psychological functions. These bodily facts are of the first importance once the psychological operations have been described, because only bodily agents and forces (not psychical or mental mechanisms) are to be admitted as terms of energetic explanation.

In our first set of experiments we examined a type of emoving which was as remote as possible from acting; while in the second half emoving appeared in many instructive admixtures with closely related actional forms.

The main issue of Set I was the discovery of the nature of the predicamentive occasion (the pivot of emoving) and of its functional relations to the stage of seizure. While the experimental situation led to variable ligations in time as between these two emotive phases, it has appeared that in the complete-in-itself emotive course the organism characteristically involves itself in a dramatic way through the creation of the predicament, and that the predicament resolves itself into the consequent phase of seizure, which, in its turn, suffers a natural decline and gradual obliteration.

Since it is of the functional essence of emoving that the demands of the predicamentive occasion cannot be satisfied, as the actional problem is, by appropriate movements of body or limb, we sought in Set II instances where a train of more or less resolving movements entered to temper the emotive course. In some instances, emoving was practically eliminated by the prompt playing of a melodic fragment suggested in the presented song-title; practically, but not wholly, for always there was the foreshadow of the stabbing twinge which meant that the playing of the notes might not reach its goal. In other instances, there was more of a demanding and dramatic occasion, which set deeper the emotive coloring of the performance.

Here our most important problem was to see how the emotive function distinguished itself from the actional. The solution lay in those turns and shadings depicted in our reports which made the primary occasion predicamentive. The anticipative moment is obvious. We have noted the 'aura of futurity' in the occasion; but this moment is more than a simple prophetic forecast which also attaches to the determinative aspect of acting. The occasion rather becomes a future in which the emotive observer is implicated as by a threat. The event *impends* in the sense that it holds a foreboding for the individual. That is to say that it holds a certain inimical value for the self-object.

Taken all together and set against the extensive and significant background of history, the experiments here written down have directed the present writers toward three main conclusions.

(1) The first conclusion is that the subject of 'the emotions' may well be taken from the half-explored region of arid and frozen *things*, physical and mental, and concretely examined as a kind of *event* among other events through which the living organism constantly moves. So considered, emotion appears as an emoving, *i.e.* as a specific and well-marked function bearing a striking resemblance to the other resolving functions of elaborative thinking and acting.

(2) The second conclusion is that emoving may properly be called 'psychological' both from the nature and course of the event itself and from the character of its issues and products.[19]

(3) The third conclusion, drawn quite directly from our observational reports, is that no single item, detail, feeling, stimulus, or response is to be identified with emoving, which is an entire integrated train. This organized train it is which makes the tone-heard, the anticipative imagining, the searching, the flashes, the signals, the blundering movements over the keyboard, and all the other details, really emotive.[20] This is what we have meant by 'context.' It is as much a caricature of emoving to resolve it into a perception or a feeling as to dismember it into a 'sensation with its aura of affect,' to name it a response to a situation, to confuse it with assumed

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[19] When 'products' have been confused with inciting conditions the whole problem of emoving has miscarried. To speak of an 'emotional situation' (a thing to be created by the organism) as the *cause* of an 'emotional response' is a plain *petitio.*

[20] No demand is here made upon *Gestalttheorie* and other popular doctrines which elevate totality and the unitary aspect of things into an explanatory cause.

subcortical connections, autonomic systems and visceral violences, or to identify it with 'expressive' movements and neuro-electrical potentials.[21]

These conclusions imply neither a classification of emotions at large (a minor and secondary problem) nor a general survey of the emotive field. Most of the competent research in both of these directions remains to be done and done with better methods and a clearer envisagement of problems than the last half-century has provided.

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[21] An adequate description of the bodily resources and sequels of emoving remains to be written. It can hardly be written until an empirical account of emoving itself has been taken over and understood by physiologist and pathologist. Competent descriptions of bodily organs, systems and processes, without an appeal to the frozen entities of the emotions, are another matter. They would be an accomplishment of the greatest service to the psychological student of emoving.

THE PSYCHOLOGY OF SOCIAL CULTURE

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A decade ago it appeared that social psychology, under the influence of behaviorism, had finally attained scientific status as an explanation of social interaction in terms of stimulus and response. Among the laws and principles which it employed to account for social behavior, the chief were those of conditioning, particularly to social and symbolic stimuli, and of habit formation. Closely related to habit, and in some respects scarcely distinguishable from it, was the conception of social attitude, a generalized habit of preparatory neuro-muscular adjustment to a social stimulus operative to a large extent through the vocal symbolism of speech. Focussing as it seemed to do the fundamental principles of behaviorism for an attack on the problem of the everyday interactions of individuals in society, the concept of social attitude became the leading theoretical tool of the social psychologist, and experimental investigation in this field of recent years has consisted mostly of an attempt to measure and define social attitudes. This investigation has brought to light some very interesting facts, interesting and no doubt important in many cases, but whether the results have been commensurate with the energy expended remains an open question.

There is no doubt, however, that the application to the social field of the principles above-mentioned, and others logically linked with them in the behavioristic approach, has greatly enlarged our understanding of certain departments of social behavior, notably the initial stages of learning and the routine adjustments of orderly social life. But they furnish no explanation for other features of social life and conduct held by earlier social psychologists to be distinctive and outstanding. Such features, for example, are the participation by individual members in an inclusive social, or group, experience whose comprehensive organization is in some degree reflected in the experience of the associated individuals, and the influence and authority of social ends or values in shaping the opinions, determining the decisions, and coördinating the activities of individual members. One need not wish for a return to this earlier approach, or for a reinstatement of the social mind or consciousness in this branch of psychology, to be convinced of the importance and authenticity of these features of social life. Hence, one may welcome as a sign of real progress the

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emergence of new facts and new views which indicate the importance of immanent order, organizing values, and functional relationship in the field of social life and behavior, and demonstrate the inadequacy of the theoretical framework of stimulus-response behaviorism to account for these features.

It will prove helpful, therefore, to remind ourselves at the start of some of these contemporary advances in socio-psychological theory which bear directly upon the psychology of social culture.

By his studies of the mental growth and social relations of children, Piaget[1] has shown that intellectual development and social adjustment go hand in hand: the child passes from a primitive ego-centrism in which he perceives and imagines things only in relation to his own actions and interests to that of social objectivity, by the discovery of logical relations between things and classes of things which coördinate the perspective of different individuals in the social and really objective world. It is not without significance, furthermore, that concepts of psychoanalytic theory, like identification, introjection, and projection, are winning their way to quite general acceptance among social psychologists,[2] despite the fact that they are not easily reconcilable with behavioristic interpretations. This is because, besides being supported by clinical findings, they give genuine assistance in explaining the social adjustments of infancy and early childhood.

The chief contribution of the Gestalt school to social psychology lies in the development of the field of theory of social behavior by Lewin[3] and Brown.[4] The basis for this theory is found in the alleged fact, for which these psychologists have gathered a considerable body of evidence, that when dynamic or motivational attitudes are aroused in the field of social behavior, an object present in the situation of an individual and related to some organic need arouses in him a tension which is not merely intra-organic but directed outward in a line of stress upon the external object, and that this goal-directed tension determines the grouping and organization of objects in the individual's total field of perception and action. Not unrelated to this insight is the view recently advanced by F. H. Allport[5] that the behavior of the individual (as an expression of his personality which has always a social reference) is best understood when described teleonomically. This is the describing of an individual's behavior in terms of the purpose or purposes he seems to be carrying out, of what he seems to be 'trying to do.' Allport is explicit in denying that this implies teleology in the ordinary sense of postulating an end or goal antecedently in the consciousness of the individual which impels him to acts that accomplish some result beyond the acts themselves. The end or goal in terms of which it is proposed to characterize the act has no place except in the act itself. In making use of such description, we mean only that if we were performing the movements made by the individual we should say we were doing thus and so. Since the words which the agent speaks are a part of his total behavior, these may be used as evidence of the correctness of teleonomic description, not of course at their face value but in relation to the environmental circumstances and the rest of the individual's behavior.

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[1] T. Piaget, *The Moral Judgment of the Child,* 1932, 1-418; Principal factors determining intellectual evolution from childhood to adult life, in *Factors Determining Human Behavior,* Harvard Tercentenary Publications, 1937, 32-48.

[2] G. Murphy, L. B. Murphy, and T. M. Newcomb, *Experimental Social Psychology,* 1937, 189.

[3] K. Lewin, *A Dynamic Theory of Personality,* 1935, 1-286.

[4] J. F. Brown, *Psychology and the Social Order,* 1936, 1-529.

[5] F. H. Allport, Teleonomic description in the study of personality, *Char. & Personal.,* 5, 1937, 202.

Value, whose significance, says the writer of a currently published work on the psychology of personality, "has been ignored by psychologists until relatively recent years,"[6] is given a central importance by Sherif who treats the customs, laws, traditions and standards of society as socially established values.[7] Social norms or culture-products are, as he points out, at first external to the individual and act upon his organism from without, but not as discrete sensory stimuli present and operative among those furnished by successive social stimulus-situations. Such situations are always perceived as relational wholes, the result of the organization of external and internal factors of stimulation. Social culture-products also—language, music, architecture, furniture, tools, etc.—have a structure, an order and organization of their own, which is directly perceived as real.[8] Understanding of the meaning of such objects, at first only partial, increases rapidly in the first years of life. As the result of responding thus to the culture-products dominant in his environment, the individual interiorizes the norms of his society. Once appropriated they exert, as Sherif emphasizes, a constant influence upon his perceptions and attitudes as 'frames of reference,' *i.e.* internal factors which to a large extent predetermine the relationship and context of things as they appear in the field of perception and action. Agreeing with Sherif in fundamental principles L. B. and A. G. Murphy[9] have attributed the sterility of much research on the social behavior of children to the 'atomistic' approach which has treated the child's act as a response, isolated from the child's total personality and detached from the situation in which it occurs, to a stimulus standardized for uniformity in experimental procedure. They recommend as the only valid procedure the method of functional analysis which recognizes the high degree of interdependence of situation and response, and defines behavior traits not as response elements but as aspects of the responses of whole organisms to whole situations.

Anthropological research has lately established the fact that the various elements in a social culture—the different institutions, traditions, magical and religious rites, art-forms, etc., comprised in it which had been previously studied singly and by the comparative method—prove when examined in their relation and as composing the total culture of a particular group, to be functionally interdependent and to constitute a logically and practically organized system. The social (and psychological) import of these discoveries is indicated with due scientific caution by Malinowski.[10] All the elements in a primitive culture, he affirms, prove on investigation to be functionally related. These are no idle survivals or disconnected traits; in even the most queer and sordid customs there is a core of rational and social principle. The vast machinery of culture is maintained and regulated by a body of traditional lore. This is made possible by language which enables man to formulate general rules and condense them into concepts. Thus to systems of action there correspond systems of knowledge which exist even among the lowest primitives. Knowledge, magic and religion are answers of primitive man to the fundamental realities of life. Necessary to remove internal conflict and to organize the community, they become essential factors of spiritual and social integration. Benedict[11] and Mead[12] have made a deep impression upon current thought in social and abnormal psychology by their dramatic portrayals of the culture patterns of primitive groups whose social behavior they have studied. These writers go somewhat further in finding a psychological rather than a biological or economic explanation of the organization of primitive cultures. Culture patterns, they hold, by virtue of their definite configurative sets, act selectively and creatively in accepting, rejecting and re-modelling personality traits, i.e. related sets of attitudes which have individual consistency.[13] Hence it follows that with each culture-pattern there goes in the primitive community an approved type of average or ideal man. Toward the development of this type, the educational systems and all the prevailing institutional attitudes are adapted. Deviants from this type are regarded as abnormal, and are treated as such, abnormality having no meaning except in terms of the approved and authoritative personality-pattern.

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[6] R. Stagner, *Psychology of Personality,* 1937, 181.

[7] M. Sherif, *The Psychology of Social Norms,* 1936, 1-210.

[8] *Ibid.,* 32, 56 ff.

[9] L. B. and A. G. Murphy, The influence of social situations upon the behavior of children, in *Handbook of Social Psychology,* 1935, 1051-1093.

[10] B. Malinowski, Culture as a determinant of behavior, in *Factors Determining Behavior,* 1937, 133-168; *Crime and Custom in Savage Society,* 1926, 125.

When we consider the total import of these recent contributions to psychological theory which stress the importance of the principles of functional interdependence and organic interrelation in every department of social behavior, one thing at least is clear. This is that any attempt to explain such complex products of social life and activity as systems of traditional belief and ceremonial observance, industrial methods, political procedures and artistic creations, in terms of stimulus-response interaction in which stimuli and responses are linked causally as serial events in nature, is foredoomed to failure—the failure that awaits upon all such attempts at over-simplification in the interests of scientific explanation. The result of inter-activity between organism and environment is, in the case of the human individual, to constitute and define for him relational wholes in the way of perceived and imagined objects, purposively effective actions, and emotional-aesthetic experiences. These objects, because they are *not* supplied as identical imprints of the same physical stimulus-pattern to all individuals in the range of its influence, require to be communicated if they are to compose a common social world. As we know, this is accomplished through the associating, by the mechanisms of social conditioning, of these objects, procedures and experiences with a variety of minor motor responses, chiefly verbal, but also in the way of tentative indicatory movements and changes of facial expression and gesture. Because socially conventionalized and acquired in infancy by all group-members, such responses—particularly, of course, language, which is elaborated *pari passu* with the expansion of the social world—act as stimuli capable of re-presenting (through nervous mechanisms not well understood) both to the individual who makes the response and to the one who hears or sees it, the objects with which it is associated. Thus they serve as means of communication, and upon such communication the development of a social culture is entirely dependent.

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[11] Ruth Benedict, *Patterns of Culture,* 1934, 1-291.

[12] Margaret Mead, *Sex and Temperament in Three Primitive Societies,* 1935, 1-335.

[13] Margaret Mead, The use of primitive material in the study of personality, in *Char. & Personal.,* 3, 1934, 10, 12.

More may be learned, however, from an examination of the products of social culture than that they presuppose the intercommunication of the varied results in the way of response-objects which follow in the case of different individuals from the processes of dynamic re-adjustment continually going on between organism and environment. It is reasonable to expect that an examination of the distinctive characteristics exhibited by these products of group culture will throw light upon the organic activities instrumental in their production. Failure to make use of this essential source of information lessens the value of Mead's able analysis of the psychological factors involved in human social life and coöperative activity.[14] This failure is the more striking in Mead's case because of his initial and convincing argument to the effect that all association of the specifically human sort depends on communication which is "responsible for the appearance of a whole set of new objects in nature," and through which alone a living organism attains social selfhood.[15] By keen and penetrating analysis in terms of organic behavior, Mead derives the language-symbol from the animal-gesture and shows the biological function of the latter. But when we ask what it is which, when communicated, gives distinctive character to genuinely social behavior, his answer is 'attitudes,' organizations of different parts of the nervous system that are going to be instrumental in producing acts, and which therefore are capable of representing not only what is immediately taking place but what is going to take place. In such an attitude, the act as a whole can exist in the nervous system determining its later stages, by it can be represented alternative courses of action, and responses to classes and kinds of objects, *e.g.* the characters we assign to 'horse' as idea or concept.[16]

Now it is undoubtedly true that attitudes are thus communicated. But just as certainly, objects of perception and thought, and plans of action in their characteristic quality-patterns and spatial, temporal and means-end relations, can be communicated. The equivalent of these objects can be formulated, if desired, in terms (partly hypothetical) of nervous organization, but not without the loss of that structure of differentiated quality and relational pattern which is a key to their vital and social significance. The primitive tribesman who boasts to his fellow of his success in inventing some new weapon or device for killing or capturing an animal does communicate something of his own attitude of self-confidence and self-congratulation, but if his communication is to have much social interest it must reveal something of the nature of the new weapon or plan of attack, and of how it works. Or if it is a magical spell whose evil effects on himself he is lamenting, the content of his communication cannot be exhaustively described for purposes of social psychology in terms of his attitude of hostility or of resignation, but must include some facts concerning the character and operation of the supernatural agency of injury and death.

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[14] G. H. Mead, *Man, Self and Society,* 1934, 1-401.
[15] *Ibid.,* 79, 172 f.
[16] *Ibid.,* 11.

To the scientific student of the cultural institutions of man, who is desirous of learning what psychological activities engage in their production, it is bound to seem an abortive and illogical procedure directly to convert these culture-products into attitudes and habits of social adjustment. The straightforward and reasonable method, if one seeks such psychological explanation is to begin with an examination of these cultural objects themselves, and to hope to find in their own characteristics the needed clue to the psychological processes and activities operative in their production. When we confront the endless variety of social culture-products, of economic, familial, political and educational institutions, of traditional beliefs, of magic and religion and recreation, this seems indeed a formidable undertaking. But concerning even this vast and diversified array, it is possible to make a preliminary generalization—a generalization which on first mention does not seem to go far beneath the surface, but will furnish at least a starting-point. This is—not, to be sure, that all these varied culture-products are neatly divisible into a few groups—but that they do exhibit three distinct modalities, practical, cognitive, and aesthetic. The first is clearly predominant in industrial methods and forms of political organization, the second in the body of traditional lore, and the third in the various forms of fine art. Other culture-products, such as kinship-systems and family-organizations, religion, magic, education and recreation, show no one of these aspects to be outstanding, but all three present and inter-penetrating. And no social institution or culture-product (this is a fact to be emphasized) is exclusively practical, cognitive or esthetic; even those most distinctively characterized as one or the other, such as systems of ideas or beliefs, or fine-art creations, possess in some degree the other two modalities in some degree, and all three intimately united.

The presence of these three generic features in varying degree in all forms of social culture points quite plainly and unmistakably to three activities of the human organism as operative in their genesis. In terms of organic behavior, these activities are known to psychologists as verbal, kinetic or motor, and emotional or visceral responses, and so far as they signify the main sorts of adaptive habit, are recognized to be the three vehicles of social behavior. Such descriptive labels are useful in indicating the classes of response-organs exercised by the activity in question. They are too general for purposes of this discussion, however; since they suggest nothing concerning the social function of these activities and, unless this is taken into consideration, it is impossible to be sufficiently definite and apposite in pointing out the specific organic structures which they exercise. If we call them the 'capacities' of articulate speech, of manual-motor contrivance, and of emotional aesthetic expression, this is with no thought of reifying them as faculties, but of connecting specific organic responses with well-defined social functions.[17]

In the case of the first, articulate speech, the connection between definite anatomical structures and the social function of influencing the behavior of others through communication is evident and well-understood. Responses of the specialized bodily organs of vocal utterance, supplemented by gesture and eventually by handwriting, develop through exercise and social learning into language-habits. The social stereotyping of speech-responses and their integration into the elaborate symbolism of language is, as we well know, one of the principal bio-social sources of social culture. The second of the three capacities under consideration stands in need of more detailed and precise description, however. To call it motor or kinetic is quite inadequate to its function in the development of social culture. It is the activity of practical contrivance employed in the fashioning and use of tools, the manufacture of weapons and implements, clothing and habitations, and (with the assistance of speech) in the marshalling of individuals for the preservation of order and discipline, and for group-defense, attack and migration. Primarily of manual, manipulative character, it may be looked upon with respect to organic source as a response of the human hand (with opposed thumb peculiar, as Darwin emphasised, to the human species), assisted by connected muscles of the wrist and arm, and made effective by the erect posture which frees the hand for manipulation and brings it under easy guidance by the eye, and by the mechanisms of bodily locomotion which move it to places of most advantageous application. Likewise the third capacity is insufficiently described for our purposes as visceral. While emotions may originate as responses of visceral muscles and glands, it is not as a visceral response but rather by virtue of its overt bodily expression that emotional activity influences social life and culture. In fact, no more definitely descriptive name suggests itself for this activity than emotional expression, or perhaps emotional-aesthetic expression. This includes responses of the facial muscles in what we call the expression of the countenance, and also the movements of gesticulation, posturing and exclamatory vocalization. The word 'aesthetic' was added because these emotional expressions soon become attached by social conditioning to their observed appearance in the perceptual patterns presented by others' behavior. Hence these expressive movements become not only means of relieving organic tensions but of communicating them to others. It is to be further noticed in this connection that emotionally expressive responses, such as the movements of gesticulation, posturing and vocal exclamation, pass over by degrees into those of dancing, dramatic and pictorial portrayal, and song, whence they develop in due course into the various branches of fine art.

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[17] L. B. Murphy, *Social Behavior and Child Personality,* 1937, 321.

Let us turn back now to the varied forms of social culture and see whether their intrinsic properties and relations do not throw further and very important light upon the organic processes instrumental to their production. Perhaps the most obvious relationship embodied in and displayed by culture-products, a relationship particularly prominent in the wide field of economic institutions, is the means-end relation. The relation of implement or technical procedure to the result or end it accomplishes is the outstanding feature of industrial tools and methods, of weapons and tactics of warfare, and of many political procedures and the ritual performances of magic and religion. Malinowski remarks[18] that he received his first intimation of the character, power and influence of Malenesian magic when he heard the loud chant of the Trobriand magician in the midst of the terrific uproar and commotion of a tropical hurricane. He then realized that while the professed purpose of the magician was to quiet the storm, the real psychological function of his performance was to restore confidence and optimism among the people. In its simplest form, as illustrated by tools, weapons, and practical methods, this may be called the means-end relation. As exhibited by more complex and highly developed culture-products, such as machines and technical methods and manufactures, it appears as the relation of joint-efficacy, the relation of diverse materials and forces to the unitary result to whose accomplishment they severally contribute.

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[18] B. Malinowski, Culture as a determinant of behavior, in *Factors Determining Human Behavior,* 1937, 157, 158.

The attempt has been made by psychologists committed to mechanistic theory virtually to eliminate this means-end relation as a dynamic factor in social life and activity by explaining all means-end adaptation and tool-learning, even among men, in terms of trial-and-error reaction in the case of the first, and of the establishment of symbolically conditioned habits of motor response in the case of the second. But this attempt may be confidently said to have failed. As has been well stated, in describing the procedure of invention in the field of material culture: "The lucky accident is not enough, it must be seen by a person who has in mind some problem to solve."[19] Experimental studies have proved that insight into instrumental relations frequently governs constructive play and tool-learning with very young children, and, so far as the use of material objects as instruments is concerned, is sometimes operative in the behavior of apes. Relevant to the question are also the conclusions of Tolman[20] and fellow-investigators that, in order to explain the learning of animals much lower in the scale of nervous organization than the ape, we must attribute to the organism of the animal (the maze-running rat) certain 'means-end readinesses' which enable it to respond to environmental objects as stimuli, in their instrumental or tool-serving, relation to the goal.

A second outstanding feature possessed, or relationship exhibited by the products of human social culture, is consistency and coherence of 'meaning,' *i.e.* of logical structure and pattern. This of course applies particularly to the intellectual tradition of the group, the body of ideas and beliefs accepted and handed down by successive generations. But while pertaining chiefly to the ideational field, this relation of logical consistency or pattern extends its influence over many fields of social practice, such as religious ceremonial, family organization, marriage customs, and even (in the prescriptions and taboos it sanctions) over such routine matters as the choice and preparation of food. It is a fact, well-established but still needful of emphasis, that orderly systems of concepts, defined and employed with a fair degree of consistency to represent classes and kinds of objects, so far from being exclusively a by-product of advanced culture and intellectual sophistication, play an important part in the social life of some, possibly many, primitive groups. Such an ideological system may provide the theoretical basis for an elaborate division and subdivision of the tribe on a totemistic basis into exogamous groups, or for a complicated method of reckoning relationship and transmitting name and authority, or it may supply the mythological foundation and framework for magical rites and religious ceremonial which stimulate and direct coöperative effort. No doubt some of these primitive ideologies relating to agriculture or animal husbandry have definite economic value, and it cannot be doubted that a body of traditional lore, commonly believed in and understood, would increase mutual understanding and confidence, hence be functionally effective in strengthening the coöperative tendency. But the most striking evidence of the reality and importance of this feature of consistency and coherence of generalized (conceptual) character which man finds in his world when he responds to it intellectually is furnished by the tenacious adherence of primitive groups to customs and practices in their total effect practically disadvantageous, because they follow logically from some traditionally authorized scheme of social relations. Thus according to Margaret Mead[21] the social life of the Mundugomor suffers great injury because of their adherence to the principle or conception of the "rope" in organizing family life, a system which neither organizes people into patrilineal or matrilineal groups, but is based on the theory of a natural hostility that exists among all members of the same sex and the assumption that the only possible ties between members of the same sex are through members of the opposite sex.

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[19] C. Wissler, Material culture, in *Handbook of Social Psychology,* 1935, 560.

[20] E. C. Tolman, *Purposive Behavior in Animals and Men,* 1932, 29, 95.

The organic activity through which man responds to consistency of generalized character and permanence of pattern in the world, by constructing ideological systems, is that of articulate speech. Speech enables man to respond to the attributes and qualities which identify classes and kinds of objects, and to the patterns of order and relation which these objects present, by giving them names. Thus he obtains for these products of intellectual analysis and abstraction, concrete symbols in the way of internal motor responses capable of unlimited variation, abbreviation and combination among themselves, which are capable (through mechanisms set up by kinaesthetic conditioning) of acting as stimuli to arouse the patterns of brain-activity which represent these significant features of the real world. While experimental results may indicate, as a recent work informs us[22] that logical consistency plays a minor part in determining what attitudes or cultural interests are held simultaneously by the individual members of civilized society, this is in large part due to the impact of new and arresting forms of social stimulation against which the intellectual powers of the poorly educated majority are able to make little or no headway. Taking human history as a whole, there is no room for doubt as to the influence of social tradition when reinforced by congruent institutional stimulation, or that, other things being equal, this influence is proportionate to the logical order and consistency of the established system of beliefs.

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[21] Margaret Mead, *Sex and Temperament in Three Primitive Societies*, 1935, 176.

The products of primitive art used for the embellishment of social, ceremonial and religious ritual, and for the adornment and decoration of implements of industry and weapons of war, clothing, family habitations and communal houses, bring to our notice a third type of relationship. This is the patterning of colors and textures, of forms and movements, of tones and rhythms, into schemes or designs whose order and arrangement afford pleasure both by virtue of their own intrinsic properties and because of the objects of common human interest and concern which they suggest to the imagination. Without attempting an incursion into the field of aesthetics, it will be sufficient for purposes of the present discussion to mention two facts which are, as I think, evident and beyond dispute. One is that beauty in art-products depends upon their possessing in some degree certain objective properties, such as order and organization, symmetry, proportion and balance among their diverse sensory components. The second fact is that these properties, familiar and indisputable, connect themselves with activities of emotional expression in the human organism. It has been already noticed that the various kinds of artistic creation, the dance and song, dramatic portrayal and pictorial representation, are outgrowths of the emotionally expressive responses of gesture, bodily movements and posturings, vocal cries and verbal repetition, accent, etc. By means of these overt responses the individual expresses strong feeling, and, through the perceptual patterns which they arouse in others, communicates to them the emotions which stir him. This aspect of the aesthetic experience has been fully treated by Langfeld in an article wherein he states: "There is little doubt that aesthetic expression is the best means for the communication of affective states and therefore one of the most fruitful sources for the study of such communication as well as for an examination of the states themselves."[23]

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[22] G. Murphy and others, *Experimental Social Psychology,* 1937, 1031, 1045.

[23] H. S. Langfeld, The place of aesthetics in social psychology, *Brit. J. Psychol.,* 27, 1936, 141.

We may imagine, as the starting-point of this particular cultural development, an individual in the throes of strong emotion and by its expression furnishing himself with visual, auditory and kinaesthetic stimuli. If, on occasion, the pattern of sense-imagery presented, or the sequence and order of vocal sounds, of own bodily movements seen and internally sensed, was such as to arrest attention by its own satisfying properties, to increase and stabilize pleasurable emotion and to offset or divert painful emotion, he would tend to repeat it with resulting fixation in habit. Such patterns would tend to be imitated through ordinary processes of conditioning, particularly by those affected by the same emotion. If extended, elaborated and refined by technical contrivance, they would become incorporated in the cultural heritage, musical, dramatic, decorative, etc., of which later generations would avail themselves in the expression and communication of emotion. The descriptions of Benedict and Mead have supplied us with impressive and convincing illustrations of the importance of art in giving unity, imparting individuality, to different primitive cultures. We think of the Zuni tribe whose life has centered in the artistry of socio-religious dance and ceremonial, and of the Tschambuli who live principally for art and among whom the women are considerate and good humoured enough to provide the opposite sex with the economic wherewithal in order that each man may be an artist.

When we review thus briefly the outstanding features or (as it has been convenient to call them) relationships exhibited by the main classes of culture-products, it appears that they do throw light upon the nature and bio-social function of the three organic activities responsible for their production. The foregoing paragraphs will have been seriously misleading, however, if they have seemed to imply that each of these three activities of articulate speech, practical contrivance, and emotional expression worked in independence of the other two or was alone operative in any major department of human culture. Of course, just the opposite is true. The three work in closest interdependence and are jointly effective in every field of social achievement. In the work of mechanical contrivance and social invention, men deal in anticipative forecast and methodological routine with classes and kinds of materials and arrangements, forces and processes, verbally symbolized and unceasingly talked over. They are influenced in the credence they give to old beliefs and new conjectures by the practical results achieved when they act as these ideas suggest or direct. The artist must exercise a high degree of technical skill in the manipulation of his materials if he is to obtain the desired aesthetic effect. The coöperative employment of skilled handicraft in the production of masks, effigies and decorative details to beautify some religious ceremonial has its inspiration in mythological lore of which workers and watchers are continually reminded. Men talk (as well as whistle) while they work; their hands are busy with routine tasks as they discuss affairs of this world and the next. What is affirmed, therefore, is only that each one of these three activities is dominant in a certain major department of social culture because it responds effectively to that feature or relationship of external objects with which this branch of social achievement is primarily concerned.

In view of the evidence thus supplied, we cannot doubt that each one of these three activities of conceptual verbal formulation, practical invention and emotional-aesthetic expression, when persistently and skillfully employed, may become an independent and original source of satisfaction to the human individual. It follows then that culture-products have values of their own, based on the fact that objects of the external world possess properties and disclose relations which answer to the efficacy of these three activities—in addition to the values they derive from the fulfilment of organic and economic need. This would mean that the weaving of the accumulated observations and experiences of generations into a coherent and logically formulated tradition has its own intrinsic interest and value, that the power gained over physical forces and over animal and human behavior-tendencies through the invention of technical instrumentalities is likewise an independent and authentic source of satisfaction, that music and dancing, dramatic and pictorial representation, are delights in themselves apart from any ulterior purpose which they may subserve. There is evidence to show, in the opinion of many psychologists, that any effectively organized adaptive response, regularly exercised and firmly intrenched as habit, acquires a motive force of its own. But the activities under consideration are much more than examples of complex response-integrations stabilized as habits through social training, of which there are many in the field of social behavior. Each of the activities in question exercises a specific set of bodily response-organs, inter-adjusted to serve a single bio-social function, and the combined effect of the operation of all three is to bring into existence a type of social life distinctive of the human species which depends upon the communication of verbal interpretations and emotional experiences, and the coöperative exploitation of the resources of the natural world.

So much may be affirmed, without at all denying that these cultural activities have in the course of social history been mainly employed in the service of organic and utilitarian need. Men were first prompted to recall and talk over past experiences, to make plans and to discuss practical ways and means, in order to insure their own safety and a continuation of those enjoyments attendant upon the satisfying of primary appetites. They devised implements and weapons, they contrived methods of tilling the soil, of domesticating animals and of constructing houses, in order to obtain food, clothing and shelter. Music, dancing and dramatic performance were engaged in, masks, costumes and decorations were introduced, to enhance the effect of religious ritual which had the social utility of intensifying the feeling of corporate unity and enthusiasm needed for coöperative effort in hunting and warfare. And such intrinsic satisfactions as these cultural activities themselves yielded were only incidental to the successful discharge of the routine duties of industrial and family life. It is only at a late period in social evolution and in culturally advanced societies that knowledge, practical power, technical mastery, and beauty, are sought as intrinsic values, or ends in themselves. But it in nowise follows from these undeniable facts that distinctive cultural interests do not exist or that they have not been in some measure operative as motivating factors throughout the whole history and development of man's social culture. Consequently, it is impossible to agree with a view held by not a few social psychologists that in all the higher intellectual and artistic vocations the autonomic drives of hunger and sex are the fundamental motivating force.[24] Or with the assertion that the goal-functions of culture are the satisfaction of the four wishes for security, response, recognition and new experience.[25]

Field-theoretical explanation, as formulated in the works cited from Lewin and Brown, has special advantages when applied to the problem of the part played by values in social behavior. Field-theory understands the behavior of human individuals in terms of the structure of the organism-environment field as constituted by the dynamic interplay of physical and social factors. Such an approach is illuminating in explaining the function of values, including the values of social culture-products with which we are concerned. First, because it makes values objective, converting them into 'field' forces. No longer sequestered in the consciousness of the subject, they operate as positive and negative valences, the attracting forces of goal-objects and the repelling forces of barriers or obstacles to goals. And not only are values themselves objectively operative; they determine the dynamic organization of the whole field, *i.e.* the parts played by various objects in it. In such a field, certain objects derive position, prominence and meaning from their relation as ways or means to the goal; other objects derive character and importance from their relation as obstacles or barriers to the goal. Furthermore—and this is the vital point in the present context—not only does value determine the objective organization of the behavior-field for the agent projecting a course of action; it does so also for other individuals in the field who observe his behavior. They observe his behavior (to repeat the term recommended by F. H. Allport) *teleonomically.* They perceive his actions in their transitional, or functional, relation to the result required to round out and complete them, in their 'gap-closing' function. So understood, values take their rightful place as objective factors operative and observable in the world of common perception and action.

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[24] See *e.g.* F. H. Allport, *Social Psychology*, 1924, 65.

[25] T. K. Folsom, *Social Psychology*, 1931, 493.

Field-theory has a second advantage of enabling us to explain how a group of individuals can participate in the realization of a common set or system of values such as those of social culture, without having recourse to a hypothetical social consciousness of an inclusive sort in which all share. Such a participation on an objective basis becomes a simple empirical fact, so far as we directly perceive (not infer) what others are trying to do; the aimfulness of their performance. Brown refers in illustration to the way in which, in the economic class-struggle, the divisions between classes constitute barriers which possess common negative valence for all members of the inferior class.[26] To understand the bearing of this upon participation in the values of social culture, it is essential to bear in mind that the crucial fact here on which the whole argument depends, *viz.* that we perceive others' behavior in functional relation to the result which completes it, applies not only to the observation of outwardly directed action or motor performance, but also to perceiving what they say and how they feel. When talking with an acquaintance I do not, except in rare cases, first listen to and perceive his words, then translate these verbal symbols into terms of my own conscious experience and finally reach a conclusion as to their meaning. Instead, I respond to the sounds of his voice by perceiving or knowing the objects or relations to which they refer. The words may be few, grammatically disjointed and fragmentary, but sufficient to evoke in me the dynamic brain-patterns which present to me the objects his thought is concerned with. So also with the expressive movements of others; we perceive them dynamically, in terms of the instigating objects toward which the organic and personal tension displayed in his behavior is directed.

The type of explanation to which reference is made has one limitation, however, when utilized to account for the influence of the values of an organized social culture upon individual behavior. The principles of field-theory as formulated by its authors have been developed, at least in the social field, almost exclusively in connection with the processes of social adjustment through overt action, while the values of social culture are dynamic factors in the cognitive and aesthetic fields as well. Thus Brown, who has dealt most extensively with the social applications of field theory, characterizes the activity of individuals in the social fields as social locomotion. He makes it quite clear, to be sure, that by locomotion he does not mean physical movement but rather the sequence of changes by which the individual reaches his goal, and by freedom of social locomotion the number and diversity of goals open to attainment by the individual in the social world.[27] But the social activities with which he is almost entirely concerned in his detailed exposition are those of inter-group adjustment where goals are attainable only through overt action and motor performance. The limitation in question is perhaps more clearly apparent in the distinction made by Lewin and Brown between different degrees, planes or dimensions of reality in the psychological activities of the individual. According to Brown the plane of fundamental reality is that of perceiving and work; other planes of less reality are (in order of decreasing reality) conceptual thought, phantasy and dreaming.[28] It is true that in an earlier context he makes the reservation that thinking, or even day-dreaming, may in certain circumstances have a higher degree of reality than perceiving. But so far as social behavior is concerned, it is plain that the primary function assigned to conceptual thought and imagination is that of substitute or compensation for the more real activity of overt social adjustment. The plane of fundamental reality in social life is that of perception and work. Now most assuredly the compensatory function of 'rationalization' and day-dreaming in social behavior is highly important. But to neglect the part played by conceptual thought and constructive imagination in ordering and stabilizing social relations on more real and satisfying levels is seriously misleading. One thinks of the advance from the earth-centered and man-centered view of the world (which remains that of common perception) to the conceptual cosmic system of modern science which is the basis of modern civilization.

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[26] *Op. cit.,* 137.

The truth is that conceptual thought, so far from removing those who exercise it from the actual world of social life and intercourse, is prerequisite to coöperation among human individuals in any real sense of the world. This is the view confirmed by Piaget's investigations, which have shown that intellectual development from the ego-centrism and animism of early childhood to the objective and mechanistic view of the later period, proceeds concurrently with increasing social coördination which culminates in the capacity for genuine coöperation. For voluntary coöperation among equals (which as Piaget emphasises is the foundation of democracy in social and political organization)[29] requires that the individuals engaged, while preserving severally their own perceptual perspectives and unique interests and aptitudes, shall have, as a common field of action, a world of objects whose relations are defined and ordered similarly for all. It could not be otherwise, since only thus could a group of freely acting individuals pool their original efforts on the common working basis required for productive achievement under the conditions set by the determinate sequence of events in the external world. Analogous factors shape the social situation in the field of discussion. Exchange of ideas and information, of opinion and belief, through the symbolism of speech, would not be interesting or fruitful except for individual differences in outlook, experience and interpretation, which make such communications worth while and create whatever flavor and piquancy they possess. But equally would such communications be totally devoid of interest, would be quite unintelligible and meaningless, if they did not all refer to a common objective world, and have their setting in a common framework of objective relations whose logical coherence and pattern are the work of conceptual thought. In the case of phantasy, there is more ground for holding that it possesses less reality as a field of socio-psychological activity than does the world of perception and overt performance. But here a distinction must be made. If we refer to such flights of subjective phantasy as are found in idle reverie or 'castle building' or in the compensatory day-dream, we deal with a field of activity admittedly lacking in objectivity, whose objects have little general social significance. But if it is the phantasy of the creative artist we have in mind, the situation is quite different. Few would deny that the creations of high-grade artistic imagination possess reality in their way as great as observed facts of nature and social behavior, or even scientific theories; would deny, in short, that the field of aesthetic appreciation and communication possesses social objectivity. This is not primarily because art-products embody sensuous properties and patterns (though this is essential), or that they conform to the conceptually formulated order of object and events (though this is necessary, at least to some slight degree), but principally because of the effect they have in arousing and releasing, steadying and tranquillizing, emotion. By virtue of these properties, objects which arise as flights of individual phantasy have the power of suggesting features of vital and enduring significance in the life-history of man and the nature of the world. Hence their production and enjoyment constitutes an objective field of social culture in whose activities and satisfactions all individuals can to some extent participate.

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[27] J. F. Brown, *op. cit.*, 51, 58.
[28] *Op. cit.*, 285.
[29] J. Piaget, *The Moral Judgment of the Child*, 1932, 324-325, 366.

The social psychologist may therefore agree that some of the values which men jointly realize in producing the works of social culture are objective and universal, without assigning these values to a transcendental or sublimated sphere. The values of knowledge, of practical power or technical mastery, and of beauty are objective because based on properties and relationships of the environing world. They are realized by three activities of the human organism, the bio-social activities of articulate speech, practical contrivance and emotional-aesthetic expression, which answer to these properties and relationships of external objects and events. The values in question are universal because realized to some extent by all who employ the three activities in the daily routine of social adjustment. Their realization, at the start and in the main, is incidental to the fulfilment of organic and economic need. Only by exceptional individuals, whether in primitive groups or in civilized societies are they pursued as goals for their own sake, but these few make a disproportionately large contribution to the sum-total of man's cultural achievements.

'SUBJECTIVE' COLORS FROM LINE-PATTERNS

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The color-effects observed when one gazes steadily at a black and white pattern, *e.g.* a tiled floor or a piece of finely cross-ruled paper, have recently been mentioned in two separate reports. The first report, made by Skinner in 1932,[1] describes "a paradoxical color effect" from an arrangement of white disks on a black background (see Fig. 2 j); and the second, by Luckiesh and Moss in 1933,[2] reports "definite color-effects" from parallel diagonal lines (see Fig. 3). Skinner thought that his paradox was something new, but Luckiesh and Moss related the color-effects of their pattern to those which Benham produced with disks in 1894.[3]

The phenomenon reported by Skinner and by Luckiesh and Moss is an old one; at best it is a case of old wine in new bottles. Counting the present instance, it has been 'discovered' no less than five times. It was first observed and reported by Fechner in 1838.[4] He discovered it accidentally while working on a black and white disk. He had prepared a disk of 18 concentric rings which varied, by equal amounts of black and white, from the innermost that was all black to the outermost ring that was all white (see Fig. 1 a). Expecting to see a series of gray rings differing in shade when the disk was rotated, he was astonished to discover that colors appeared at rotations below the rate of fusion. He found that different colors were reported by different *O*s, that the colors ranged from red to greenish blue, that they were usually not highly saturated, and that their saturation and their position on the disk depended upon the speed and direction of rotation.

Fechner's observations were verified by a number of his contemporaries[5] (see

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* Accepted for publication June 1, 1935.

[1] B. F. Skinner, A paradoxical color effect, *J. Gen. Psychol.*, 7, 1932, 481-482.

[2] M. Luckiesh and F. K. Moss, A demonstrational test of vision, this JOURNAL, 45, 1933, 137 f.

[3] Anon., An "Artificial spectrum top," *Nature*, 51, 1894, 113 f.

[4] G. T. Fechner, Ueber eine Scheibe zur Erzeugung subjectiver Farben, *Pogg. Ann. d. Physik u. Chemie*, 121, 1838, 227-232.

[5] H. W. Dove, Ueber Scheiben zur Darstellung subjectiver Farben, *Pogg. Ann. d. Physik u. Chemie*, 151, 1848, 526-528; *Farbenlehre*, 1853, 281-283.

Ernst Brücke, Untersuchungen über subjectiver Farben, *Pogg, Ann. d. Physik u. Chemie*, 160, 1851, 418-447; Ueber den Nutzeffect intermittirender Netzhautreizungen, *Ber. d. kaiser. Akad. d. Wiss., math.-natur. Kl.*, 49, 1864, 128-153.

Ogden Rood, On a new theory of light, proposed by John Smith, *Amer. J. Science & Arts*, 30, 1860, 182-186; Subjective optical phenomena, *ibid.*, 31, 1860, 417.

David Brewster, On certain affection of the retina, *L., E., & D., Philos. Mag. & J. Science*, 4th ser., 21, 1861, 20-24.

H. Aubert, *Physiologie der Netzhaut*, 1865, 377-380.

H. Helmholtz, *Handbuch der physiologischen Optik*, 1, 1867, 380.

Sigmund Exner, Bemerkungen über intermittirende Netzhautreizung, *Arch. f. d. ges. Physiol.*, 3, 1870, 214-240, esp. 230-234.

A. Kunkel, Ueber die Abhängigkeit der Farbenempfindung von der Zeit, *Arch. f. d. ges. Physiol.* 9, 1874, 197-220.

Fig. 1 b–i) and various explanations of the phenomenon were offered. Fechner himself explained the colors, which he called "subjective," by retinal inertia. The effect of white light upon the retina did not, so he thought, die out with equal rapidity for all the rays. Helmholtz, on the other hand, was of the opinion that the 'subjective colors,' which were dubbed "Fechner's colors" by Brücke in honor of their discoverer,[6] were due to differences in the latent periods of the various hues, *i.e.* to the differences in time at which the different colors reached their maximum intensity.

While the explanation of these colors was being discussed in Germany, John Smith in England, in 1859, 'discovered' for a second time that colors could be produced "by alternate light and shade in various proportions."[7] He used a disk (see Fig. 1 d) upon which five concentric rings of various proportions of black and white were painted, and obtained a "great variety of colors . . . amongst others, a pure red, and various shades of purple, pink, yellow and blue." The same results, he found, could be produced "by cutting out spaces in the white card and causing it to revolve on a black surface."[8] He "was satisfied that the experiments were original and not explicable by the present recognized laws." He thought that he had demonstrated "that varieties of color are produced by pulsations of light and intervals of shadow in definite proportions for each shade of color."

A report of Smith's experiments published in America[9] led Rood to repeat the observations.[10] Though corroborating Smith's results, Rood disagreed with his explanation. He preferred to take his stand with Fechner, who had, 20 years earlier, as he pointed out, come to the conclusion that the colors were subjective.

Interest in the phenomenon died out and it was apparently forgotten. In any case, the third 'discovery' was made by F. J. Smith in England in 1881.[11] He discovered accidentally, while revolving the large wheel of a bicycle between himself and the source of light, that various colors were produced by the interruptions of the spokes. "Different rates of revolution give different colors," he reports. When, for example "120 spokes pass before the observer per second, violet light shines out vividly; when 65 pass, red appears." He thought that the interruptions of the spokes decomposed the light and that there was a relationship between spoke-interruptions and wave-length. Soon after the publication of this report, Hannay,[12] using black-and-white disks which were rotated 'too rapidly for the eyes to notice the pattern and too slowly to combine into a gray,' explained the color effects that he obtained in terms of the latent periods of the different color-organs. The eye responded, as he believed, quickest to red, a little more slowly to green, and slowest

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[6] Cf. E. C. Sanford, A laboratory course in physiological psychology (Exp. 137, Some phenomena of rotating disks: Fechner's colors), this JOURNAL, 5, 1893, 401; also F. W. Bagley, An investigation of Fechner's colors, this JOURNAL, 13, 1902, 488 f.

[7] John Smith, On the production of colour and the theory of light, *Rep. Brit. Ass. Adv. Science,* 29, 1859, 22-23.

[8] A method recently utilized by G. A. Fry, see footnote 34 below.

[9] John Smith, On the cause of color and the theory of light, *Amer. J. Science & Arts,* 29, 1860, 276-278.

[10] Ogden Rood, On a new theory of light, proposed by John Smith, *Amer. J. Science & Arts,* 30, 1860, 182-186; Subjective optical phenomena, *ibid.,* 31, 1860, 417.

[11] F. J. Smith, Apparent decomposition of sunlight by intermittent reflecting surfaces, *Nature,* 24, 1881, 140.

[12] J. B. Hannay, Colour perception, *ibid.,* 25, 1882, 604-605.

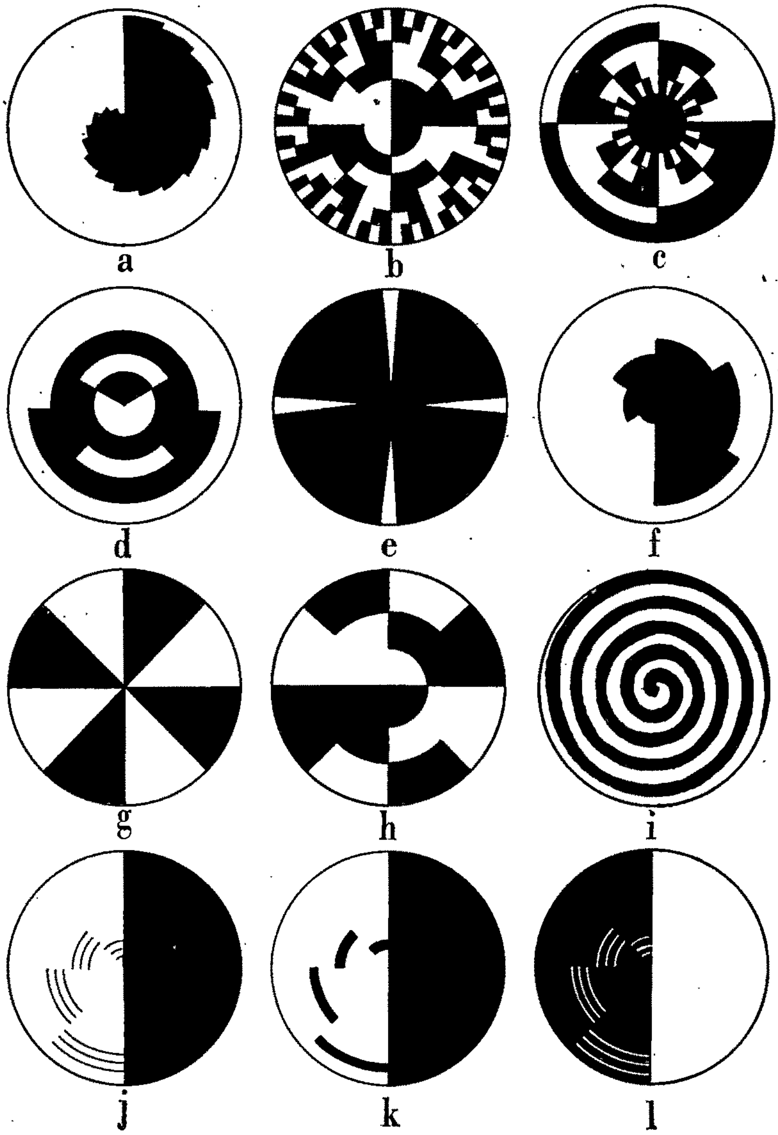

FIG. 1. VARIOUS DISKS USED IN THE PRODUCTION OF 'SUBJECTIVE' COLORS

a, Fechner; b and c, Brücke; d, John Smith; e, Rood; f, Aubert; g, h, and i, Helmholtz; j, Benham; k and l, Finnegan and Moore.

to blue. He thought, as did John Smith before him, that the experiment was of great theoretical importance as its explanation would "allow of a decision being arrived at between the theories of Young and Hering."

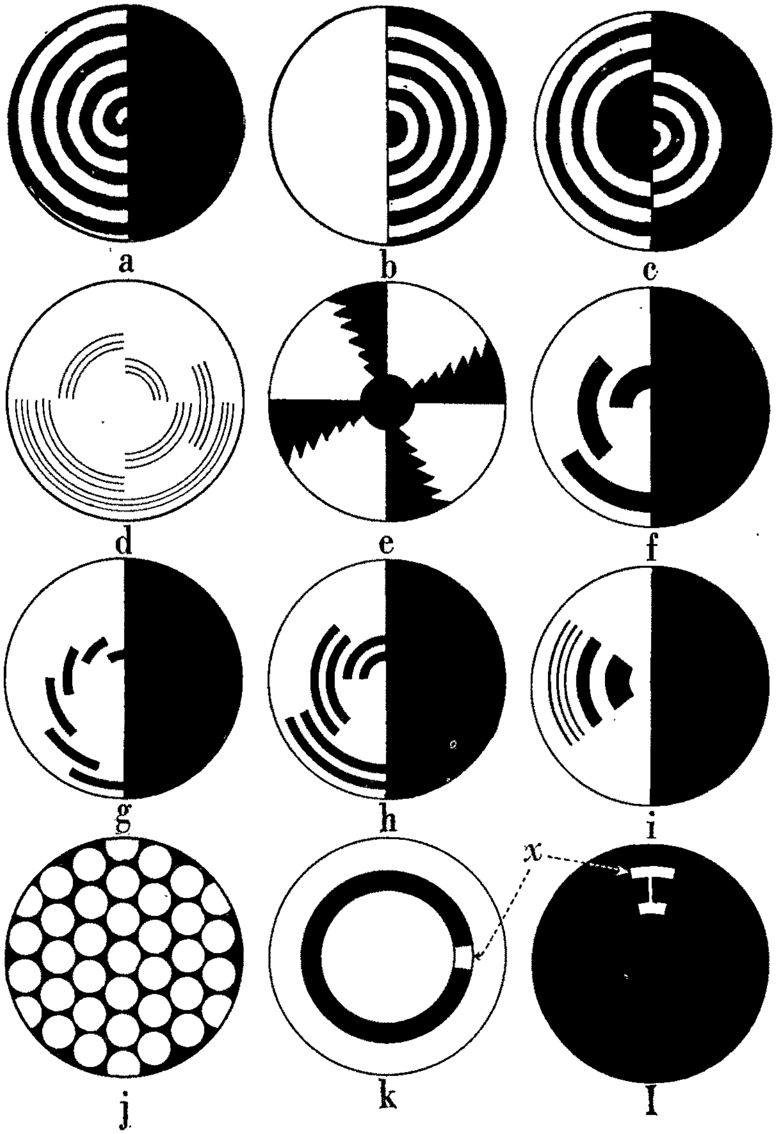

Fig. 2. Various Disks Used in the Production of 'Subjective' Colors
a, b, and c, Hurst; d, Bagley; e, f, g, and h, Baumann; i, Piéron; j, Skinner (stationary); k and l, Fry (*x* represents cut-outs).

Napier Smith[13] objected to Hannay's explanation, arguing that a given speed of rotation should, according to Hannay, yield but one color, while as a matter of fact one may observe "a dozen different colors side by side." Though venturing no explanation himself, Napier Smith pointed out that explanations which state that the effects are "physiological" and that the colors are "subjective" are not explanations in the true sense of the word.

Sanford included an experiment entitled "Fechner's colors," with citations from Helmholtz and Aubert, in the "Laboratory course in physiological psychology" which he published in 1893.[14] Despite that reference, the phenomenon was 'discovered' for a fourth time in 1894, when Benham constructed his "artificial spectrum top."[15] The top, a toy rotated between thumb and finger, carried a black and white disk of novel construction. "One half is black, while the other half has twelve arcs of concentric circles drawn upon it." The arcs, arranged in 4 groups of 3 each, were of 45° (see Fig. 1 j). Possibly because these disks differed from the old and also because the color-effects were greatly enhanced, the old phenomena were not recognized and the investigations which followed were reported without reference to the earlier work as if they concerned something entirely new. Numerous investigators studied the phenomena under various conditions. For example, Liveing varied the speed of rotation[16] and substituted sodium light for daylight.[17] Benham, too, used sodium light[18] and varied the speed of rotation.[19] Abney used monochromatic and mixed lights.[20] Finnegan and Moore varied the structure of the disk—making the arcs 1 cm. broad instead of lines, and placing white linear arcs on the black half of the disk instead of black arcs on the white (see Fig. 1 k and l)—and the speed and direction of rotation.[21] Turner devised a lantern slide of Benham's disk which enabled him not only to demonstrate the phenomenon to large audiences but also to study it by means of transmitted light of varying color and composition.[22] Hurst developed three new disks—"in which one half the disc was black and the other white, with a white spiral on the black, or a black spiral on the white, or with both at different distances from the centre on the same top"[23] (see Fig. 2 a, b, and c)—which intensified the phenomenon and which could be used without infringing upon the copyright on Benham's disk.[24] Bidwell varied intensity of illumination and speed and direction of rotation.[25]

Regarding the observations there was little dispute. The hue, tint, and chroma of the 'subjective' colors were found to depend upon speed and direction of rotation and upon intensity, hue, and complexity of illumination. With a change in direction of rotation, the order of the colors on the disk is reversed. With very rapid and very slow rotation no color results. With extremes of illumination the saturation of the colors is reduced.

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[13] Napier Smith, Colour perception, *ibid.*, 26, 1883, 30-31.
[14] *Op. cit.*, this JOURNAL, 5, 1893, 401 (Experiment 137).
[15] Anon., *op. cit.*, *Nature*, 51, 1894, 113 f.
[16] G. D. Liveing, On Benham's artificial spectrum, *ibid.*, 51, 1894, 167.
[17] *Ibid.*, 200.
[18] Benham, The artificial spectrum top, *Nature*, 51, 1894, 200.
[19] *Ibid.*, 321.
[20] W. de W. Abney, The artificial spectrum top, *Nature*, 51, 1894, 292.
[21] J. M. Finnegan and B. Moore, The artificial spectrum top, *ibid.*, 292.
[22] Dawson Turner, The spectrum top, *ibid.*, 439.
[23] C. H. Hurst, A true spectrum top and a complementary one, *ibid.*, 510.
[24] Newton and Co., The artificial spectrum top, *ibid.*, 463.
[25] Shelford Bidwell, On subjective colour phenomena attending sudden changes of illumination, *Proc. Roy. Soc. London*, 60, 1896, 368-377; On the negative after-images following brief retinal excitation, *ibid.*, 61, 1897, 268-271.

Regarding the explanation of the observations there was, on the other hand, small agreement. Even after Edridge-Green,[26] and Cattell[27] in 1895, called attention to the fact that Helmholtz, Rood, and Fechner had years before experimented upon and explained the phenomenon, the disagreement continued. The majority opinion, as shown in Bowditch's review in 1896[28] and Rivers' in 1900,[29] leaned, however, toward the theory of Helmholtz that the colors were due to differences in latent period, *i.e.* to the fact that red rises more quickly than green and that green rises more quickly than blue—facts attested to by Kunkel.[30]

In 1902, Bagley,[31] after a review of the literature, undertook the most comprehensive and thorough study of the phenomenon since Brücke. She studied the effects of rate of rotation, width and length of line, relative size of sectors, contrast, intensity of illumination, and background (see Fig. 2 d). Since that paper, the phenomenon has been extensively studied by three investigators: by Baumann,[32] from 1912-1918, who noted the effects of illumination, size of disk, order and size of sectors, and *O*'s distance from the disk (see Fig. 2 e-h); by Piéron,[33] in 1922, who used several different disks (see Fig. 2 i) and monochromatic illumination; and by Fry,[34] in 1933-1934, who, using transmitted light and disks with open sectors (as John Smith, Rood, and Hurst did before him), observed the effect of the following variables: brightness of the open sectors; frequency and duration of exposure (*i.e.* rate of rotation and size of sector); and brightness and frequency of stimulation of the surrounding retinal areas (see Fig. 2 k and l).

In the recent reports[35]—the fifth 'discovery'—the phenomena were obtained from stationary patterns. Heretofore, rotating disk-patterns had always been used. Indeed, according to Bagley, movement is "one of the essential conditions upon which the production of the colors depends."[36] Movement may, however, be involved even in the stationary patterns, since the colors may result, as Luckiesh and Moss suggest, from "rapid eye-movements" over them and thus the "effect may be analogous to the color-effects produced by a rotating Benham disk."[37]

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[26] F. W. Edridge-Green, The artificial spectrum top, *Nature,* 51, 1895, 321.

[27] J. McK. Cattell, The spectrum top, *Science,* 2, 1895, 13.

[28] H. P. Bowditch, The sense of vision, in W. H. Howell's *An American Textbook of Physiology,* 1896, 789 f.

[29] W. H. R. Rivers, Vision, in E. A. Schäfer's *Textbook of Physiology,* 1900, 2074 f.

[30] A. Kunkel, Ueber die Abhängigkeit der Farbenempfindung von der Zeit, *Arch. f. d. ges. Physiol.,* 9, 1874, 197-220.

[31] Bagley, *op. cit.,* this JOURNAL, 13, 1902, 488-525.

[32] C. Baumann, Beiträge zur Physiologie des Sehens: IV. Subjektive Farbenerscheinungen, *Arch. f. d. ges Physiol.,* 146, 1912, 543-552; V., *ibid.,* 166, 1916, 212-216; VII., *ibid.,* 171, 1918, 496-499.

[33] Henri Piéron, Le mécanisme d'apparition des couleurs subjectives de Fechner-Benham, *Année psychol.,* 23, 1922, 1-49.

[34] G. A. Fry, Modulation of the optic nerve-current as a basis for color-vision, this JOURNAL, 45, 1933, 488-492; Color phenomena from adjacent retinal areas for different temporal patterns of intermittent white light, *ibid.,* 714-721; Color sensations produced by intermittent white light and the three-component theory of color-vision, *ibid.,* 46, 1934, 464-469.

[35] Skinner, *op. cit.;* Luckiesh and Moss, *op. cit.*

[36] Bagley, *op. cit.,* 489.

Problem. Our problem in this study springs from the suggestion that the color-effects are due to eye movement. We wished specifically to determine what part, if any, eye movement played in the perception of these 'subjective' colors. Before turning to that problem, however, we undertook to learn the effect of (I) knowledge and instruction, (II) color of pattern and background, and (III) width of line and spacing. We wanted to discover what conditions yielded the most pronounced effects so that we could use them in the main experiments (IV) on eye movement.

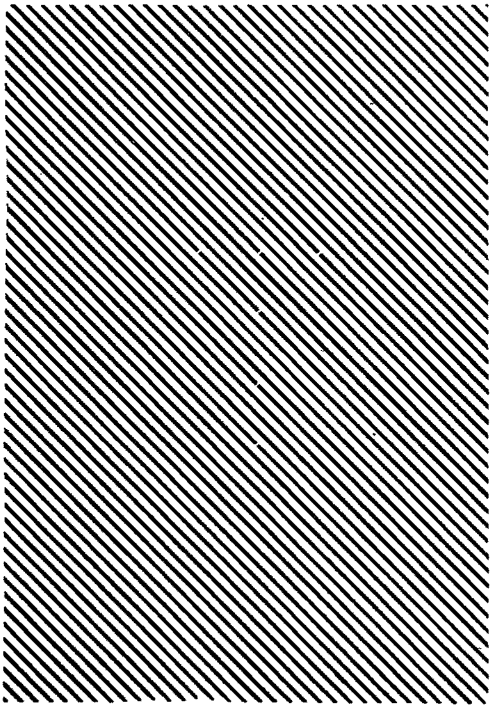

FIG. 3. THE LUCKIESH-MOSS STIMULUS-PATTERN
(Actual size used; centered on 6 x 9 in. sheet.)

In all parts of the study, we used the Luckiesh-Moss pattern (Fig. 3) with occasional modifications which will be described in the proper context.

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[37] Luckiesh and Moss, *op. cit.,* 137 f.

I. Effect of Instruction and Knowledge

The first part of our study concerned the effect of instruction. Owing to to the recent publication of the stimulus-figure[38] and to our students' familiarity with the phenomenon, we were unable to obtain a group of competent *O*s who were unacquainted with the color-effects. We therefore added knowledge of the phenomenon to our list of experimental variables.

Procedure. The stimulus-figure was a pattern of diagonal lines about 0.5 mm. wide and about 1 mm. apart, covering a rectangle approximately 9 x 6.5 cm. (see Fig. 3) and printed in dead black on egg-shell white paper. This pattern was presented to *O*, for 1 min. by means of a simple exposure apparatus, in a neutral gray booth illuminated by daylight mazda bulbs. *O*'s head was held firmly in position by means of a head rest which was so adjusted that *O*'s eyes were opposite, and 14 in. from, the center of the stimulus-pattern when exposed.

Observers. Of the 22 *O*'s who served in this part of the study, 9 were acquainted with the phenomenon under study and 13 were uninformed. All were competent observers.

Instructions. Three instructions were used. The first was general, simply asking the *O*s to observe the figure and to describe what they perceived.

> *(a)* "Look at the stimulus-pattern, which will be exposed directly in front of you, and describe what you observe."

The second instruction was also general regarding *O*'s task, but specific as to his visual fixation.

> *(b)* "Look at the stimulus-pattern with far-fixation, *i.e.* fixate beyond the plane of the pattern, and describe what you observe."

The third instruction specifically told the *O*s what to look for and what special phenomenon they were to describe.

> *(c)* "Concentrate upon the pastels which appear in and among the diagonals and describe the qualitative changes (hue, tint, and chroma) that occur during the course of the observation."

Every *O* served three times under each instruction.

Results. (1) 'Subjective' colors. Table I shows the distribution of the *O*s, observing with and without knowledge, who reported colors in at least one of the trials under the three instructions.

Knowledge of the phenomenon favors, but by no means guarantees, observation and report. Two of our 9 *O*s serving with knowledge did not report, and as developed in answer to questions after the experiment, did not observe the colors under instruction *a* or *b*. Neither, on the other hand, does a lack of knowledge guarantee that the colors will not be seen. Approximately a third of the *O*s serving without knowledge, 4 out of 13, reported colors under *a;* and under *b* the number was increased to about half, 6 out of 13. With the specific instruction *c,* all *O*s in both groups reported the presence of colors and described them.

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[38] Our study was begun during the spring of 1933, a few months after the publication of Luckiesh and Moss's observations.

TABLE I

NUMBER OF *O*S REPORTING COLOR UNDER DIFFERENT INSTRUCTIONS

Instruction	Knowledge	Colors reported	Colors not reported
	with	7	2
a	without	4	9
	total	11	11
	with	7	2
b	without	6	7
	total	13	9
	with	9	0
c	without	13	0
	total	22	0

Under all three of the instructions, and in particular under instruction *c,* all the primary and intermediate hues were reported by some of the *O*s, although generally not as appearing at one and the same time. The hues were usually of low saturation and light tint. Sometimes, however, saturated colors were reported and also spectral distributions. Descriptive terms for the latter were "small rainbows" and "the sort of thing you see when oil is on water." As a rule, however, only one or two colors were seen at a time or by a given *O*. Three *O*s reported blue alone and one reported yellow alone. The most frequent report was of yellow and blue. Indeed, in approximately 75% of the experiments, the first color reported was yellow or blue, or yellow and blue together.

In general the colors were observed in diamond-shaped patterns in the plane of the paper at right angles to the diagonals. Sometimes they appeared as nodules in the white spaces between the black lines, which then were wavy. A few instances were reported in which the colors appeared as dots over the pattern.

(2) Phenomenal movement. Phenomenal movement was reported under instruction *a* by 18 *O*s. In the main, two types of movement were described: (1) movement of particles over the figure as a whole and (2) movement of the lines in the pattern. Movement over the figure as a whole is characterized as follows: "rapid streaming of particles at right angles to the diagonal lines of the figure;" "shimmering or flowing;" "flickering like heat waves over the figure;" "specks as if it were raining;" "movement like the swirls on water when a pebble is dropped in a quiet pool." Movement of the lines in the pattern is characterized as "wavy movements of the black lines" or "the black lines seem to wiggle."

Under instructions *b* and *c*, these movements were reported less frequently. In a few instances, however, a third kind of movement was reported. The pastels themselves moved, *e.g.* "streaks of blue moving around" or "running or blowing of the colors."

The effect of instruction is again evident. Under *a*, a general instruction to inspect the figure, the *O*s reported everything that they perceived, and movement was one of the more striking phenomena. Under *b* they were primarily concerned with the task of keeping their fixation beyond the plane of the stimulus-figure and their reports were fragmentary, being chiefly comments upon the difficulties of far fixation. Under *c*, the instruction to observe the pastels, the *O*s concentrated upon the colors and were little concerned with details of the stimulus-figure or other attendant phenomena.

In view of the results obtained in Part I, we used in the succeeding parts of the study *O*s with knowledge and only the instruction to observe and to report specifically upon the colors.

II. EFFECT OF THE COLOR OF PATTERN AND OF BACKGROUND

The second part of our study concerned the effect of the color of the stimulus-pattern and of the background. The pattern was printed in red, yellow, green, blue, black, and white ink upon red, yellow, green, blue, black, neutral gray, and white backgrounds.[39] It was also printed in black on various kinds of white paper, viz. tissue, bond, egg-shell, English finish, and coated. The figures were centered on 6 x 9 in. backgrounds.

Procedure. These various combinations were exposed as in the experiments of Part I, to 6 of our most highly trained and competent *O*s.[40]

Instructions. The following instructions were given to *O* at the beginning of every experimental period.

> "Look at the stimulus-pattern and describe in detail the colors observed and the appearance of movement of any kind."

Results. (1) Color. The 'subjective' colors were most pronounced with the following combinations of pattern and background: (1) black diagonal lines on white background; (2) blue on green or white; (3) black on green or yellow; and (4) green on yellow. In no case were pronounced color-effects produced with yellow or white diagonals. Many of the combinations yielded no 'subjective' colors and some only different shades and chromas of the background color. The best color combinations of figure and background produced less pronounced effects than black on white.

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[39] The colored backgrounds were the primary shades of the Milton Bradley papers.

[40] Miss L. Bestor, graduate student in psychology; Mr. J. D. Coakley and Mr. W. S. Neff, assistants in psychology; Dr. A. D. Glanville, research fellow in psychology; and the authors.

The range or variety of colors reported was about the same as in Part I. Their shapes, however, were more varied. Four different patterns were reported: (1) a reticular pattern, *i.e.* the diamond-shaped pattern reported in Part I; (2) irregular areas or blotches of pastel colors ill-defined in outline or form; (3) bands, streaks, zig-zag and waving forms; and (4) figures in which the interstices between the diagonal lines are striated, nodulated, or beaded with color.

(2) Movement. Movement of some kind seemed to be inherent in the perception. No less than six types were reported by the *O*s.

(a) Streaming. The most frequent type, reported by all the *O*s, was the streaming movement, a "movement of small particles in direction downward to the left at right angles to the diagonals." The movement was like "the drift of a very fine snow," and its rate varied from "a slow drift" to "a rapid streaming." This type of movement did not involve the 'subjective' colors but was a movement of fine particles, white or gray in quality, through which the stimulus-pattern and the 'subjective' colors were seen.

(b) Random movement. A second class of movement was described by the *O*s as a "movement of a shadow or cloud," "fluctuation of lighter and darker areas," "moving blobs, indefinite both as to shape and direction," "clouds of color floating over the figure." This type involved colors as well as grays and was random over the figure without definite direction.

(c) Moving bands and zig-zag lines. In addition to these movements of irregular areas, movements of bands and zig-zag lines were described. For example: "jagged designs that fluctuated in position," "fluctuating zig-zag lines," "waving bands of color," "iridescent bands which moved over the stimulus-figure, never constant nor well-defined in direction or in form," "bright and dark bands, which oscillate and follow the direction of the 'streaming' movement"—described in (*a*) above.

(d) Movement of stimulus-lines. The lines of the stimulus-pattern often themselves exhibited movement. They were reported as "moving violently," "wriggling slightly," "approaching one another and then swinging apart again rapidly, giving the appearance of a body of water during a heavy storm," and as "breaking up into dashes and moving against the 'streaming' movement," as "waving," and as "curling."

(e) Circular movement. A fifth kind of movement was circular. "Dark gray and blue splotches move around the center of the pattern," "a circular movement in the field," "a rapid movement of large gray areas, approximately circular." This type of movement is like that described in (*c*) in that the moving pattern is ill defined, but it differs in the fact that the direction of movement is well defined.

(f) Shimmer, seething. In addition to these five types, still another is discernible. It is the most indefinite and vaguest of all. It is movement without something definitely moving and is described as a "wave-like shimmering of the whole pattern," a "violent seething in all directions," as "movement similar to that observed when a pattern is observed through heat waves."

The moving patches, zig-zag lines, and bands were frequently reported as colored. The color frequently, though not always, conformed to the hue of the background of the stimulus-pattern. Blue and gray were notable exceptions. Moreover, the zig-zag lines in particular failed to conform to the quality of the background, though they were perhaps to some extent at least influenced by it.

The stimulus-patterns most favorable to the perception of movement, as judged by the rapidity, amount, and number of kinds of movement reported, were (1) black figure on white background, (2) red on white, (3) blue on white, (4) black on yellow, and (5) white on blue. The combinations least productive of movement were, in general, those with white or yellow figures. As a rule, the paler and less pronounced the figure, the less likely is the pattern to produce phenomenal movement. With one exception (white figure on blue background), the best combinations were dark lines and light backgrounds; in other words, those most nearly approaching the optimal combination of black figure on white background.

The movement phenomena reported by our *O*s are similar to, but not identical with, Ferree's "streaming phenomenon."[41] The chief difference is that our movements never blotted out the stimulus-pattern, but were superimposed upon it.

(3) Color and movement. No direct relation obtains between the insistence and variety of the 'subjective' colors and the amount and diversity of the perceived movement. Some of the combinations, *e.g.* yellow figure on gray background, were equally poor for color and for movement; some, *e.g.* blue figure on green background, were equally good for both; and some *e.g.* white figure on blue background, were poor for the pastels and good for movement. No combination yielded good pastels and poor movement. On the whole, all the color combinations of figure and background were highly productive of phenomenal movement and poor in the production of the pastels. None gave pastels of the qualitative range or brilliancy given by the black figure on the white background.

(4) Paper texture. The texture of the paper in the black-white series (black figure on tissue, bond, egg-shell, English finish, and coated stock) had, under our conditions, no discernible effect upon the *O*s' reports. Experiments with these stimulus-combinations were scattered haphazardly among the others. They all appeared, in contrast to the color-combinations,

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[41] C. E. Ferree, The intermittence of minimal visual sensations, this JOURNAL, 19, 1908, 114-129; The streaming phenomenon, *ibid.*, 484-503.

to be equally good in yielding the phenomenal effects, viz., colors and movements.[42]

III. Effect of Line-Width and of Spacing

Having found that the black-white combinations were the most favorable for the arousal of the 'subjective' colors, we next turned to discover the optimal width of the pattern-lines and of the intervening spaces.

Procedure. A series of stimulus-patterns, similar to the Luckiesh-Moss in size and form was prepared by varying width of lines and spaces systematically. The figures, drawn in India ink, were centered upon 6 x 9 in. white cards. Five line-widths (0.5, 1, 2, 4, and 6 mm.) and five space-widths (1, 2.5, 5, 7, and 9 mm.) were used. As every line-width was combined with every space-width, there were in all 25 stimulus-patterns in the series. With the exception of the stimulus-patterns, procedure, *O*s, and instructions were the same as in Part II.

Results. (1) Color. The results were unequivocal. Narrow lines in combination with narrow spaces yielded numerous and brilliant pastels; narrow lines and wide spaces, or broad lines and narrow spaces, yielded few and very unsaturated pastels; and broad lines with wide spaces yielded none. Under our conditions, the wider the lines and the larger the interstices, the fewer and the less saturated were the 'subjective' colors. For example, when the optimal line-widths (0.5 and 1 mm.) were combined with the optimal space-widths (1 and 2.5 mm.), the pastels covered a wide range of quality and brilliancy. When, however, the optimal line-widths were used with the larger space-widths or the optimal space-widths with the larger line-widths, either no colors were perceived or the colors were very desaturated and evanescent, fading quickly. When line-widths of 4 and 6 mm. were combined with space-widths of 7 and 9 mm. no colors were seen.

The color qualities reported in this part of the study were similar to those reported in Part I c. Of the stimulus-patterns yielding color, yellow and blue were reported together in 43%, yellow alone in 34%, and blue alone in 8%. In addition to yellow and blue, red and green were reported together in 7.5% of the patterns and red alone in 7.5%.

(2) Movement. Phenomenal movements were observed only with the narrow lines and spaces. The conditions effective in this series of experiments for the perception of pastels were also effective for the perception of movement. Some indication was obtained, however, from the non-optimal patterns that the phenomenal movements depended upon eye movement. One *O* (*C*) reported, when wide lines and narrow spaces were used, that "the entire card seemed to vibrate . . . and in

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[42] It is quite possible, had the black-white combinations been presented in a series by themselves, that some textures would have yielded more pronounced effects than others, but that was a variation in procedure that we did not pause to consider.

unison with eye movements." Again, when wide lines and wide spaces were used, "movements occurred only when I moved my eyes."[43]

IV. EFFECT OF EYE MOVEMENT

Having determined the optimal instructions and the optimal quality and form of the stimulus-pattern, we turned to discover the effect of eye movement.

Procedure. The Luckiesh-Moss pattern printed in dead black on egg-shell white paper—the pattern used in Part I—was presented to the *O*s in a modified Dodge tachistoscope.[44] The exposure-times, controlled by means of a condenser-charge apparatus,[45] were: 45, 115, 382, 455, 522, 752, and 873 ms. and 1, 2, 3, 4, and 5 sec. The fixation-point was placed, in all but a few experiments with one *O*, in the plane of the stimulus-figure. With the exceptions described, the procedure was the same as in Part II.

Results. Since eye movements may be made during exposure-times longer than 100 ms., the possibility of our *O*s moving their eyes was ruled out in only one of the intervals—45 ms. Not only were no 'subjective' colors reported by our *O*s in that interval, but they were not reported in any interval shorter than 3 sec. In intervals of 3 sec. and longer, they were reported by all our *O*s. That there is a dependence of the 'subjective' colors upon eye movement cannot, in face of these results, be questioned; but the fact that exposure-times capable of yielding eye movement did not yield color indicates, as we believe, that eye movement is not the only condition necessary for the production of color.[46]

The few experiments with near and far fixation yielded 'subjective' colors during shorter exposure-times than in the other experiments with fixation in the plane of the stimulus-figure. The *O*s and the experiments were too few in number, however, to warrant generalization. It is, nevertheless, worthy of note that this result corroborates those obtained in Part I b, in which fixation was beyond the plane of the stimulus-pattern. The phenomenon seems to be enhanced by fixation outside of the plane of the stimulus-figure. This may be due to movements—of convergence, accommodation, or both—that are involuntarily made to bring fixation into the plane of the figure. The relative disadvantage of fixation in the plane of the figure may after all, then, be sheerly a matter of eye movement.

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[43] Another interesting observation was made by *D*, at first with wide lines and spaces and then, after the phenomenon had caught his attention, with the narrower lines and spaces but never with the narrowest lines and spaces. The observation was that "the stimulus-patterns possess tridimensionality, the diagonal black lines look as ribbons projecting at right angles to the page."

[44] J. G. Jenkins, A simple tachistoscope of many uses, this JOURNAL, 45, 1933, 150. Cf. also Raymond Dodge, An improved exposure apparatus, *Psychol. Bull.*, 4, 1907, 10-13.

[45] W. D. Moeder and J. G. Jenkins, A condenser-charge timer, this JOURNAL, 45, 1933, 742-744.

[46] We do not know whether eye movements were actually made during the briefer exposures; all we know is that they might have been made, and that it is highly improbable that they were not made at least during the intervals as long as 1-2 sec. This part of our study should be repeated with a recording of eye movement during the exposures. The recently devised method of recording eye movement by means of the electrical potentials of the muscles of the eyes offers an excellent means of accomplishing this end.

SUMMARY

(1) A pattern of narrow black lines close together on a white background gives rise, when regarded steadily for short periods of time, to the perception of pastel colors—with or without prior knowledge and specific instructions to look for them.

(2) The colors are usually of low saturation and of light tint. The hues, while predominantly yellow and blue, cover the entire spectral range.

(3) The colors are enhanced when fixation is not in the plane of the figure.

(4) The colors appear in four positions upon the stimulus-card: (a) perpendicular to the diagonal lines of the figure in diamond-shaped patterns; (b) in bands or zig-zag lines, also perpendicular to the lines of the pattern; (c) in ill defined masses or clouds; and (d) in the spaces between the black lines.

(5) While some stimulus-patterns of colored figure and background give rise to the perception of the 'subjective' colors, no combination was found which approximates the black on white combination in effectiveness.

(6) Widening the diagonal black lines and increasing the intervening white spaces decreases the effect.

(7) Six kinds of phenomenal movement are reported: (a) streaming, (b) random, (c) fluctuating zig-zag lines and bands, (d) movement of the stimulus-lines, (e) centrifugal or circular, and (f) shimmering or seething.

(8) The 'subjective' colors are to some extent dependent upon eye movement, but eye movement, though necessary, does not appear to be a sufficient or compulsory condition.

THE SPECTRAL LOCATION OF PSYCHOLOGICALLY UNIQUE YELLOW, GREEN, AND BLUE

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Color vision has had as much attention, both experimental and theoretical, in scientific literature as any other single subject. One might expect to find an adequate answer to almost every question in the field he might raise; but the assumptions upon which the various experiments have been based are widely varied, and the theorization that has been offered has been conceived with specialized aims. As a consequence important problems have been obscured or have been passed by on either side because they happened not to lie within the theoretical scope of investigations which might be expected to include them. The particular problem with which we are concerned is such a one; namely, the specification, in experimental terms, of the primary or unique colors.

Seven points of reference combined in an equation have been proposed as necessary and sufficient for the psychological description of all possible colors.[1] Red, Green, Yellow, Blue, and White have been generally accepted as necessary to the qualitative description of colors, but questions have been raised about black and gray. Black, however, is the *psychological* as well as the logical complement to white, and experimentation has shown it to be a positive color quality, necessary as a point of qualitative reference.[2] Helmholtz's comment is interesting in this connection: "Black is a real sensation, even if it is produced by entire absence of light. The sensation of black is distinctly different from the lack of all sensation."[3] Southall reaffirmed this position: "The physicist is right in thinking of a black object as sending no stimulus to the retina, but he is wrong when he assumes that there is no black sensation correlated with this absence of stimulation. It is absurd to say that black is the absence of visual stimulation. Black is a positive sensation just like any other visual sensation, and just as real and distinct as the sensation of white or of yellow and blue or of red and green."[4]

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* Accepted for publication November 15, 1938.

[1] F. L. Dimmick, A reinterpretation of the color pyramid, *Psychol. Rev.*, 36, 1929, 83-90.

[2] E. B. Titchener, A note on the sensory character of black, *J. Philos., Psychol., & Sci. Methods*, 13, 1916, 113-121; A further word on black, *ibid.*, 649-655; F. L. Dimmick and C. H. Holt, Gray and the color pyramid, this JOURNAL, 41, 1929, 284-290.

[3] H. Helmholtz, *Physiological Optics*, 2, 1924, 131.

[4] *Ibid.*, 60 (footnote 1).

The question of gray, raised more recently,[5] points to a similar treatment of that color, although it has certain aspects that set it off as distinct from the other six colors: to wit, it has no complement, it combines with any color, and as Titchener[6] and Stumpf[7] have said, it may be taken as the zero-point of the "pro-intensive" series.

The standards for black and white have been fairly well established: for white a magnesium oxide surface, viewed at an angle of 90°, with heterogeneous light striking it at an angle of 45°, and observed through a reduction screen of very low reflectance;[8] for black the color of lightless space, obtained by looking through an aperture into a box, the interior of which is non-reflecting.

The standard middle gray has been specified experimentally in terms of the proportion of two reflecting surfaces.[9] The equivalent luminance of this standard is 5±.5 foot lamberts or 0.011 ±.001 candles per sq. in. measured by the Luckiesh-Taylor Brightness meter.[10]

When we seek comparable standards for the remaining four primary colors, however, we do not meet with similar success.[11] We find a large amount of data, little of it substantiating any part of the remainder—data obtained by different methods, on different assumptions, and for different purposes. There is relatively little agreement even as to a procedure which is adequate to establish an experimental standard for primary colors.

Hering was well aware of the necessity for distinguishing the psychological primaries from the physiological processes of the retina and from the Maxwellian mixture primaries and has characterized the three principal concepts pertaining to primary colors, concepts which too often we find confused and interchanged.[12] While few authors fall neatly into any one of his categories, it is well to recognize the separate concepts which are involved in the various discussions of primary colors. Hering himself does not maintain the consistent separation of his concepts that many citations of his work would lead one to expect. He had a definite aim for his theorizations and the original distinction was made for the purpose of leading up to his systematic conclusions. That accomplished, there was no value, for him, in insisting upon the cleavage between the concepts. Consequently we find him defining *Urfarben* as an obviously psychological term,[13] but later using it in a physiological sense, for those colors which do not shift in hue with a change from foveal to peripheral vision.[14] The two are descriptive of different phenomena, the one of a psychological judgment, the other of a retinal condition; but agreement between the two was valuable for Hering's theory. He did not, however, hesitate to point out the disagreement between similar facts where they did not support his theory; for example, the disparity between the mixture complementaries and the psychological primaries. The same lack of a consistent separation of concepts is found throughout the literature and seriously impairs the reliability of various standards given for the primary colors.

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[5] F. L. Dimmick, A note on the series of blacks, grays, and whites, this JOURNAL, 31, 1920, 301-302; The series of blacks, grays, and whites, *Psychol. Rev.*, 32, 1925, 334-336.

[6] Titchener, Visual intensity, this JOURNAL, 34, 1923, 310.

[7] C. Stumpf, *Die Attribute der Gesichtsempfindungen,* 1917, 20, 27, 32, 72.

[8] D. B. Judd, Preparation and colorimetric properties of a magnesium-oxide reflectance standard, *Nat. Bur. Stand.*, LC-395, 1933, 1-4. The reference here is not to white light but to a "seen" white light, or to white as a color quality.

[9] F. L. Dimmick and George McMichael, The psychological determination of the limits of pure gray, this JOURNAL, 45, 1933, 313-314.

[10] M. Luckiesh and F. K. Moss, *The Science of Seeing,* 1937, 323; M. Luckiesh and A. H. Taylor, A brightness meter, *J. Opt. Soc. Amer.*, 27, 1937, 132.

[11] Judd, Hues of the spectrum colors, *Nat. Bur. Stand.*, LC-454, 1935, 1-6.

[12] E. Hering, *Grundzüge der Lehre vom Lichtsinn,* 1, 1907, 2-3; Prüfung der sogenannten Farbendreiecke, *Arch. f. d. ges. Physiol.*, 47, 1890, 425.

In Table I,[15] we have summarized the values given in the literature, but direct comparison of those of one author with those of another is not valid, as will be obvious from the following discussion.

(1) Helmholtz gives the spectral location of the primary colors and some of the non-primaries, but this specification is not exact and there is no description of the experimental procedure involved. For example, he says: "Red is the color at the less refrangible end of the spectrum, which shows no marked difference from the extreme end of the spectrum to about line E. . . . From the line C to the line D the red passes through orange . . . into golden yellow. . . . There is a rapid transition of color from the line D to the line b. First there is a narrow strip of pure yellow which lies about three times as far from E as from D. Then comes green-yellow, and finally, pure green between E and b. Between the lines E and F the green becomes blue-green and blue, and from F to G there are different shades of blue. . . . Violet is the region in the spectrum from the line G to the line H or L."[16] These descriptions can, of course, be translated into mμ, but it is evident from his treatment of them that Helmholtz does not intend them to be more than a matter of obvious, careful inspection without experimental controls. Later writers have often referred to Helmholtz as if he gave exact specifications of the principal colors, but they cannot have correctly interpreted Helmholtz's intention.

(2) Von Bezold,[17] for example, follows Helmholtz's specifications exactly, and offers no further substantiation of those values.

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[13] Hering, *Grundzüge* (*op. cit.*), 40-43; Zur Erklärung der Farbenblindheit, *Lotos,* 1, 1880, 81 ff.

[14] Hering, *op. cit., Lotos,* 82; Prüfung der sogenannten Farbendreiecke, *op. cit.,* 423.

[15] Judd, Hues of the spectrum colors, *op. cit.*

[16] Helmholtz, *op. cit.,* 64 f.

[17] Wilhelm von Bezold, *Theory of Color* (Trans.), 1876, 20-22.

(3) Donders,[18] in 1884, made a careful determination of unique yellow. He required of his *O*s a short period of so-called "neutral" adaptation in a room illuminated only by light from a cloudy sky falling through a 'white' ground glass window. The experiment was performed on 111 eyes, but Donders does not state how many reports were obtained from every *O,* nor does he follow a precise method in obtaining the judgments, but, instead, what he calls a "slow backward and forward motion," decreasing in range, in the region of the 'pure yellow.' With this procedure there is a greater opportunity for successive contrast to be effective, as he notes. Donders had a definite idea of the difficulty of a color judgment of this type, for he does not require his observers to indicate a 'pure' yellow, but to judge the limits of yellow, *i.e.* that point where it becomes noticeably reddish or greenish, and he calculates the unique yellow as the mean between the two. Donders' experiment seems more adequate than most, in spite of certain uncontrolled

TABLE I

SUMMARY OF THE VALUES (IN $m\mu$) GIVEN BY THE VARIOUS AUTHORS FOR RED, YELLOW, GREEN, AND BLUE

Author	Date	Red	Yellow	Green	Blue
(1) Helmholtz	1866	686.9	589	526–516	486
(2) Bezold	1874	686.9	589	526–516	486
(3) Donders	1884		582		
(4) Hess	1888	PR*	574.5	495	471
(5) Hering	1890	PR*	577	505	470
(6) König and Dieterici	1893	PR*		505	470
(7) Voeste	1898		560	490–500	460
(8) Ladd-Franklin	1902	PR*	576	505	470
(9) Exner	1902	494(C)		508	475
(10) Von Kries and Schottelius	1908	606.6	574.4	503	
(11) Westphal	1910	445+655†	575.5	516	478.8
(12) Dreher	1911		575	509	477
(13) Ridgway	1912	644	577	520	473
(14) Goldytsch	1917	692‡			467
(15) Allen	1919	680		570–470	420–400
(16) Goldmann	1925		568	505	468
(17) Priest	1926	680	583	515	475
(18) Brückner	1927		578	498	470
(19) Schubert	1928		574	500	467
(20) Maerz and Paul	1930	644	585	521	452
(21) Purdy	1931		576	504	476
(22) Hecht	1931	650–700		505–500	470
(23) Weld	1932	624 599	571 496		454
(24) Ornstein, Eymers, and Vermeulen	1934	630	581–576	555–502	487
(25) Verbeek and Bazen	1935	667–646	580	536	480–477.5

* PR indicates that the author states that all spectral reds are yellowish or that red lies toward purple from spectral red. (C) gives the complementary wave length to non-spectral "pure" red.

‡ A mixture of these two wave lengths give a true red.

† 692 has 11.5% yellow.

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[18] F. C. Donders, Farbengleichungen, *Arch. f. Anat. u. Physiol.,* Physiol. Abt., 1884, 518 ff.

factors, because it is dependent upon a single experimental concept, that of a yellow that is neither reddish nor greenish. Primary yellow is thus defined as that yellow which, under the conditions outlined above, corresponds to 582 mμ on the average with extremes of the range at 472 mμ and 594 mμ.

(4) Hess,[19] worked with Hering and, following Hering's theory of color vision, produced results in substantiation of it. The primary colors which he sought to establish through experiment are the so-called 'unveränderlich Farbentöne,' that is, those colors which change only in saturation and not in hue toward the periphery of the visual field. These primaries, which are physiological, were found by Hess to correspond to a red lying farther toward the purple than that of the extreme red end of the spectrum, a yellow of 574.5 mμ, a green of 495 mμ, and blue of 471 mμ. The experimental conditions under which they were obtained are not made explicit. The four primaries form two pairs of complementary hues, which agrees with Hering's contention. Hess implies that the same hues are also the psychological primaries, but there is no attempt to verify the statement.

(5) Hering fails to describe any experiments in which he systematically determined the spectral location of the *Urfarben*,[20] but in a footnote he cites 470 mμ for blue, 577 mμ for yellow, and a color slightly less yellow than 505 mμ for green, the red lying, as with Hess, farther toward the purple than the red end of the spectrum.

(6) König and Dieterici use the data of wave-length mixture to substantiate hypotheses regarding the physiology of the retina and the psychology of color quality.[21] They determined the mixture curves for their spectral primaries with 4 *O*s, 2 normal and 2 anomalous trichromats. From the points of intersection of these curves they deduced the Grundempfindungen. "Wir wollen nunmehr unter 'Grundempfindung' eine solche Empfindung verstehen, der ein einfacher (d.h. durch keine Art des Reizes weiter zerlegbarer) Prozess in der peripherie des Nervus opticus entspricht."[22] It is difficult to state whether the Grundempfindungen were also intended to be "simple sensations" *i.e.* psychologically unique colors. Something of the sort seems to be implied, but the relation of these concepts to one another and to the concept of *Urfarben* is not clear. The conditions of observation are not described but it is stated that each comparison was repeated at least ten times in the bright region of the spectrum, and at least twenty times in the dark or blue region.[23]

(7) Voeste,[24] in 1898, introduced a still different concept of invariable hues. He reported on the qualitative changes of spectral colors, between 660 mμ and 430 mμ attendant upon retinal fatigue. By matching light of a known wave-length projected upon a fatigued portion of the retina with light of the same wave-length projected upon an unfatigued area, he found three invariable points which showed

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[19] C. Hess, Über den Farbensinn bei indirektem Sehen, *Graefe's Arch. f. Ophth.*, 35, 1889, 1-62.

[20] Hering, Prüfung der sogenannten Farbendreiecke, *Arch. f. d. ges. Physiol.*, 47, 1890, 425 ff.; Zur Erklärung der Farbenblindheit, *Lotos*, 1, 1880, 81 ff.

[21] Arthur König, and Conrad Dieterici, Die Grundempfindungen in normalen und anomalen Farbensystemen und ihre Intensitätsverteilung im Spektrum, *Zscb. f. Psychol.*, 4, 1893, 241-347.

[22] *Ibid.*, 324.

[23] *Ibid.*, 296.

[24] H. Voeste, Messende Versuche über die Qualitätsänderungen der Spectralfarben in Folge von Ermüdung der Netzhaut, *Zscb. f. Psychol.*, 18, 1898, 257-267.

no shift in hue as a consequence of fatigue. These points correspond to 560 mμ, from 490 mμ to 500 mμ, and 460 mμ. Voeste used five intensities, and the time of observation of the fatiguing light was 10 sec. in all cases except with the lowest intensity where the time of fixation was 15 sec. Here again there is not sufficient information regarding the conditions of observation to permit the comparison of Voeste's data with those derived from other procedures.

(8) Ladd-Franklin has cited wave-length specifications for four primary colors,[25] but there is no indication that these values were obtained experimentally.

(9) Exner[26] published work supplementing that of König and Dieterici. His procedure is evidently identical with theirs, and his results deviate little from theirs. The experiment was carried out on 4 *Os* with normal color vision, and each measurement was repeated 10 times during a period of several days. No more is said of the actual conditions of the experiment, but Exner's work is considered an extension of that of his predecessors.

(10) Von Kries and Schottelius published some experiments in 1908 which, in part, bear upon this subject.[27] In the section of interest to us, the experimenters sought to establish values for what they call 'reine Prinzipalfarbe.' *O* was required to make judgments on the basis of the 'immediate sensation-impression' compared to a "pure color" standard previously shown. There seems to have been too much knowledge on the part of *O* of the nature of the experiment, too little control of the conditions of observation, and too few cases.

(11) Westphal says, in stating his problem:[28] "In dieser Untersuchung über die Urfarben sollen die Wellenlängen, die verschiedenen Personen zur Erzeugung von reinem Rot, Gelb, Grün, und Blau dienen, bestimmt werden. Es wurde davon ausgegangen, als reines Rot sei solches zu bezeichnen, das weder nach Gelb noch nach Blau neige, als reines Gelb ein solches, das weder nach Rot noch nach Grün neige, usw."[29] He is, then, setting his problem as psychological, and he follows this up with the instructions to *O* to give the judgment 'Urfarbe' at the point where the first trace of the new color component becomes visible. His account of the conditions of the experiment is more adequate than most. All observations were obtained between 9 A.M. and 1 P.M.; there were 10 *Os*, 3 of whom were color-blind; each value given seems to be the average of 4 'pairs' of presentations, one ascending and one descending series constituting a pair; the method used throughout was the method of limits, and there was no knowledge on the part of *O* except that essential to the method.

(12) Dreher investigated the invariable hues,[30] both with a change of intensity and with a shift from foveal to peripheral vision, but the two concepts were kept

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[25] Christine Ladd-Franklin, Vision, *Baldwin's Dict. Philos. & Psychol.,* 1, 1902, 767 f.

[26] Franz Exner, Über die Grundempfindungen im Young-Helmholtz'schen Farbensystem, *Sitz. d. Wein. Acad., math.-nat. Kl. 3,* 2a, 1902, 857-877.

[27] Lotte von Kries and Elisabeth Schottelius, Beitrag zur Lehre vom Farbengedächtnis, *Zsch. f. Sinnesphysiol.,* 42, 1908, 192-209.

[28] Hans Westphal, Unmittelbare Bestimmung der Urfarben, *ibid.,* 44, 1910, 182-230.

[29] *Op. cit.,* 182.

[30] Edgard Dreher, Methodische Untersuchung der Farbentonänderungen homogener Lichter bei zunehmend indirektem Sehen und veränderter Intensität, *Zsch. f. Sinnesphysiol.,* 46, 1912, 1-82.

distinct and investigated separately. The *O*s, 4 in number, served in from 4 to 20 trials on a single match. Light adaptation was maintained at all times and the time of observation was 1 sec. Dreher was careful to keep the exit slit of his apparatus small enough to insure a nearly monochromatic stimulus, and to obviate the influence of after-images and fatigue of the observer. He states that he followed the method of limits, but it appears that he used unequal steps, and that he often acted as both *E* and *O* at the same time. In this experiment he located the invariable hues. The values given in Table I are for the "subjektiv reine Farben," for which the procedure is not given. Dreher does not, however, equate the different types of primaries, as some others have done.

(13) Ridgway cites wave-length values for what he calls "the six psychologically distinct colors of the solar spectrum," and his statement has at times been referred to as defining primaries.[31] But there is no indication of how these values were obtained. Evidently it is a casual statement without experimental evidence.

(14) Goldytsch published an article in 1917 which is interesting from the point of view of method.[32] Using a Hering window he attempted to establish the yellow valence of spectral red in terms of the percent of 'pure' blue which must be added to the red in order to produce a contrast effect of 'pure' green. This procedure involves several assumptions which are of doubtful validity. The first is that the presence of yellow is more easily and accurately detected in green than in red. Our results, as well as those of most investigators, indicate that judgments of color are more difficult in the region of the green. The second assumption is that a contrast effect is more accurately judged than the original color. The last is that a unique color will necessarily produce a unique contrast effect, which is, in fact, assuming a one to one correspondence between the physiological and the psychological processes involved. He used 3 *O*s, and he fails to state how many trials were required for each value, or what psychophysical procedure was followed. The 'pure' blue which he added to the red he chose by a casual inspection of the spectrum. The judgment required of *O*, that of a 'pure' green, is too complex and difficult a judgment to require of an *O*.

(15) Allen compared curves of the persistency of vision throughout the spectrum, of an eye adapted to diffused daylight, with similar curves of an eye fatigued by light of specified wave-lengths.[33] The curves of the fatigued eye showed 5 types of elevations or deviations from the normal curve, depending upon the wave length of the fatiguing stimulus, and four points of transition from one type of elevation to another where the normal and fatigue curves coincided. Three of the elevations he takes to be indicative of three fundamental color sensations because they involved in each case only a single region of the spectrum corresponding to red, green, and violet respectively. Allen's method is in some respects analogous to that of König and Dieterici, and of Exner, but he neglects to state the number of *O*s or the number of trials.

(16) Goldmann sought to establish the 3 Urfarben according to the following

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[31] Robert Ridgway, *Color Standards and Color Nomenclature,* 1912, 7.

[32] L. Goldytsch, Messende Untersuchungen über die Gelbvalenzen spektraler roter Lichter mit Hilfe einer neuen Methode, *Zsch. f. Biol.,* 67, 1917, 35-56.

[33] Frank Allen, On the discovery of four transition points in the spectrum and the primary color sensations, *Philos. Mag.,* 38, 1919, 55-81.

criteria:[34] (1) uniqueness of color for sensation; (2) no change of hue with a change of intensity; (3) no change of hue with a change to peripheral vision; (4) no change of hue with a change of saturation; and (5) the complementarism of hues in compensation and in successive and simultaneous contrast. Before the experiment there was a 10-min. period in the dark, and the duration of observation was 2 sec. There is no indication of the number of *O*s, but there seem to have been 'at least three' observations for each specification.

(17) The data of Priest are unpublished,[35] and were supplied by D. B. Judd of the Bureau of Standards.

(18) Brückner made determinations of the Urfarben as a preliminary to another experiment.[36] The criterion was that the color should appear 'neutral,' *i.e.* unique, for both direct and indirect vision. From 8 to 13 determinations were made for each of the 4 primaries, but no further data are given.

(19) Schubert investigated the Urfarben under various conditions of illumination.[37] His definition of Urfarben is much the same as that of Goldmann. He gives no data concerning the procedure in the experiment, but he found that the Urfarben, determined under specific conditions of illumination, were complementary under those conditions, and were not complementary under somewhat different conditions.

(20) Maerz and Paul cite wave-length specifications for several colors,[38] none of them necessarily unique colors.

(21) Purdy made an investigation both of the invariable hues and of what he calls 'critical' or unique colors.[39] Using various intensities measured in photon units, he found 4 invariable points, one of them extra-spectral, in the purple-red. The spectral invariables are: 571 mμ, 506 mμ, and 474 mμ. He then made a determination of the Urfarben as defined by Westphal. These values have been given in Table I. We have here the advantage of having both types of data determined under the same conditions and by the same observer. Purdy finds a close measure of agreement between the two, the discrepancy being largest in the case of yellow. The physical conditions under which the investigation was carried out were very carefully controlled, but Purdy fails to describe his psychological procedure.

(22) Hecht follows the König-Dieterici assumption in regard to color specification, with no more specific information concerning procedure or conditions.[40] After a very careful consideration of a great variety of experimental data, he states that it is virtually impossible to utilize the curves of "color" mixture to represent anything other than "color" (stimulus) mixture.[41] He admits, however, that König

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[34] H. Goldmann, Über Dauerwirkung farbiger Lichter auf das Auge, *Arch. f. d. ges. Physiol.,* 210, 1925, 70-115.

[35] Judd, Hues of spectrum colors, *op. cit.*

[36] A. Brückner, Zur Frage der Eichung von Farbensystemen, *Zsch. f. Sinnesphysiol.,* 58, 1927, 322-361.

[37] G. Schubert, Sind urfarbige Lichterpaare komplementär? *Arch. f. d. ges. Physiol,* 220, 1928, 82-100.

[38] A. Maerz and M. R. Paul, *A Dictionary of Color,* 1930, 5.

[39] D. M. Purdy, Spectral hue as a function of intensity, this JOURNAL, 43, 1931, 541-559.

[40] S. Hecht, The interrelation of the various aspects of color vision, *J. Opt. Soc. Amer.,* 21, 1931, 615-639.

[41] Hecht, The development of Thomas Young's theory of color vision, *ibid.,* 20, 1930, 237 ff.

thought of the mixture primaries as corresponding both to the fundamental color sensations and to the physiological primaries, and states that there is a tacit agreement within the field that this corresponds to the actual facts. Such a statement brings one of the most pressing of our problems into the open at last and indicates the need for experimental data to exchange for present assumptions.

(23) Weld attempted to determine statistically the wave-length boundaries between the "six spectral colors."[42] His 38 *O*s, most of them casual visitors to the laboratory, were asked to set the cross-hairs of a spectroscope ocular on what they considered the boundaries between red and orange, orange and yellow, etc. In regard to his general methodology Weld says: "The writer can think of no better basis of doing this [*i.e.* of standardizing the wave-length boundaries between the six spectral colors] than to take the average judgment of a sufficient number of people." Lewin has made the point that a statistical treatment of data can add nothing to the validity of the data themselves,[43] and Weld's data do not appear to be highly valid. As Luckiesh and Moeller pointed out in an investigation of the naming of common signal colors, "color names are by no means standard for a group of persons, even though the colors are common ones of fair saturation, and the names assigned are often different when the color is viewed singly than when it is viewed among a group of other colors."[44] In addition several of Weld's *O*s were color-blind.

(24) Ornstein, Eymers, and Vermeulen obtained results which, while they do no pretend to be exact, are interesting.[45] They simulated rail-road conditions, using a flash of color to the dark-adapted eye, and found very little agreement in the naming of colors, but this is of course due in part to the fact that, their problem being a practical one, they used relatively uncontrolled conditions.

(25) Verbeek and Bazen investigated the distribution of the 'chief' colors of the spectrum.[46] One hundred *O*s were shown a series of lights of 21 wave-lengths twice, *i.e.* in both spectral orders. They were asked to look at a stimulus, after dark adaptation, and to tell its color, in the following terms: "Red, orange, yellow, green, blue, white, gray and doubt." While they found certain points of agreement, the results were in general exceedingly scattered.

Thus we find in the literature that there is relatively little agreement upon the procedure appropriate to such an investigation. Several distinct concepts are employed without due cognizance of the fact that they are divergent. The question of the principal or unique colors has not been attacked as a single experimental problem and with a controlled psychological methodology. In view of the incompleteness of these earlier attempts, we undertook to make an experimental determination of the spectral location of the psychologically unique colors.

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[42] L. D. Weld, Some observations on spectral color discrimination, *Proc. Iowa Acad. Sci.*, 39, 1932, 209 f.

[43] Kurt Lewin, The conflict between Aristotelian and Galileian modes of thought in contemporary psychology, *J. Gen. Psychol.*, 5, 1931, 141-175.

[44] M. Luckiesh and J. W. Moeller, Naming the common signal colors, *J. Opt. Soc. Amer.*, 13, 1926, 465 ff.

[45] L. S. Ornstein, M. J. G. Eymers, and D. Vermeulen, Farbenerkennungsprüfungen mit Rücksicht auf die Brauchbarkeit von Signalgläsern, *Proc. kon. Akad. Amst.*, 37, 1934, 385 ff.

[46] H. P. J. Verbeek and M. L. Bazen, Distribution of the chief colors of the spectrum, *Physica*, 2, 1935, 380 ff.

EXPERIMENTS

Apparatus. The instrument used was a double-prism spectroscope fitted with an auxiliary eye-piece to afford *O* a Maxwellian view,—the exit slit necessary to insure an apparently monochromatic stimulus was too narrow for a proper judgment of hue. This slit (0.13 mm. wide) passed a band 6 Å wide at the red end of the spectrum, whereas, according to Jones,[47] nowhere in the spectrum are differences of less than 10 Å distinguishable. The drum of the spectroscope was calibrated in millimicrons and accurate settings of 0.5 mμ could be made. The source of light was a 300-w., 115-v. Mazda projection bulb, rated at 24 lumens per watt and a color temperature near 3000°K. The entrance slit was set at 0.01 mm. in width.

Table II shows the intensity-values of the stimuli in the regions of the spectrum under examination.

TABLE II

INTENSITY-VALUE OF THE STIMULI

Wave-length	Band width (mμ)	Intensity (Ft. lam.)	Pupil diam.* (mm.)	Photons
582	.6	9	3	872
515	.45	5	4	798
475	.25	1.6	5	432

* The diameter of the pupil was estimated by inspection but our values approximate those obtained by Reeves' photographic method. (Cf. P. Reeves, Rate of pupillary dilation and contraction, *Psychol. Rev.*, 25, 1918, 330–340.)

A head rest kept *O*'s head in a constant position relative to the ocular. *O* was screened from direct light, as well as from a view of any of the apparatus except the ocular. The experiment was carried out in a dark room, illuminated by an Ivanhoe North-Light Lamp of 500 w., so that the general illumination was constant and dark-adaptation was avoided. Results were obtained from ten observers, three men and seven women, using foveal vision of the right eye. All the observers had normal color vision. The *O*s were: D. D. Scovil, P. Wolz, elementary students in psychology; E. F. Faust, K. Fitch, F. Brent, E. Brown, N. H. Ludlow, W. McClaren, advanced students in psychology; Dr. C. C. Dimmick and Dr. F. L. Dimmick.

Procedure. The procedure we followed answered the two requirements: (1) that *O*'s judgment should be simple and unequivocal, and (2) that it should not be affected by any color standard outside this experiment, such as a "good green," etc. Therefore, *O* was never required to judge the presence of the color under investigation, but was asked to report the point of first appearance of the bordering color, separate determinations being made for each border of a single color. For example, in determining the location of unique yellow, the point of just noticeable redness in yellow was first established, and later the point of just noticeable greenness. In a like manner both borders of green, and of blue were investigated. The actual location of the unique color could thus be assumed to lie between the two experimental values. The location of red could not be determined with this apparatus because, as is generally recognized, all spectral reds are yellowish.

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[47] L. A. Jones, The fundamental scale of pure hue and the retinal sensibility to hue differences, *J. Opt. Soc. Amer.*, 1, 1917, 63-77, esp. 74.

TABLE III

Boundaries of the Yellow, Green, and Blue Regions for All *O*s

O		Red in yellow	Green in yellow	Yellow in green	Blue in green	Green in blue	Red in blue
	av.	584.92	580.87	542.71	511.04	485.00	467.72
F	h	.94	1.02	.58	.84	1.10	.81
	σ	.75	.69	1.22	.84	.64	.87
	av.	584.40	577.48	500.65	492.50	481.23	471.63
CD	h	.74	.99	.71	.71	.64	.90
	σ	.95	.71	.99	.99	1.10	.78
	av.	582.78	577.30	496.07	491.26	481.58	459.92
S	h	.96	1.25	1.05	.91	.77	.84
	σ	.74	.56	.67	.78	.92	.84
	av.	584.02	579.33	519.06	490.10	483.42	469.23
W	h	.78	.89	.27	.88	.76	.63
	σ	.91	.79	2.62	.80	.93	1.12
	av.	576.33	574.15	505.84	503.25	475.10	459.23
L	h	.80	1.002	.68	.34	.64	.47
	σ	.88	.70	1.04	2.08	1.10	1.50
	av.	582.25	579.63	539.11	506.10	486.88	464.61
M	h	1.16	1.49	.46	.75	.91	.84
	σ	.61	.47	1.53	.94	.78	.84
	av.	593.01	584.32	536.01	513.27	477.93	466.95
FD	h	.72	.89	.29	.68	.71	.92
	σ	.98	.79	2.44	1.04	.99	.77
	av.	586.50	583.71	530.32	511.73	487.70	469.99
KF	h	1.16	1.09	.36	.64	.61	1.11
	σ	.61	.65	1.96	1.10	1.16	.64
	av.	593.62	579.99	557.72	510.75	489.37	474.82
FB	h	.83	1.07	.38	.38	1.22	.54
	σ	.85	.65	1.86	1.86	.58	1.31
	av.	584.79	576.97	538.70	513.01	480.28	469.18
BB	h	1.12	1.01	.39	.42	.69	.78
	σ	.63	.70	1.81	1.68	1.02	.91
Av.		585.26	579.36	526.62	504.31	482.25	467.32
m.v.		3.58	2.32	16.69	5.06	3.73	3.72
Location of unique color		Yellow		Green		Blue	
by average		582.35		515.45		475.1	
by median		582.7		514.6		476.5	

The final values were obtained by the Method of Constant Stimuli, preceded by a considerable amount of practice with the Method of Limits in which a high degree of stability of judgment was set up in the *O*s and the regions of change were roughly determined.[48] At the beginning of every observation period *O* was instructed as follows: "After a ready signal, you will be shown a color. You are to judge whether that color is reddish or not-reddish." Several practice judgments which were not included in the calculations were taken at the start of every observation hour. Every determination is based upon 100 judgments on each one of seven equally spaced stimuli which had been found by preliminary work to lie in the region of doubt. The size of the stimulus-steps was 5 Å. Rest periods were given every 10 min., and observation periods were limited to 1 hr.

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[48] E. G. Boring, Urban's tables and the method of constant stimuli, this JOURNAL, 28, 1917, 280-293.

Results. The results are shown in Table III. The limits of each one of the three regions are given for every *O* with the two measures of reliability, h and σ. The average limits for all the *O*s are calculated, together with their mean variations. Finally, the mean of each region is taken as the most representative or 'best' yellow, green, and blue. These last values we obtained both by averaging the calculated limits of the regions and by taking the median J.N.D. step obtained from Jones's tables.[49] The two determinations do not differ significantly due to the fact that the stimulus-values of the J.N.D. remain fairly constant. Since the stimulus-difference for a J.N.D. by juxtaposed comparison is never less than 1 mμ. we may state the three standards in the nearest mμ. for convenience as yellow, 582; green, 515; blue, 476.

Although the limits of the regions show variations from *O* to *O*, the general conformity is striking. It bears out our contention that the human *O* may be used as an instrument, and will give reliable and accurate results. Care must be taken, however, in the standardization of the *O*s and the control of the conditions of observation. The greatest individual variations are due to conditional factors that could be eliminated on the basis of information gained in the course of the experiment. In every case but one, *O*'s average deviation about the limit of a region was no more than 2 J.N.D. of the juxtaposed comparison type, which have smaller values than judgments indicating the direction of difference. The greater variability in the yellow-in-green region involves at least two factors: (1) this region lies near the peak of the sensitivity curve, and the *O*s reported difficulty in distinguishing between the greater brilliance and the appearance of yellow; (2) since the J.N.D. in this region are 2 or 3 times as large as those in the other regions, the steps of 0.5 mμ. were too small, decreasing the relative stability of our *O*s. The magnitude of the errors did not, however, appear to us sufficient to warrant the repetition of the determinations at this time.

When we compare our results with those of earlier investigators (Table I) it is evident that we agree most closely with those who, as nearly as we can judge, were attacking the problem from the same angle. This is notably true with respect to Donders, Westphal, and Priest.

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[49] Jones, *op. cit.*, 74.

The averages which Judd obtained from the values cited by various authors,[50] lie close to the points found in our experimental determination. While this correspondence seems to indicate the presence of common factors, the validity of those averages depends upon the assumption that such common factors existed, and until we had experimental confirmation of that fact, the assumption was only a lucky guess. One would scarcely have been justified in basing further work on so uncertain a basis. Such, indeed, was the situation that gave rise to the present investigation.

Likewise, it cannot be assumed that the spectral locations of the several unique colors will coincide either with those of the mixture complementaries or of the invariable hues. It is well known that the complements of red and green are both perceptibly bluish.[51] Helmholtz's value for the complement of spectral red, which is decidedly yellowish, lies between our unique green and unique blue. Until we have determined the location of unique red in terms of spectral red plus unique blue we cannot carry the comparison further. Our blue and yellow do not coincide exactly with Helmholtz's complementaries. Both lie to the red of their complements so that their mixture should leave a residual red comparable to the residual yellow from the mixture of unique red and green. This fact may have importance for the physiological hypothesis of color vision since it may prove as valid to state that red is a result of the mixture of blue and yellow as to state that yellow results from the mixture of red and green.

SUMMARY

A careful survey of the literature on color reveals no adequate experimental determination of the spectral location of the psychologically unique red, yellow, green, and blue. In most cases there has been a failure to distinguish between mixture primaries, invariable hues and psychologically fundamental colors. In a series of experiments, carefully controlled both with respect to the physical stimulus-conditions, and the psychological conditions of observations, we obtained with 10 *O*s the spectral value for yellow 582 mμ, for green 515 mμ, and for blue 476 mμ.[52] All but one (red) of the seven reference standards which are necessary for the psychological description of all colors are now adequately specified.

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[50] Judd, Hues of the spectrum colors, *op. cit.*

[51] J. H. Parsons, *An Introduction to the Study of Colour Vision,* 2nd. ed., 1924, 38 ff. A. C. Hardy, *et al., Handbook of Colorimetry,* 1936, 30 f.

[52] Red must be obtained by mixing blue with spectral red. Its determination requires a different apparatus and will be reported in a subsequent paper.

THE INFLUENCE OF PAST EXPERIENCE UPON PERCEPTUAL PROPERTIES

By Karl Duncker, Swarthmore College

'Past experience' was for a long time an extremely popular explanatory concept in psychology. Hardly any of the major problems managed to keep from being slurred over and trivialized by reference to past experience. Even in perception there seemed to be no limit to its efficacy. Sensory organization, the constancies, apparent movement—they and many others—were at times attributed to the magic of learning. Then the reaction set in. Except for the field of learning proper, 'past experience' came to be almost synonymous with something whose possibility a decent experimenter should exclude at the very outset. That was a gain, for as long as 'past experience' held sway no other explanation seemed to have a chance. I think we are now free enough to turn around and ask: what, after all, is the effect of past experience upon present experience? More precisely: *how do traces influence the phenomenal appearance of perceived objects?*

Direct influence.[1] First of all, past experience may supply 'meanings' that come to be experienced as intrinsic properties (tertiary qualities) of the things in question, such as the 'sit-on-able-ness' of a chair (Tolman) or the 'man-ness' of the patch in Fig. 1. A sound distinction can be made, however, between *empirical meanings* proper and the underlying perceptual material (including configuration). How, under what conditions, and to what extent, is this realm of 'pure' perception affected by past experience? Again there is one ready answer: as long as the configurational factors inherent in the present stimulation are not too strong, past experience is definitely capable of altering certain *configurational* features of the percept in question. I found the conditions of Fig. 1 well suited to the demonstration of one particular way in which past experience may 'reconfigurate' perception; namely, indirectly, through changing the empirical meaning. Several of the configurational properties in the area on the left of one or the other of the two lines will undergo changes as the area on the right is exposed (for the first time) and the left area thereby is transformed into part of a man—from being nonsensical, or a dog's head, or a whale with a white eye. Some of these changes cannot be at-

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* Accepted for publication June 13, 1938.

[1] For the indirect influences see p. 265.

tributed directly to the mere addition of so much configurated area, but must be ascribed to the influence of past experience (here acting through a shift of empirical meaning provoked by the added area). Such changes affect (1) direction or orientation—the dog faces to the left while the same portion of the man is not so polarized; (2) emphasis, shift of focus —which, on purely configurational grounds, should much sooner have shifted to the right than up to the man's head; (3) grouping or segregation—the area of the man's arm is segregated from the area of the trunk, a segregation, or boundary, that had not existed before; (4) depth or stratification—the arm disappears behind the blouse; (5) figure and ground —both on the dog and in the whale the white spot is a figure, focal in the rôle of the whale's eye and peripheral on the dog's muzzle (cf. 2), while in the man it is part of the ground 'seen through.'

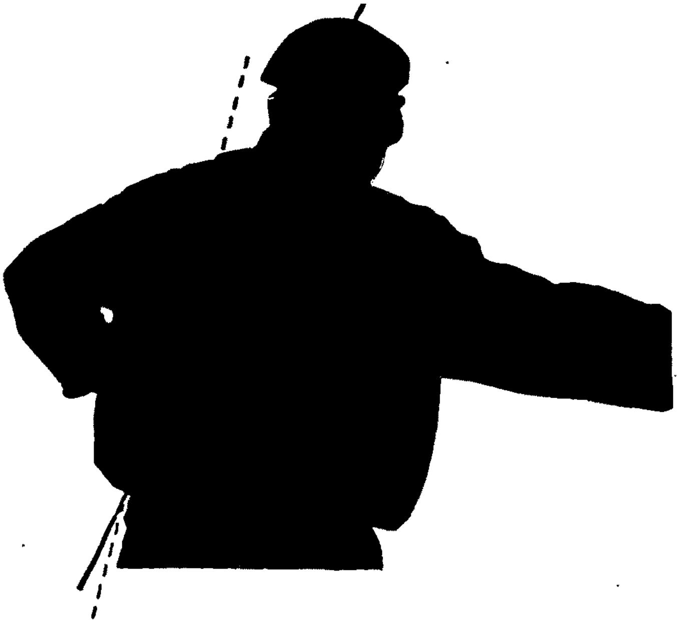

FIG. 1. FIGURE THAT WAS PRESENTED, FIRST PARTIALLY AND THEN AS A WHOLE

Of systematic experiments on other kinds of effect which past experience may have upon present configuration, I wish to name the following: Rubin,[2] working with nonsensical patterns in which either the black or the white area could be seen as 'the figure,' found that patterns which had been learned under the instruction

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[2] Edgar Rubin, *Visuell wahrgenommene Figuren*, 1921, 6-21 (Die figurale Nachwirkung).

'white figure,' and patterns which had been learned under the instruction 'black figure,' were, in a test series containing both types of patterns, spontaneously organized the way they had been learned. Gottschaldt,[3] following up a suggestion of Wertheimer,[4] found that as many as 500 repetitions of a simple figure would not help it to be recognized when given as an integral part of a strong configuration. On the other hand, in a series where two simple figures were exposed in a regular rhythm the one that was 'expected' at a given moment would win out over the other when both were exposed simultaneously in a joint pattern with liminal intensities. In a somewhat rash conclusion Gottschaldt postulated that past experience could modify configurations only through giving rise to overt expectations ('specific vectors'). Braly[5] found a perseverative dominance of a learned type of geometrical figures, which were made out of dots and were tachistoscopically exposed. Leeper[6] found, among other things, that previous experience with one of the two aspects of an ambiguous picture made that particular aspect more dominant in the joint picture. Djang,[7] applying Gottschaldt's part-recognition method to relatively weak configurations, found that under such conditions distinctly positive results could be obtained.

May it be emphasized that only in the cases of Rubin and Djang have we clear cut proofs of organization without a vector of expectation. That such is possible is also proved by Fig. 1, as well as by an observation of my own; whenever I have read music for a while all minor inhomogeneities of my visual field look like musical symbols although my 'mind' has already turned away from all concern with music.

After all this it can no longer be doubted that, under somewhat ambiguous conditions, past experience does have a voice in matters of organization. The next question to ask is: can past experience also influence the sensory *material* (the 'stuff'), can it induce *qualities* such as color, taste, or such qualities as 'spatialness,' 'movingness'?[8]

Among the causal factors involved in brightness and color constancy Hering mentions "memory color" to which psychological factor however he assigns but a minor rôle compared with that of the peripheral adaptive mechanisms.[9] Katz agrees with Hering as to the relative unimportance of memory color: "Thus I cannot assign any great importance to memory colour in connection either with our experiments or with colour constancy in general."[10] However to study memory color in its own right, to investigate its conditions and to assess its potency, lay outside their field of interest (for reasons mentioned above, p. 255). Moreover it was regarded to be "impossible, or at least very difficult, to arrange quantitative experiments with memory colours, on account of their peculiar lability."[11] True, memory color is unstable, its influence seems, more so than that of other factors, to require an 'objective attitude,' *i.e.* attending to the colored object instead of attending merely to the color phenomena themselves; it is true also that it took poor illumination for Matthaei to see in a photograph the chromatic colors of the objects represented[12]—but all this does not mean that memory color is any the less worth investigating. Being interested in the effect of past experience upon sensory material we saw no reason for not subjecting memory color to a special experimental treatment.

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[3] Kurt Gottschaldt, Über den Einfluss der Erfahrung auf die Wahrnehmung von Figuren, I, *Psychol. Forsch.*, 8, 1926, 261-318; II, *ibid.*, 12, 1929, 1-87.

[4] Max Wertheimer, Untersuchungen zur Lehre von der Gestalt, *ibid.*, 4, 1923, 301-350.

[5] K. W. Braly, The influence of past experience in visual perception, *J. Exper. Psychol.*, 16, 1933, 613-643.

[6] Robert Leeper, A study of a neglected portion of the field of learning—the development of sensory organization, *Ped. Sem.*, 46, 1935, 41-73.

[7] Siao-Sung Djang, The rôle of past experience in the visual apprehension of masked forms, *J. Exper. Psychol.*, 20, 1937, 29-59.

[8] As to the latter type of quality it has been claimed by the so-called 'empiricists' that past experience is quite fundamental in certain fields. How else, so it was argued, but through some associative communication with visual space and visual movement could, for example, a series of kinesthetic sensations ever have gained the qualities of 'spatialness' and 'movingness'? This hypothetical influence of past experience need, by the way, not be due to a point-to-point association, but could be due to a total reinterpretation of one system of differences in terms of the general (spatial) quality of the other (visual) system. Unfortunately, owing to the generality of this alleged effect of past experience, no one has as yet succeeded in devising an experiment which would prove that learning was involved.

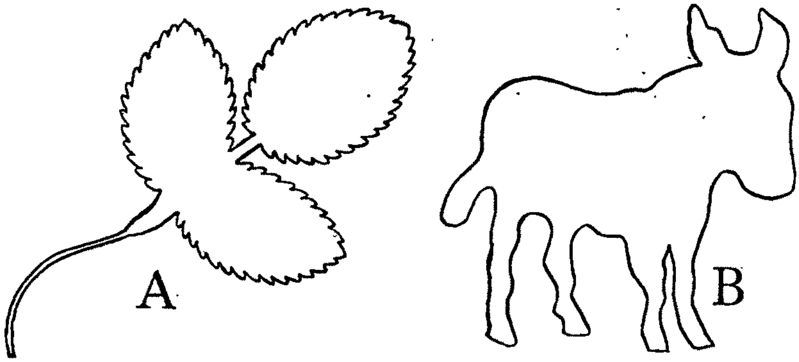

FIG. 2. FIGURES USED IN THE EXPERIMENTS ON MEMORY COLOR
(A is a milliner's green leaf; B is a donkey cut from the same green leaves and patched together.)

Experiments on memory color. To experiment on memory color it was necessary to work for just the reverse of what the classical experimenters had been so anxious to obtain. We had to devise a setting that would eliminate all other reasons for one object to show more color constancy than another, except memory color.

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[9] Ewald Hering, *Grundzüge der Lehre vom Lichtsinn,* 1920, 6-20.
[10] David Katz, *The World of Colour,* 1935, 265.
[11] *Ibid.,* 162.
[12] R. Matthaei, Die Welt der Farbe, 1927, 310 (Cited in Katz, *op. cit.,* 162).

Materials. To this end we got some artificial green leaves (such as are used for trimming hats), one leaf was retained as a 'leaf' (cf. Fig. 2A), as convincingly a green leaf as one might desire, and out of some of the others we cut and patched together the picture of a 'donkey' (cf. Fig. 2B), equal to the leaf in all respects but shape,[13] *i.e.* in color, texture,[14] and size.[15] These two green objects, pasted on white paper, were (alternatively, not simultaneously) exposed to such red illumination as would allow them to display but little of their greenness. (If replaced by a circular figure of the same make, the latter would look gray, with only a bare touch of greenness.) Now, if memory color is a factor at all, the leaf, known as green, should appear greener than the donkey, known as gray.

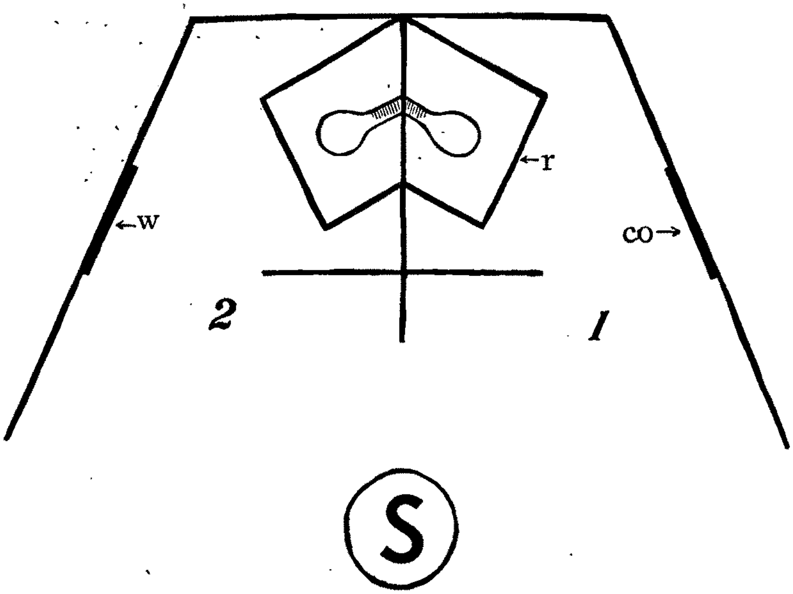

FIG. 3. DIAGRAM OF THE APPARATUS USED IN THE EXPERIMENTS ON MEMORY COLOR

(S = subject; co = critical object; w = color-wheel; r = red glass.)

Apparatus. Fig. 3 shows the set-up. Owing to a plate of red glass in front of its lamp, Chamber 1 was under a red illumination which was homogeneous in the area of the critical object and then, spreading out over the white background, gradually faded away into shadow. In Chamber 2 the light was normal and, corresponding to the place of the critical object in Chamber 1, there was a color-wheel which *S* had to match with the object by looking back and forth and telling the experimenter in which direction it should be changed (lighter, browner, greener). On the wheel 4 different sectors could be exposed: black, white, a light green, and a yellowish orange.

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[13] As for difference in shape see below p. 262.

[14] Figs. 2A and 2B give no more than the outlines of the objects used which in addition showed the veins of leaves.

[15] The difference in size between 2A and 2B was reversed in direction between the experiments of Table I and those of Table II.

The procedure was as follows. The 'donkey' was exposed in Chamber 1 while on the color-wheel in Chamber 2 some mixture in the phenomenal vicinity of the crucial object was offered, *e.g.* 300° black, 20° white, 20° orange, and 20° green. After *S* had expressed his wish the wheel was stopped—which, in order to prevent objective cues, *S* was not allowed to watch—and the sectors were changed in the desired direction. It required, on an average, 6 trials to arrive at a somewhat satisfactory equation. Then the donkey was replaced by the leaf. In about 50% of all cases the same mixture that had satisfied the donkey was offered with the leaf, in the other cases *S* was presented with an additional 20° white which made the color-wheel look too light. The whole procedure, however, was not disclosed to *S* who, on the contrary, was led to believe that the color-wheel was changed in the most irregular fashion. When, after about 6 trials, the leaf equation had been obtained, the leaf was again exchanged for the donkey, and *S*, presented with a mixture which he did not know to be identical with the one satisfying the leaf, was again asked to match. Between 3 and 5 such matching were carried out with every *S*, the minimum series being donkey, leaf, donkey, and the maximum being donkey, leaf, donkey, leaf, donkey. (The donkey was given first to prevent a carry-over from the leaf—for the leaf was more liable to color the donkey than vice versa, owing both to the coincidence of its memory color with the color disguised by the illumination and to its reality, non-picture, character. This factor of temporal order, however, cannot be called upon to explain the fact and the direction of the difference in greenness found between the leaf and the donkey.)

Results. In the first and major series of experiments—in which the material was of a light green color, like that of the young leaves of a beech-tree, and the donkey was a little smaller than the leaf—11 *S*s took part in the experiments. For 6 of them the leaf appeared decidedly and measurably greener than the donkey (cf. Table I). For 2 *S*s the leaf tended to look a little greener, but no equation could be set up. For 3 *S*s there was no difference at all. It is interesting that of these 'negativists,' one was a painter (a painter is used to abstracting from objective connotations), another was a psychologist who knew the problem and moreover was "distinctly uninterested in and poor on colors," and the third was a medical student, also more sophisticated than the majority of the *S*s. There was no case of reversal, *i.e.* of the donkey being greener than the leaf.

Table I presents in degrees the angles of the green (G), orange (O), and white (W) sectors which were found equivalent to each of the two critical objects.[16] Every figure represents the average of from 1 to 4 values, each being the outcome of about 6 trials. This table shows that these 6 *S*s were just about agreed as to the magnitude of the difference. The mixture which was equivalent to the leaf contained on an average 100% more green and 100% more orange than the one equivalent to the donkey. But what does this difference mean in phenomenal terms? The numerical differences certainly give impression of a much greater difference than was actually seen. Actually they correspond to a phenomenal difference which is *little more than just distinctly noticeable* (when the two equivalents are shown side by side). The phenomenal mean—as to greenness—between the leaf-equivalent and a pure gray of equal brightness was found to be somewhere in the close vicinity of 25°G, 13°O and 33°W, that is to say, the donkey's equivalent is just about half as green as the leaf's equivalent.

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[16] The remaining portions of the color-wheel were black.

TABLE I

MIXTURES ON THE COLOR WHEEL (IN DEGREES) THAT WERE EQUIVALENT TO THE COLOR OF THE DONKEY AND THE LEAF

(G=green; O=orange; W=white. Remaining portions of the wheel were black.)

S	Donkey				Leaf			
	G	O	W	No.	G	O	W	No.
1	30±5	30±5	25±5	2	55	25	15	1
2	26±3	7±4	15±3	4	58	21	0	1
3	33±9	13±4	110±13	3	50±10	18±8	95±5	2
4	29±1	15±2	24±1	2	63±2	63±2	0±0	2
5	25±0	11±1	13±3	2	55	50	0	1
6	33±3	38±3	10±10	2	80	40	0	1
Av.	29	19	33		60	36	18	

TABLE II

MIXTURES ON THE COLOR WHEEL (IN DEGREES) THAT WERE EQUIVALENT TO THE MODIFIED STIMULUS-OBJECTS

(G=green; O=orange; W=white. Remaining portions were black.)

S	Donkey				Leaf			
	G	O	W	No.	G	O	W	No.
1	29±1	10±1	0±0	2	79±6	27±1	0±0	2
2	31±4	6±1	0±0	3	59±0	16±3	4±4	2
3	26±1	7±0	11±2	2	42±4	10±1	3±3	2
Av.	29	8	4		60	18	2	

In a second series of experiments the material was of a darker green, like that of full-grown leaves. The donkey was this time a little larger than the leaf. There were 4 *S*s. With 3 of the *S*s, the leaf was decidedly and measurably greener than the donkey. For 1 *S* there was no difference. Table II gives the results for the 3 *S*s. As will have been noticed Table II is in perfect accordance with Table I, despite the differences in shade and size.

Besides memory color the only differences between donkey and leaf which could possibly cause phenomenal changes are the difference in *shape* and the difference between a *picture* and a real object. Why, however, should the shape of a three-divisioned leaf look greener, or show more color constancy, than the shape of a quadruped animal? True, the donkey was a little more dispersed than the leaf, but control observations showed that this factor cannot be regarded as a rival of memory color. As to picture versus reality, why should real objects look greener, or show more color constancy, than pictures when made of the very same material? In any case control observations proved that the leaf also looked greener ✓ than a simple figure that had no picture character, *e.g.* a circle.

We are then safe in stating that under our conditions memory color exerted a noticeable and measurable influence upon the color of objects exposed under adverse illumination. Traces may color perceptual objects. It remains to specify the conditions.

Four features proved to be relevant: (1) The two objects should not be exposed simultaneously side by side. There is that peculiar lability of color phenomena under adverse illumination which is liable to rule out a difference as soon as strong factors favoring equality are brought in. This tendency towards *'assimilation'* is not a function merely of the amount of difference, for we found that the two equivalents, though having by definition the same difference, were much less endangered by assimilative tendencies. They held their own even when exposed side by side. (2) The illumination should not be too one-sided, *i.e.* too *monochromatic*. For an effect of memory color to show itself as markedly as it did in our experiments we found it necessary for the memory color to have some degree of sensory support, *i.e.* to increase a given shade rather than to create one that is not represented at all on the stimulus side. (3) In our setting, the awareness of a separate and *abnormal* illumination was not itself a function of memory color—as it would have been had it been left to the leaf to indicate to *S* that something must be 'wrong' with the illumination. There are cases of this sort, however, and it is here that a more drastic effect of memory color may occasionally be observed: a sudden switch-over to a different illumination with correspondingly different surface colors. (4) *S* should not know the issue and thereby be led to adopt an unduly critical or abstract *attitude*. As stated above, the influence of no other factor involved in color constancy is so liable to be impaired by a 'subjective' attitude *i.e.* abstracting from the colored object, as is memory color. It is obvious that an effect issuing from the object as such must require a relatively 'objective' (concrete) attitude. What was not predictable, however, certainly not on the basis of Katz' statement,[17] is the fact that memory color proved tenacious enough to maintain itself in spite of an attitude of comparing and matching colors which certainly is itself a pretty abstract and artificial one. This means at the same time that the difference between the equivalents represents only the lower limit of what difference might be experienced with a more natural attitude.

Experiments on memory taste. The following experiments were carried out to show whether, how, and to what extent, memory taste may affect actual taste.

Procedure. To arouse customers' buying curiosity, I take it, some firms recently placed upon the market 'white chocolate,' a cream-colored substance in which only the oil of the chocolate fruit is used and which tastes like milk chocolate but for a slight reduction of flavor (at least in the kind we used). To find out whether the milkish or creamlike visual appearance of this kind of sweet would suggest a corresponding element of taste appearance, *S* tasted, in succession: (1) a piece of ordinary brown milk chocolate, while blindfolded; (2) a piece of white chocolate, while blind-folded; (3) as (1) but with eyes open; and (4) as (2), with eyes open. If the influence we are looking for existed, (4) should taste more milky, or less chocolate, or at least more different from the rest, than (2). After each experience, *S* was asked to describe it, and, from (2) onwards, to compare it with the preceding experiences. Between two successive trials he was asked to drink some water to cleanse his mouth from the previous taste. The transition from the blindfolded to the seeing state was made to seem like an unintentional occurrence. While in (1) and (2) strict care was taken that *S* had his eyes closed before the chocolate was taken out of the bag, when it came to (3) this precaution was not only dropped but in the pause preceding it the experimenter, as though in preparation for the next trial and while casually conversing, handled the next piece in front of *S* until he was sure that it had been noticed; if *S* remarked that, unfortunately, he had got a glimpse of it, the experimenter would say: "Oh, that does not really matter." The same happened before presentation of (4) with still less pretended caution.

Results. Seven subjects took part in the experiment. In four cases the fourth piece was reported to taste milkier than the rest, including the second ("too buttery," "taste of condensed milk," "like creamtoffee," twice) in one case it was said to have "no chocolate taste, less than the rest," and in another case it had "less taste than any." Only in one case there was no difference, but this subject (Professor Katz) was also the only one who knew of white chocolate. He had at once become suspicious, especially when the second piece was given, that white chocolate was in the game. This case is then to be subtracted, leaving *100%* (more or less)

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[17] Katz, *op. cit.,* 162.

positive cases, of which ⅔ were in terms of milkishness and ⅓ in terms of 'non-chocolate' or 'different.'

In four more experiments, in order to improve the experimental technique, we changed the instruction to the effect that *S* was explicitly asked to compare (2) with (1) and (4) with (3) and to compare the respective differences. To our sincere surprise there was *no effect* of memory taste in any of these additional four cases. Gradually the reason dawned upon us and was then amply verified by pertinent remarks of the *S*s. The instruction to compare (2) with (1), and (4) with (3), had managed to arouse in all four *S*s a suspicion that objectively (3) and (4) were identical with (1) and (2). This suspicion arose at the latest when the blindfolding was abandoned after (2), which, although it had not been done any more conspicuously than in the first experimental series, now coincided with the hiatus between the two pairs and therefore no longer passed as harmless chance. We do not venture to say that a very keen and trained introspectionist could not have resisted falling for the stimulus-error, but, however that may be, we do think that the suspicion was not the only factor reducing the effect of memory taste. The very grouping in the rhythm 1 2′, 3 4′, with its configurational bias towards assimilation between analogous members of the two pairs could not but help to reduce the effect (compared with the earlier arrangements where the fourth piece had a more homogeneous background from which to stand out). At any rate it is in itself an interesting fact that so minute a difference in the procedure could overthrow the memory effect. This points to the highly complex nature of memory taste and is in good accordance with what was found in the case of memory color (cf. the overthrow of the effect when the two objects were exposed simultaneously side by side).

Conclusion. The experiments reported in the two preceding sections (on memory color and memory taste) have shown that there are conditions under which past experience (some trace-system) affects sensory material, no search (or vector) being involved. These conditions were found to be analogous to those applying to sensory organization (configuration proper): (1) weak opposition from, or, better still, some support from present stimulation; (2) a perceptual attitude that, instead of 'skimming off' (and thereby actually creating) 'sense-data,' aims at 'the true' properties of objects known or expected. Memory qualities thus represent the most elementary, still quasi-sensory, way in which knowledge may 'transcend' immediate stimulation.

All those types of learning-effect so far discussed belong to a special class which should be well marked off from another class containing instances of *indirect* influence of past experience. There is a great variety of cases in which past experience comes in but does no more than supply conditions for other factors to work upon. It sets, as it were, the stage for the dynamics of forces which in themselves have nothing to do with past experience or learning (at least not with the learning that set the stage). Only recently this sort of indirect influence has been explicitly acknowledged within the field of perception.

In experiments on the effect of past experience upon induced movement Krolik found that a perceived picture whose object is known as occurring within some typical setting would tend to reproduce this setting and thereby affect other perceived objects almost as if that setting too had been actually perceived.[18] Thus the picture of a lighthouse which was objectively moved in relation to the picture of a ship would, by calling up the whole maritime scene, induce the ship to move—according to the general law that of two objects in relative movement one of which forms the environment or 'frame of reference' of the other, the latter will tend to take over the movement.[19] That is to say, it is not the movingness, but the surroundedness, of the ship that is 'copied' from past experience.

Among the stimulus-objects used by Mikesell and Bentley, there were 'conventional' ones, *e.g.* undistorted letters, in which past experience made for a more unitary configuration which then, in its turn, affected contrast, through laws that apply to unitary configurations (and contrast) as such, no matter how they have arisen.[20] (This shows that convention may work *through* configuration.) A similar sort of thing may occur in all constancies, because here, essentially, two variables are involved, an object and a medium, which are so interrelated that if past experience affects the one it cannot but indirectly affect the other too. Thus an empirical cue for size must affect distance and an empirical cue for distance must affect size. (In our experiments on memory color we had a direct influence since the illumination was already fully defined by other cues.)

There are cases, closely akin to that of Krolik, where past experience does nothing but determine the *level*, the *norm*, by deviation from which an object then acquires new properties. For example a noise is 'loud' when preceded by low noises. These cases are of great importance in the field of tertiary qualities. An unusual costume looks 'odd,' an unusually scanty (or rich) costume may look 'indecent' (or 'pompous'), not because, properly speaking, habit determines indecency or pompousness, but because habit determines the level, a deviation, in a certain direction, from which is the adequate stimulus for the quality 'indecent,' or 'pompous.' Anthropologists, in stressing the rôle of custom, have been inclined to overlook the indirectness of much of its influence.

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[18] Walter Krolik, Über Erfahrungwirkungen beim Bewegungssehen, *Psychol. Forsch.*, 20, 1935, 47-101.

[19] This law is not itself a product of past experience because it also applies to objects not known as moving.

[20] W. H. Mikesell and Madison Bentley, Configurations and brightness contrast, *J. Exper. Psychol.*, 13, 1930, 1-23. Cf. also Wolfgang Metzger's discussion Gestalt und Kontrast, *Psychol. Forsch.*, 15, 1931, 374-386, esp. 382.

DIFFERENTIAL EFFECTS OF CURARE UPON HIGHER AND LOWER LEVELS OF THE CENTRAL NERVOUS SYSTEM

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In an earlier article from this Laboratory,[1] Girden and Culler demonstrated that a conditioned pattern could be acquired, retained and revived by curarized dogs. The animals are prepared as follows. Both semi-tendinosus muscles are exposed; either one contracts sharply when the homolateral hind paw is duly stimulated (electric shock). The right member of the pair is first conditioned until it replies promptly and vigorously to the sound of a bell. The animal is then curarized intra venam; whereupon we find that, even though this right muscle is still capable of a slight but clear twitch when the paw is stimulated, the CR (conditioned reaction) which was previously manifest has utterly vanished. The left semi-tendinosus is now conditioned under curare to the same sound (shock to left paw), until it twitches every time the bell is rung. Upon recovery from the drug (three or more hours later), the dog is again tested with bell; the right muscle's *normal* CR reappears clear and strong, whereas the curare-CR in left muscle betrays no sign of its presence. Upon re-curarization, bilateral reversal once more occurs; the normal CR on right side wholly vanishes while the curare-CR on the left side again emerges. The facts schematize as follows:

Condition	Semi-tendinosus muscles	
	Right	Left
Normal	CR established	
Curare	CR vanishes	CR established
Normal	CR reappears	CR vanishes
Curare	CR vanishes	CR reappears

These results carry a real theoretic challenge. We see that a dog can be independently conditioned on separate levels or by disparate patterns

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[1] E. Girden and E. Culler, Conditioned responses in curarized striate muscles in dogs. *J. Comp. Psychol.*, 23, 1937, 261-274.

of the central nervous system to the same stimulus at the same time. It may well be called an experimental form of *dual personality:* the animal replies by two independent behavior-systems to the same stimulus, one in one state and one in the other. Normal learning proceeds at cortical levels, curarized learning at other (presumably sub-cortical) levels.

The following mechanism was hypothetized to account for these phenomena. The animal under curare is functionally decorticate (cortex depressed); what conditioning occurs is, therefore, sub-cortical in nature and locus, being inhibited when the cortex returns to dominance. Conditioning of the normal animal however is predominantly cortical in nature and locus, and therefore disappears when curare depresses cortical activity.[2]

In this paper we seek to test the validity of this inferential mechanism with evidence drawn from direct stimulation of motor roots and of cortex.

Procedure. Under complete general anesthesia (ether), the trachea is cannulated and the right M. semi-tendinosus dissected out. After its distal end has been freed, the blood and nerve-supply being kept intact, the muscle is available for ready observation and manipulation. Several segments of the cord (sixth and seventh lumbar, first sacral) are so exposed that the ventral roots which innervate M. semi-tendinosus can be fully isolated and directly stimulated with suitable electrodes. The left motor cortex finally is laid bare around the cruciate sulcus. With the animal thus prepared, the head is snugly secured in a holder, the fore and hind limbs being so disposed as to be comfortable for the animal and convenient for examination by the observer. Stimulation is provided by a rectangular pulse of variable amplitude and duration, generated in a chronaximeter with a pentode-circuit so designed as to maintain a horizontal current of uniform amplitude despite large and sudden changes in tissue-resistance. This latter feature is of utmost value and import, since the stimulus remains true despite unavoidable and unknown changes in moisture, ionization and polarization. Unipolar electrodes, with diffuse positive lead to head-holder or to the animal's exposed skin, are used; a silver bulb, less than 1 mm. in diam., or in lieu thereof an acuate wick soaked in blood serum or saline, on the cortex; on the cord a silver hook, fitted to disengage the motor root gently from body-fluids and hold it free of adjacent tissue. With both cortex and motor-root in each animal, a spot was first located which seemed to be optimal for stimulation; that is, for eliciting a perceptible contraction of M. semi-tendinosus with lower current-density (during 500 ms.) than any other. This point was duly noted and used throughout all later tests. During stimulation every care was employed to keep each spot warm (with the lamp which supplied both light and heat) and moist (with physiological saline or blood serum); but, to obviate dissipation of the stimulating current, each spot was gently and effectively dried just prior to applying the electrode. Indeed, a special assistant stood by as needed, to mop the ventral root with cotton pledgets. Two normal limens were regularly secured for cortex and spinal root; if these two differed by more than 5 to 8%, stimulation was continued with added precaution until figures within that range were achieved. Thereupon curare was cautiously introduced (through V. saphena) until standard symptoms appeared: respiratory paralysis, depression of corneal lid-reflex, muscular flaccidity and inertness under stimulation. Mechanical ventilation was in all cases required. After curare-limens had been determined for both cord and cortex once or preferably twice, the animal was given time to recover, insufflation being continued until every trace of respiratory blockage had vanished. After not less than three hours, final control tests were made; no data are included without these check-readings from the post-curare normal state.

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[2] *Ibid.*, 273 f.

Excluding operative fatalities, no less than 36 dogs were used; but our protocols for over half of them were too meager for use. Curare is at best a tricky instrument, never pure, rarely uniform. A good many succumbed before final recovery from the drug; in other cases it was virtually impossible for obscure reasons (some independent variable or some imperceptible lesion?) to determine what the rheobase truly was. Despite this wastage, the 12 dogs listed below contributed each one or two complete sets of data: a 'set' consisting of six independent measures, one each before, during and after curare for both cortex and ventral roots. It is the records from these dogs which are here reproduced and analyzed.

Limens, whether of current or of duration, were measured in the customary manner. Of three collaborators, one would manipulate the negative electrode, guarding the tissue as above explained; a second would prepare optimal conditions for observing, under controlled illumination, the exposed M. semi-tendinosus, whose response was the ultimate criterion for all thresholds. Since but a few fibers contract under liminal stimulation, the locus and pattern of this critical response had to be studied and noted with care for use in all later tests on that animal.[3] A third collaborator operated the chronaximeter, first setting the current amply high for a generous response, then attenuating it in small stated steps until the muscle fell from 100% through 50% to 0% in its replies; then starting below zero, he raised the pulse-amplitude until the muscle-response again appeared each time. The mean of the two 50%-points was taken as the 'limen.'

Results. The records divide properly into two groups: (a) the *time,* whether in milliseconds (cortex) or micro-seconds (ventral roots), required for a current whose magnitude equals twice the normal rheobase, to evoke the critical (just perceptible) response of M. semi-tendinosus; (b) the *current,* whether in milliamperes (cortex) or micro-amperes (ventral roots), required to elicit the same criterion. In all cases, we first measure the liminal current (rheobase) for indefinite time (500 ms.); this current is then doubled and the liminal duration (chronaxy) measured. But under curare, though the rheobase has changed, we keep on using the same current-amplitude as in the normal state. In this paper we are not concerned with the chronaxy as such; rather with the *relative durations* of the same current in the two states. This duration happens to be true chronaxy in the normal state but not so in the other; hence the term, with all its controversial implications, is carefully shunned throughout this paper.

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[3] The normal locus and pattern of response may change in some degree under curare.

TABLE I

Minimal Time Required to Evoke Critical Response of Right M. Semi-Tendinosus Under Normal and Curarized Conditions

Dog	Cortex (ms.)			Motor roots (mms.)		
	normal	curare	normal	normal	curare	normal
1	5.70	10.90	5.70	67	74	67
2a	5.30	5.30	5.30	76	53	53
2b	5.30	6.40	4.70	53	76	59
3a	3.20	4.00	4.00	112	89	89
3b	4.00	5.50	4.00	89	167	89
4	5.40	4.80	2.50	114	75	83
6	6.70	6.40	7.10	71	75	67
7	6.70	5.65	5.40	68	110	73
8	5.50	9.10	5.50	71	40	89
9	5.00	5.50	5.50	72	45	76
10	5.30	4.20	2.60	93	77	138
11a	5.50	7.80	4.80	68	110	88
11b	4.80	5.50	4.50	88	58	75
12	6.60	7.80	4.95	144	81	64
14	5.20	5.20	4.60	260	105	245
(1) Sums	80.20	94.05	71.15	1446	1235	1355
(2) Means	5.347	6.270	4.743	96.4	82.3	90.3
(3) Mean-diff.	0.923	1.527		14.1	8.0	
(4) S. E. M.$_{diff.}$	.445	.444		13.9	12.9	
(5) Values of t	2.074	3.432		1.014	0.620	
(6) Values of P	.029	.002		.167	.273	
(7) Values of $-\log_e P$	3.5405	6.2146		1.7898	1.2983	

In Table I are mustered all the cases for which complete records from both cortex and motor root before, during and after curare were derived: 9 dogs with one complete set each and 3 with two sets each, a total of 15 acceptable records. Considering the cortex, we see that liminal duration for twice the rheobase rises from 5.347 ms. in the normal state to 6.270 ms. under curare and then again falls to 4.743 ms. as normalcy returns. Under curare the liminal stimulus-duration lengthens by 0.923 ms., after curare it again shortens by 1.527 ms.; the mean change being 1.225 ms. The values of t (critical ratio) for these magnitudes are, in order, 2.074, 3.432 and 3.000. In a system with 14 degrees of freedom,[4] the last two of these are dependably 'significant,' there being well less than one chance in a hundred of drawing so great a change by random sampling of a homogeneous distribution. The first change ($t = 2.07$) is probably, though not surely, dependable. Considering the three figures, however, we can state with assurance that, given a constant amperage (ordinate), the duration (abscissa) and therefore the quantity (area) of electric energy must be reliably augmented under curare to evoke a critical response of the S-T muscle.

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[4] R. A. Fisher, *Statistical Methods for Research Workers*, 5th ed., 1934, chap. 5.

In the motor roots, the picture is indeed different. The mean excitation-time falls by 14.07 mms. (micro-seconds) under curare, rising again by 8.00 mms. post curare; the mean curare-effect being 11.03 mms. The critical ratios are, in order, 1.014, 0.620 and 0.853. The mean stimulation-time under curare is lower than either before or after, but the change is not dependably greater than zero. The sole alternative conclusions are, that excitation-time for the ventral roots under curare is *either* the same as, *or* shorter than, in the normal state. We can thus assert with fullest assurance that stimulation-time under curare does *not* lengthen at the motor roots as it does in the cortex.

TABLE II

Minimal Current Required to Evoke Critical Response of Right M. Semi-Tendinosus Under Normal and Curarized Conditions

Dog	Cortex (ma.)			Motor roots (mma.)		
	normal	curare	normal	normal	curare	normal
1	1.875	2.250	1.875	0.200	0.150	0.250
2	1.375	1.375	1.125	0.475	0.350	0.450
3	0.850	0.950	0.775	0.700	0.450	0.750
4	0.800	0.950	0.750	0.200	0.100	0.200
(1) Sums	4.900	5.525	4.525	1.575	1.050	1.650
(2) Means	1.225	1.381	1.131	0.394	0.263	0.413
(3) Mean diff.	0.156	0.250		0.131	0.150	
(4) $\text{S. E. M.}_{\text{diff.}}$	.0793	.0445		.0425	.0500	
(5) Values of t	1.967	5.618		3.082	3.000	
(6) Values of P	.0721	.0056		.0270	.0288	
(7) Values of $-\log_e P$	2.6297	5.1850		3.6119	3.5474	

In four dogs (see Table II) we compared the minimal current instead of minimal time. We see that, when time is fixed at 500 ms., the current must be increased; just as, when current was fixed at twice the rheobase, duration had to be increased. The critical ratios (t) for cortex are 1.967 and 5.618, for motor roots are 3.082 and 3.000. With but three degrees of freedom, only 5.618 is comfortably 'significant.' The mean difference between curare and control is 0.203 (cortex) and 0.141 (motor roots); their critical ratios are 3.484 and 3.097. Here again we may infer that curare increases the quanta (current × duration) of electric energy required to stimulate, while it probably, but not surely, sensitizes the motor roots. Since $Q = i \cdot t$ (quantity of electric energy equals current times duration), we see that curare depresses cortical function, whether measured by duration or current and does *not* depress motor roots by either criterion. The statistical analysis may with profit be carried further. Even though some of the curare-changes are not separately dependable, their complete concurrence (all the cortical limens, whether of time or current, being elevated and all the peripheral limens being depressed) strongly suggests that their combined significance is far greater. When each item reënforces the others, the aggregate is more significant than is any of its parts; even as a mean is more stable than are the component measures. By Fisher's method for estimating the significance of an aggregate,[5] we determine (1) the chance-frequency, P, of each item and (2) the value of $-\log_e P$ for this chance-frequency. Twice the sum of these values in (2) equals X^2 for that particular combination of items, whether pre- or post-curare changes, in cortex or motor roots, for excitation-time or current. We can thus estimate what degree of confidence may safely be attached to each degree of complexity from the single item to the whole system.

In the subjoined tabulation, 'pre' is before and 'post' is after curare; 'time' is liminal excitation-time and 'current' is liminal value of direct-current pulse; 'C' is cortex and 'M' is motor-root; 'P' is the frequency with which a given combination would occur in random samples from a population which is really homogeneous. All estimates of 'P' are taken from "Student's" table with due regard to degrees of freedom.[6] When $P = 0.010$ or less, the result may with full confidence be accepted as significant.

Interpretation and summary. This study has no concern with technical problems of chronaxy, such as appear in the many reports of Lapicque, Monnier, Rushton and others.[7] The questions and dubieties which beset this topic need not delay us here. We have the modest but definite task of interpreting the above material in conjunction with the earlier data of Girden and Culler on conditioning in the curarized dog.

(1) We can say, with full assurance ($P < .010$; cf. Table III, A 1 and B 1) that curare elevates *both* the cortical rheobase, i (excitation-time being fixed at 500 ms.) *and* the cortical excitation-time, t (current being fixed at twice the animal's normal rheobase). In either case, the product $i \cdot t$ indicates the number of electric quanta which must be delivered to the cortical motor-point ere a response of contralateral M. semi-tendinosus can just be detected. By each criterion, more energy is needed to excite the neuro-motor mechanism. The depressant action of curare upon the higher levels of a typical motor response thus seems to be clearly demonstrated.

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[5] *Op. cit.*

[6] Student: New tables for testing the significance of observations, *Metron,* 5, 1925, 105-108.

[7] H. Davis and A. Forbes, Chronaxie, *Physiol. Rev.,* 18, 1936, 407-441.

(2) The same response of M. semi-tendinosus, when initiated at the ventral root, shows no signs of depression. The limens for both excitation-time and current are probably, but not dependably, reduced (cf. Table III, A 2, B 2, C 6); but in any case one can say with certainty that they are *not* elevated, as in the cortex. It is notable that transmission over the neuro-myal junction is fully as efficient for certain elements of the excitatory mechanism as under normal conditions. We see again the "hobble-effect" previously described;[8] that whereas a complete response is abolished by curare, the minimal response is not likewise affected; the muscle proves to be, if anything, more responsive in some of its fibers to liminal stimulation than ever. Whereas some fibers are effectively blocked, other paths are still kept open. It is one of the special features of our method that this selective action by the drug is clearly revealed. These twitches, minute in both amplitude and area, are unable to move the entire leg and are thus quite imperceptible when the limb is intact. Further the observer, with optimal placement and illumination of the exposed muscle, can spot the exact locus and distribution of active fibers better, we think, than by electric recording.

TABLE III

SUMMARY OF RESULTS

A. Time	P	B. Current	P
1. Pre and post, C	$<.010$	1. Pre and post, C	$<.010$
2. Pre and post, M	.189	2. Pre and post, M	$<.010$
3. Pre, C and M	.031	3. Pre, C and M	.014
4. Post, C and M	$<.010$	4. Post, C and M	$<.010$
5. Pre and post, C and M	$<.010$	5. Pre and post, C and M	$<.010$
C. Time and Current	P		P
1. Pre, C	.015	5. Pre and post, C	$<.010$
2. Post, C	$<.010$	6. Pre and post, M	$<.010$
3. Pre, M	.029	7. Pre, C and M	$<.010$
4. Post, M	.047	8. Post, C and M	$<.010$
9. Pre and post, C and M	$<.010$		

(3) The most sensitive fibers, which reply to minimal stimulation in the normal state, will not always retain their primacy under the drug. Instead, others which normally are above the limen will take over the liminal response.[9] This fully demonstrates the selective character of curare-action: some fibers (neural or myal) seem to be depressed; others to be sensitized.

(4) Somewhere between cortex and ventral-root passes a *plane of cleavage:* a surface whose existence is demonstrated by our data even though

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[8] Girden and Culler, *op. cit.*

[9] This will be more fully treated in a separate paper.

its location and contour cannot be definitely stated. To one side of the boundary (cephalad) is a zone of depression; to the other (caudad) is a non-depressed (possibly sensitized) area. In the former paper,[10] the boundary was assumed to lie between cortex and thalamus; the curarized animal being still capable of conditioning, Girden and Culler reasoned that the surface of differentiation extended above the diencephalon. Later evidence indicates the possibility of conditioning on still lower levels even down to the spinal cord;[11] hence it is not impossible that the plane of cleavage passes somewhere *below* the thalamus in our animals.[12]

In any case, this evidence in conjunction with the previous report by Girden and Culler, clearly indicates that "dual personality" may be effected by the differential action of curare upon higher and lower levels of the central nervous system. In the present state of knowledge, personality-cleavage is a concept derived wholly from behavioral evidence; for which concept the above data provide a possible physiological mechanism in the neural structures. How this experimental scission of a dog into two distinct behavior-systems by selective action of curare resembles the multiple personalities of human pathology is not obvious. In man the secondary state often appears to be infantile and disinhibited, as though it were relieved (as in our dogs) of cortical dominance; but in practice it seems clear that the plane of cleavage will seldom have the same locus or configuration in any two human cases. In any event, the above material provides some neural confirmation, and rationalization for the evidence that a behavior-system can be temporarily split by the differential action of drugs.

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[10] Girden and Culler, *op. cit.*

[11] P. S. Shurrager and E. A. Culler, Phenomena allied to conditioning in the spinal dog, *Amer. J. Physiol.*, 123, 1938, 186-187.

[12] Since Girden and Culler's animals were conditioned to sound, the plane must in this case pass somewhere *above* the cochlear nuclei in the medulla.

APPARATUS

MEASURING THE STEADINESS OF MANUAL MOVEMENT

By HERBERT GURNEE, Western Reserve University

There has long been need for a test of the steadiness of manual movement, a test that would meet modern standards. Most instruments of this sort have failed to meet the essential requirements of reliability. Thus the familiar tracing board of Whipple was early described as unreliable by Langfeld,[1] and recently the authors of the Minnesota Mechanical Ability Tests[2] have reported for it a test-retest reliability of 0.32 (error scores) and 0.40 (point scores). A paper tracing test has fared almost equally poorly.

The present article describes the results of an effort to develop a satisfactory tracing-movement test. By *satisfactory* is meant a test which possesses high reliability, simplicity, easy administration, portability, and low cost.

The construction of the instrument is as follows.[3] In a brass sheet, 1/32 in. thick, 22 cm. long by 8 cm. wide, a figure is cut, in the shape of an inverse S, which is 19 cm. long (linear measurement) and 4 mm. wide. The sides of the figure are arcs of true circles with radii of 66 mm. inside and 70 mm. outside. Each side of the figure is broken at equal intervals by 9 V-shaped notches, 3 mm. deep and 4 mm. across the mouth. The figure terminates at each end in a hole 1 cm. in diam. This plate is fastened to a piece of 5-ply maple, 26 cm. long, 16 cm. wide and ⅜ in. thick, into which a figure is cut corresponding to that in the metal, but 2 mm. wider and without notches. The wood is cut with an underbevel, so that a stylus will touch it only at the notches and at the ends of the figure. The insulation thus provided at the ends is introduced to prevent contact before *S*'s movement starts and after it is completed. The insulation at the notches serves the purpose of compelling breaks in the circuit if *S* tries to run along the side of the plate. The notches provide a means of controlling the rate of his movement. One unsatisfactory feature of the tracing board has been the impracticability, not to say impossibility, of ensuring uniform rate of movement.

The maple board, with plate attached, is fastened, at an angle of 45° vertically, to a wooden box 22½ cm. high, 19 cm. deep and 16. cm wide. A terminal near the top gives electrical connection with the plate. Rubber-tack bumpers are fastened to the bottom of the box to prevent slipping.

The instrument is placed in series with an electric counter (Stoelting no. 22407), four dry cells and a stylus. The stylus is made by Stoelting for use with the Whipple tracing board and has a 1/16-in. pin.

Because the top of a table proved too high, the instrument was placed on a shelf made by clamping a board to the underside of an ordinary kitchen-size table.

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[1] H. S. Langfeld, Voluntary movement under positive and negative instructions, *Psychol. Rev.*, 20, 1913, 459-478.

[2] D. G. Paterson, R. M. Elliot, L. D. Anderson, H. A. Toops, and E. Heidbreder, *The Minnesota Mechanical Ability Tests*, 1930, 87.

[3] The instrument is now obtainable from C. H. Stoelting Co., Chicago.

The top of this shelf was 64 cm. from the floor. *S* sat in a straight-backed chair (about 46 cm. high), directly in front of the instrument and half an arm's length away. A daylight desk-lamp was placed on the table just over the instrument, because in no other way could the production of shadows be eliminated. The room had only one window.

The rate of movement was controlled by a metronome which was set at 100 beats per min. Preliminary experimentation indicated this to be a satisfactory rate. *S*'s instructions were as follows:

"This is a test of steadiness. Insert the pin of the stylus about one-third to one-half of the way into the groove and move without stopping from the upper end to the lower end and back trying to avoid touching the sides. Each time you touch the sides an error will be recorded against you. Move at the rate of one notch to each beat of the metronome. It will help to watch the stylus from the side so as to see where you are going. Also, it will be to your advantage to keep the pointer perpendicular, because, if you slant it, you will be closer to the sides. You will get one practice trial with each hand and then six more trials with each hand."

TABLE I

Decile Norms for the Average Number of Contacts in Six Trials with Each Hand

Decile	Right hand	Left hand
10	0 - 4.6	0 - 8.4
9	4.7- 5.8	8.5- 9.6
8	5.9- 6.9	9.7-10.8
7	7.0- 8.0	10.9-12.0
6	8.1- 9.0	12.1-13.2
5	9.1-10.0	13.3-14.3
4	10.1-11.0	14.4-15.4
3	11.1-12.0	15.5-17.6
2	12.1-15.0	17.7-19.6
1	15.1 or more	19.7 or more

Appropriate demonstration accompanied the instructions, and the subject was cautioned against supporting his arm in any way. As a matter of fact, use of the instrument requires a freely moving elbow. The trials started with the right hand and alternated left and right thereafter until fourteen trials in all had been completed. A rest period of about 20 secs. intervened between trials, and during this time *S* was told to relax. He was not permitted to smoke or to leave his seat. Meanwhile *E* took the record of the counter. Forty-five male undergraduates, chosen at randon from an elementary course, were used as *Ss*.

In the treatment of results, the score was determined by dropping the first record for each hand and averaging the remaining six records. The higher the score, the poorer was the performance. The reliability of the instrument was determined by correlating the averages of the three odd trials against the averages of the three even trials. The resulting coefficients were 0.818 for the right hand and 0.765 for the left hand. These values do not quite reach the point, 0.85, generally accepted as the minimum of adequate reliability. By applying the Spearman-Brown formula, and doubling the number of trials, however, we are able to raise these coefficients to 0.900 and 0.867 respectively; and, since this doubling procedure corresponds to what would be a test-retest situation, it is reasonable to conclude that the instrument satisfies the conventional requirements of reliability.

The average scores for the 45 *S*s were 9.215 for the right hand and 14.102 for the left hand; the corresponding standard deviations were 3.726 and 5.658, respectively. The difference between these averages is 4.887, and the critical ratio of this difference (quotient of difference by the standard error of the difference) is 4.79, which is high enough to dissipate any doubt about the capacity of the instrument to discriminate handedness.

Decile norms for the average number of contacts in six trials with each hand have been computed and the scores are given in Table I. They are applicable, of course, only to young men and must be taken as approximations until confirmed or corrected by further standardization on larger samplings.

AN INEXPENSIVE DEVICE FOR MEASURING ROTATIONAL SPEED

By CLIFFORD P. SEITZ, Columbia University, and H. B. SHAPIRO, New York City

In searching for equipment with which to measure the rotational speed of motors and disks, the psychologist is compelled to choose from instruments designed for commercial purposes or to construct devices of his own.

At present there are available commercially tachometers which consist of a magneto generator that is directly connected to an indicating instrument calibrated in convenient units such as miles per hour or revolutions per minute. This equipment is expensive and requires reduction gearing for use at low speeds at full scale. There are other possibilities, such as bridge speedometers, which are efficient at high speeds; and pentode tube-meters operating on a condenser discharge principle, with a range of 0–15,000 r.p.s. Neither of the latter is easily constructed outside of a physics laboratory since considerable apparatus is required for calibration. Finally there is the stroboscope which, in its commercial form is expensive and of limited accuracy.

We have found the automobile speedometer a useful instrument for measuring speed in the study of flicker phenomena. Modern speedometers (since 1930) operate on the magnetic principle. A revolving magnet, positively driven from the motor shaft, sets up eddy currents which influence a metal part separated from it by a small air gap. This part is directly connected to the indicating lever which is pivoted in jeweled bearings to increase its sensitivity to small changes in current. An important feature of the design is the linear relation between speed of rotation of the permanent magnet and movement of the indicator. Adjustment is made for changes in drag due to temperature variations. The accuracy of these speedometers is given as ± 1 % at full scale. This accuracy is as good as is found in the best of the tachometers referred to above and considerably better than some.

These instruments are available new at about $15 from a number of manufacturers. Recent (airplane) models have face dials ranging in size from 2¾ to 4¼ in. in diam. The larger dials are preferable. They may also be bought from a second hand dealer at about one-fifth of this figure. When purchased new they can be obtained with a calibration in revolutions per minute. Usually the scale runs from 0–3500 r.p.m., but other ranges may be obtained. When purchased from a second hand dealer they are, of course, calibrated in miles per hour.

At present we are using a speedometer salvaged from a late model Ford. In making the calibration, the speedometer was connected by a flexible shaft (supplied by the manufacturer in lengths up to 20 ft.) to the motor or rotating shaft. This was managed by imbedding the distal end of the flexible shaft in a small piece of round brass stock and connecting motor shaft and flexible shaft with a section of rubber tubing. A gear (16 teeth, P.D. ½ in.) was fastened to the rotating shaft and meshed with a bronze gear (160 teeth, P.D. 5 in.). This served to reduce the speed of the larger gear ten times. A black paper disc was fastened to the larger, slower gear and marked with a white spot. The time for ten complete cycles at various speeds was obtained by means of a 1/100-sec. chronoscope. The plot of this data is shown in the accompanying figure (Fig. 1). It is to be noted that the values plotted are two times full scale since our rotating disk had two sectors and thus gave twice as many cycles as revolutions per second. The significant thing is the linear relationship between rotation and dial readings.

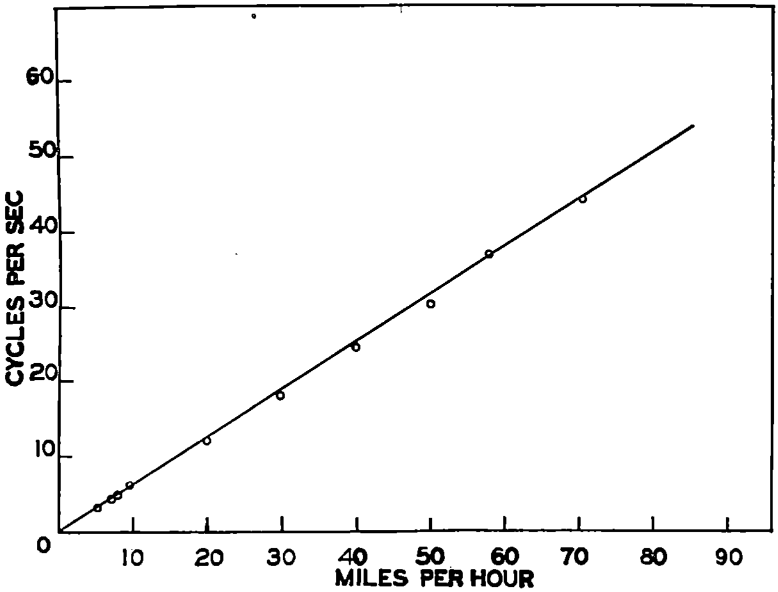

Fig. 1. Calibration of Device for Measuring Rotational Speed

Such an apparatus has several pronounced advantages. It is inexpensive; rotational speed is read directly; its calibration does not change since the instrument is meant for constant use and is, therefore, ruggedly designed; it may be simply mounted; and it may be illuminated indirectly, an important factor under conditions where it is necessary to keep the subjects dark-adapted.

NOTES AND DISCUSSIONS

DISTRIBUTION OF THE PSYCHOLOGISTS STARRED IN THE SIX EDITIONS OF *American Men of Science*

One hundred and eighteen psychologists, selected by a secret vote of the leading research workers in psychology, have been starred in the six editions of Cattell's *American Men of Science* (see List 1). The first group, 50 in number, were selected in 1903 by a dozen recognized leaders in psychology. Subsequently, groups of 12 (1909), 18 (1921), 12 (1927), 13 (1932), and 13 (1937) were selected by vote of all previously starred psychologists and men nominated for starring by one or more of the psychologists already starred.[1]

TABLE I

REGIONAL YIELD OF STARRED PSYCHOLOGISTS

(Number born per million of population or immigrants in 1870, the average date of birth.)

Regions	No.	Yield	States	No.	Yield	Cities	No.	Yield
New England	23	6.2	N. H.	3	9.4	Burlington, Vt.	2	100+
Middle Atlantic	22	2.5	Mass.	12	7.9	Boston & Sub.	6	20.0
E. N. Central	29	3.2	Conn.	3	5.6	N. Y. City	4	2.6
W. N. Central	7	1.9	N. Y.	9	2.0	Philadelphia	5	7.0
S. Atlantic	7	1.2	Pa.	10	2.9	Chicago	2	7.0
S. Central	1	0.1	N. J.	3	3.3	Laramie, Wyo.	2	200+
Mountain States	3	10.0	Ohio	7	2.5	Oakland, Calif.	2	100+
Pacific States	3	4.3	Ind.	7	4.1			
			Ill.	10	4.0			
Total U. S.	95	2.5	Wis.	3	3.0			
			Ia.	3	2.5			
Canada	4	8.0	Calif.	3	5.0			
England	4	5.0						
Germany	5	3.0						
Total Immigrants	23	4.0						

The star indicates that, in the private opinion of his peers, the starred psychologist is distinguished for psychological research. It implies either a large volume of good work or a considerable amount of especially original work. Of course it does not imply that the work done by others is not decidedly worth-while, but merely that it has not impressed the voters as quite so worthy of approbation.

The star is a recognition which not only gives the recipient satisfaction, but also increases his opportunities. It is a challenge to the recipient to continue his good work and to others who aspire to win this recognition. Vast amounts of good work have been completed as a result of friendly rivalry. Many psychologists who are not starred feel confident that they are "as good a man as . . ." and consequently set out to prove it.

The good that starring does is increased by the widened knowledge as to whom are starred and why. This widened knowledge not only encourages and puts the

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[1] Cattell's inauguration of the system of starring the leading research workers in each of twelve fundamental sciences is considered by competent judges to have been a major contribution to the growth of research in America.

LIST 1. PSYCHOLOGISTS STARRED 1903-1937

(Name; year of birth and death, if dead; and the edition in which first starred.)

Allport, Floyd H.,	1890– — , *4
Allport, Gordon W.,	1897– — , *6
Anderson, John E.,	1893– — , *5
Angell, Frank,	1857– — , *1
Angell, James R.,	1869– — , *1
Armstrong, Andrew C.,	1860–1935, *1
Baldwin, Bird T.,	1875–1928, *3
Baldwin, J. Mark,	1861–1934, *1
Bentley, Madison,	1870– — , *2
Bingham, Walter V.,	1880– — , *3
Bolton, Thaddeus L.,	1865– — , *1
Boring, Edwin G.,	1886– — , *3
Brown, Warner,	1882– — , *4
Bryan, William L.,	1860– — , *1
Buchner, Edward F.,	1868–1929, *1
Calkins, Mary W.,	1863–1930, *1
Carmichael, Leonard,	1898– — , *6
Carr, Harvey,	1873– — , *3
Cattell, J. McKeen,	1860– — , *1
Creighton, James E.,	1861–1924, *1
Dallenbach, Karl M.,	1887– — , *5
Dashiell, John F.,	1888– — , *5
Dearborn, Walter F.,	1878– — , *3
Delabarre, Edmund B.,	1863– — , *1
Dewey, John,	1859– — , *1
Dodge, Raymond,	1871– — , *1
Downey, June E.,	1875–1932, *4
Dunlap, Knight,	1875– — , *3
Farrand, Livingston,	1867– — , *1
Fernberger, Samuel W.,	1887– — , *5
Ferree, Clarence E.,	1877– — , *3
Freeman, Frank N.,	1880– — , *5
Fullerton, George S.,	1859–1925, *1
Gardiner, Harry N.,	1855–1927, *1
Garrett, Henry E.,	1894– — , *6
Gates, Arthur I.,	1890– — , *4
Gesell, Arnold,	1880– — , *4
Goddard, Henry H.,	1866– — , *3
Goodenough, Florence L.,	1886– — , *6
Hall, G. Stanley,	1846–1924, *1
Harris, William T.,	1835–1909, *1
Hollingworth, Harry L.,	1880– — , *3
Holt, Edwin B.,	1873– — , *2
Howes, Ethel P.,	1872– — , *2
Hull, Clark L.,	1884– — , *5
Hunter, Walter S.,	1889– — , *3
Hurd, Henry M.,	1843–1927, *2
Hyslop, James H.,	1854–1920, *1
James, William,	1842–1910, *1
Jastrow, Joseph,	1863– — , *1
Johnson, Harry M.,	1885– — , *5
Jones, Harold E.,	1894– — , *6
Judd, Charles H.,	1873– — , *1
Kelley, Truman L.,	1884– — , *4
Klüver, Heinrich,	1897– — , *6
Koffka, Kurt,	1886– — , *5
Köhler, Wolfgang,	1887– — , *6
Ladd-Franklin, Christine,	1847–1930, *1
Ladd, George T.,	1842–1921, *1
Landis, Carney,	1897– — , *6
Langfeld, Herbert S.,	1879– — , *3
Lashley, Karl S.,	1890– — , *4
Leuba, James H.,	1868– — , *1
Lewin, Kurt,	1890– — , *6
Lindley, Ernest H.,	1869– — , *1
MacDougall, Robert,	1866– — , *1
McDougall, William,	1871–1938, *4
McGeoch, John A.,	1897– — , *6
Marshall, Henry R.	1852–1927, *1
Martin, Lillien J.,	1851– — , *2
May, Mark A.,	1891– — , *5
Meyer, Adolf,	1866– — , *1
Meyer, Max F.,	1873– — , *1
Miles, Walter R.,	1885– — , *4
Münsterberg, Hugo,	1863–1916, *1
Newbold, William R.,	1865–1926, *1
Nichols, Herbert,	1852–1936, *1
Ogden, Robert M.,	1877– — , *3
Pace, Edward A	1861– — ,*2
Paterson, Donald G.,	1892– — , *5
Patrick, George T.,	1857– — , *1
Peterson, Joseph,	1878–1935, *4
Pierce, Arthur H.,	1867–1914, *1
Pillsbury, Walter B.,	1872– — , *1
Pintner, Rudolph,	1884– — , *3
Poffenberger, Albert T.,	1885– — , *4
Robinson, Edward S.,	1893–1937, *5
Sanford, Edmund C.,	1859–1924, *1
Santayana, George,	1863– — , *1
Schurman, Jacob G.,	1854– — , *1
Scott, Walter D.,	1869– — , *3
Scripture, Edward W.,	1864– — , *1
Seashore, Carl E.,	1866– — , *1
Sidis, Boris,	1867–1923, *2
Starbuck, Edwin D.,	1866– — , *2
Starch, Daniel,	1883– — , *3
Stone, Calvin P.,	1892– — , *5
Stratton, George M.,	1865– — , *1
Strong, Charles A.,	1862– — , *1
Strong, Edward K.,	1884– — , *3
Terman, Lewis M.,	1877– — , *3
Thorndike, Edward L.,	1874– — , *1
Thurstone, Louis L.,	1887– — , *4
Titchener, Edward B.,	1867–1927, *1
Tolman, Edward C.,	1886– — , *5
Warren, Howard C.,	1867–1934, *1
Washburn, Margaret F.,	1871– — , *1
Watson, John B.,	1878– — , *2
Wells, Frederic L.,	1884– — , *2
Wertheimer, Max,	1880– — , *6
Wever, Ernest H.,	1902– — , *6
Whipple, Guy M.,	1876– — , *2
Witmer, Lightner,	1867– — , *1
Wolfe, Harry K.,	1858–1918, *1
Woodrow, Herbert,	1883– — , *6
Woodworth, Robert S.,	1869– — , *1
Woolley, Helen T.,	1874– — , *3
Yerkes, Robert M.,	1876– — *2

starred men more fully on their mettle, but it also attracts attention to their work, and increases their opportunities for further research. It, moreover, augments the opportunities of promising persons not starred in the hope that, as a consequence of encouragement and improved facilities, they will win this coveted recognition. The various universities employing starred scientists are placing increased value upon this recognition as a proof of individual merit and institutional strength. They not only attempt to retain and attract men already starred, but also to have local men not yet starred win this high honor; to this end they often increase facilities and otherwise encourage their more promising men.

Birth. (a) Place. Table I and List 2 disclose that 95 of the 118 starred psychologists were native-born. Of these 95, the East North Central States yielded 29, New England 23, the Middle Atlantic States 22, the West North Central and the South Atlantic each 7, the Mountain and Pacific States each 3, and the South Central States 1. In proportion to population in 1870, the average date of birth, New England yielded more than twice as many as did the North Central and Middle Atlantic States, and ten times as many as the South. The Mountain and Pacific States had only small populations in 1870, and their combined yield was almost at New England's rate.

The leading states, in number produced, are Massachusetts 12, Illinois and Pennsylvania 10, New York 9, Indiana and Ohio 7, and six states with 3 each (California, Connecticut, Iowa, New Hampshire, New Jersey, and Wisconsin). Five other states yielded 2 each (Maine, Michigan, South Carolina, Vermont, and Wyoming). Twelve states yielded 1 each and the remaining nineteen states, none. Most of these latter are in the South Central section or in the West. The West had few people when most of the starred psychologists were born.

LIST 2. BIRTHPLACES: SECTION, STATE, AND CITY

New England, total 23

Maine (2)	*Dover:* Delabarre; *Vassalboro:* Goddard.
New Hampshire (3)	*Mason:* Scripture; *North Boscawen:* Patrick; *Walpole:* Nichols.
Vermont (2)	*Burlington:* Angell, J. R., Dewey.
Massachusetts (12)	*Ashfield:* Hall; *Belchertown:* Woodworth; *Boston:* Wells; *Danvers:* Whipple; *Farmingham:* Howes; *Haverhill:* Strong, C. A.; *Marblehead:* Dearborn; *Williamsburg:* Thorndike; *Westboro:* Pierce; *West Newton:* Tolman; *Winchester*: Holt; *Woburn:* Dodge.
Connecticut (3)	*Hartford:* Calkins; *North Killingsley:* Harris; *Windsor:* Ladd-Franklin.
Rhode Island (1)	*South Scituate:* Angell, F.

Middle Atlantic States, total 22

New York (9)	*Akron:* Hull; *Argyle:* McGeoch; *Binghampton:* Ogden; *New York City:* Armstrong, James, Marshall, Washburn; *Olean:* Martin; *Syracuse:* Strong, E. K.

Pennsylvania (10)	*Breadysville:* Yerkes; *Dauphin:* Poffenberger; *Easton:* Cattell; *Honesdale:* Goodenough; *Marshalton:* Baldwin, B. T.; *Philadelphia:* Boring, Carmichael, Fernberger, Langfeld, Witmer.
New Jersey (3)	*Montclair:* Warren; *Newark:* Farrand; *New Brunswick:* Jones.

East North Central States, total 29

Ohio (7)	*Chillicothe:* Woodrow; *Columbus:* Paterson; *Lebanon:* Robinson; *Painesville:* Ladd; *Sidney:* Ferree; *West Alexandria:* Landis; *Xenia:* Hyslop.
Indiana (7)	*Bloomington:* Bryan; *Bridgeport:* Starbuck; *Johnson Co.:* Terman; *Montezuma:* Allport, G. W.; *Paoli:* Lindley; *Portland:* Stone; *Southport:* Dashiell.
Illinois (10)	*Benton:* Wever; *Bloomington:* Wolfe; *Champaign:* Dallenbach; *Chicago:* Thurstone, Wooley; *Cooksville:* Scott; *Decatur:* Hunter; *Morris:* Carr; *Paxton:* Buchner; *Sonora:* Bolton.
Michigan (2)	*Muskegon:* Kelley; *Union City:* Hurd.
Wisconsin (3)	*Alma:* Gesell; *La Crosse:* Starch; *Milwaukee:* Allport, F. H.

West North Central States, total 7

Minnesota (1)	*Red Wing:* Gates.
Iowa (3)	*Burlington:* Pillsbury; *Clinton:* Bentley; *Swan Lake:* Bingham.
Missouri (1)	*Nelson:* Johnson.
North Dakota (1)	*Silverleaf:* Miles.
Nebraska (1)	*De Witt:* Hollingworth.

South Atlantic States, total 7

Delaware (1)	*Wilmington:* Newbold.
Virginia (1)	*Clover:* Garrett.
West Virginia (1)	*Davis:* Lashley.
South Carolina (2)	*Columbia:* Baldwin; *Greenville:* Watson.
Georgia (1)	*Greensboro:* Brown.
Florida (1)	*Starke:* Pace.

East South Central States, total 1

Tennessee (1)	*Jonesboro:* May.

Mountain States, total 3

Wyoming (2)	*Laramie:* Anderson, Downey.
Utah (1)	*Huntsville:* Peterson.

Pacific States, total 3

California (3)	*Diamond Springs:* Dunlap; *Oakland:* Sanford, Stratton.

Foreign-born, total 23

India (2)	Fullerton, Judd.
Canada (4)	*Prince Edwards Island* (Freetown): Schurman; *Nova Scotia* (Pictou): Creighton; *Ontario* (Rockwood): Freeman; *Quebec* (Dewittville): MacDougall.
England (4)	*Norwich:* Gardiner; *Lancashire:* McDougall; *Lytham:* Pintner; *Chichester:* Titchener.
Germany (5)	Klüver; Lewin; Münsterberg; (*Berlin*): Koffka; (*Danzig*): Meyer, M.
Russia (3)	*Estonia* (Reval): Köhler; *Poland:* Sidis; (*Warsaw*): Jastrow.
Switzerland (2)	*Neuchatel:* Leuba; *Niederweningen:* Meyer, A.
Czechoslovakia (1)	*Prague:* Wertheimer.
Sweden (1)	*Morlunda:* Seashore.
Spain (1)	*Madrid:* Santayana.

In proportion to their population in 1870, Wyoming, New Hampshire, Massachusetts, Connecticut, and California each yielded more than twice the national average. Next in order were Indiana, Illinois, New Jersey, and Wisconsin, with yields of 1.6 to 1.2 times the national average. Ohio and Iowa yielded at the national average rate while Pennsylvania yielded at somewhat above, and New York somewhat below that rate.

Of the cities and towns which yielded two or more, Laramie, Wyoming, did best in proportion to population, followed by Burlington, Vermont, and Oakland, California. Of the larger cities, Boston did nearly three times as well as Philadelphia or Chicago and seven times as well as New York.

The 23 foreign-born starred psychologists came from several countries, 4 each from Canada and England, 5 from Germany, 3 from pre-war Russia, 2 from Switzerland, 2 from British India (children of American missionaries) and 1 each from Sweden, Spain, and Prague. In proportion to the number of immigrants in America in 1880, immigrants yielded future starred psychologists at a notably higher rate than the national average yield, Canada at three times that rate, England at twice, Germany at one-fifth more.

(b) Trends in yield. The foregoing analysis of place of birth is based on the totals without regard to time of birth. As the older starred psychologists were born more than a half century before the younger ones, an analysis of yields by decade of birth has been made. A table, not published here, reveals several significant trends.

Ten of the 23 foreign-born were born in the 1860's, 5 in the 1880's, 3 each in the 1850's and 1870's, and 2 in the 1890's. In other words, nearly a third of the older group were foreign-born, a fifth of the intermediate group, but only an eighth of the younger group.

Half of the group born in the 1860's were born in cities; about three-fourths of those born in the 1870's and the 1880's, and nearly seven-eighths of the youngest group were urban born. Large cities yielded about a seventh of both the older and intermediate groups, but only one of the youngest twenty. New York City has yielded none since 1871, Boston and Chicago none since 1887. Philadelphia has yielded one or two in each decade since the 1850's.

New England led in the number born during each decade until the 1880's when it was surpassed by the Middle Atlantic, the East North Central and West North Central regions each of which yielded 6 in contrast with New England's 2. For the 1890's, the East North Central region with 6 led in numbers, the Middle Atlantic yielded 3, the West North Central region and South Atlantic each 2 and the Mountain and South Central 1, the Pacific and New England States none. The Pacific States' contributions were born one each in the 1850's, 1860's, and 1870's. The only starred psychologist born after 1899 was born in 1902 in Illinois.

A national decline in the yield of subsequently starred psychologists in proportion to population between the 1860's and the 1890's is due to the fact that the number of starred men has been held constant while the population more than doubled. This means that stars are increasingly difficult to win. At the first starring in 1903 when 50 were starred, about one-fourth of all the fairly well-qualified men were starred, but at the 1937 starring, when only 13 were starred, less than one-tenth of the younger non-starred psychologists who had accomplished considerable amounts of good work were starred.

(c) Possible explanations of the trends. The regional variations in yield from decade to decade, disclosed by the analysis of birth by decades, are partly due to the statistical inadequacy of the data. For example, when starred psychologists were produced at the rate of only one per five million people, as in the 1890's, the fact that none was born in the Pacific States, which had in 1895 only about two million people, may not be statistically significant. Similarly as New England had only about five million people in 1895, the fact that no starred psychologist was born in this region in the 1890's may not indicate sterility: one or more may win a star at a future date. It is evident, however, that there has been a real decline in New England in proportion to population since the 1870's and in the Midwest since the 1880's.

Several explanations may be offered of such progressive changes in yield in proportion to population as have occurred with respect to native-born starred psychologists. Three suggestions are: (1) The increase in yield in proportion to population evident for the East North Central States to the 1860's and for the West North Central States to the 1870's may be an indication that in order to produce more than a few leaders, a certain amount of sociological, educational maturity in local 'culture' is necessary. If so, this would help to explain the fact that none of the starred psychologists was born in the more sparsely settled northern *halves of the populations* of Michigan, Wisconsin or Minnesota, or in mining camps in the West. (2) An analogous theory is that the subsequent decline is partly due to the 'softening' which often occurs in a locality after a certain amount of culture and wealth has been attained. (3) A third theory is that the change in yield reflects an actual change in the intellectual ability of the people of the area. Rapid improvement in average intellectual ability is chiefly due to the migration hence of many superior people. Correspondingly, a subsequent regional decline in ability is due partly to migration of the superior people from the area, and partly to the greater decline in the birthrate of the superior than of the inferior levels of the population. Certain it is that hundreds of thousands of exceptionally alert Yankees moved from New England to the Midwest in the 1840's, 1850's, and 1860's. Cattell from a special questionary found, and much other evidence indicates, that a large majority of the leaders of the Midwest were of at least half Yankee descent. Likewise, a larger share of the immigrants from the 1840's-1860's were superior intellectually than has been true for later decades when immigration was easier, and manual laborers vastly predominated. Moreover, most of the earlier immigration came from northwestern Europe and Canada but an increasing majority of the later immigrants came from other parts of Europe, from which regions relatively few American leaders have come. The great growth of cities, with their large number of manual laborers in proportion to population, have likewise often depressed the intellectual level.

Another factor which perhaps helps to explain the variation in yield from time to time, as well as from place to place, is in the local demand. There is a rough correlation between use and yield (see Table VI). Local men of an age to fill new positions may be given the opportunity, and may ultimately win the national recognition.

Cattell has expressed the opinion that the most important factor is opportunity; that when good opportunities are available, men qualified to use them arise. This optimistic theory is not very well supported by the present body of data. For example, New England, New York City, and California employ a large share of the younger starred psychologists (born after 1889), but have yielded none.

The influence of leadership helps greatly to explain the distribution of place of training of subsequent leaders, and to some extent, place of birth, as relatively few have gone far from their birthplaces for their early training. For example, Wyoming's high record in proportion to population is partly due to the late Miss Downey's presence and influence at the University of Wyoming. Likewise Indiana's better yield as compared with Michigan (7 vs. 2) is partly attributable to David Starr Jordan, highly stimulating teacher of 3 and former employer (as president of Indiana University and Stanford) of 4 of Indiana's 7.

Hence, the explanation of the unequal distribution of the yield of these notables is not simple nor clear, despite the great social desirability of an understanding of what conditions are conducive to the yield of leaders. Nevertheless, such data as these help to illuminate the problem and bring its solution nearer.

Collegiate training. (a) Undergraduate education. For their undergraduate education, the 102 starred psychologists who graduated from American colleges are alumni of 54 colleges. Thirteen institutions graduated 3 or more, a total of 54: 5 each from Amherst, California, Harvard, Indiana, and Pennsylvania; 4 each from Chicago, Michigan, Nebraska, Princeton, and Wesleyan; 3 each from Columbia, Cornell, and Vassar. Seven institutions graduated 2 each (Haverford, Illinois, Ohio, Smith, Vermont, Williams, and Wyoming) and 34 graduated one. (See List 3.)

As to type of American college from which the starred psychologists graduated (bachelor's degree), 43 were from small colleges, 52 from endowed universities, of whom 35 were from state universities. The relative importance of the small college was greatest as to the older men—three times as many born before 1860 graduated from small colleges as from universities. For those born in the 1860's, 1870's, 1890's, colleges and universities about tied in the number graduated; for those born in the 1880's, the universities trained twice as many as did the small colleges—chiefly because of the large number graduated from state universities. State universities graduated a half of those born in the 1850's, about a third of those born in the 1860's, 1870's and 1890's, but nearly a half of those born in the 1880's.

Foreign universities gave collegiate training to one-fifth of those born in the 1860's and 1880's, but to only about one out of twelve of those born in the 1870's or since 1887.

Even those starred psychologists who were born in the 1840's were college graduates and also had some graduate work.

Considerably more than a half of the subsequently starred psychologists had their college work in an institution which had at the time a distinguished psychologist professor.

An analysis by decades reveals that Princeton's four alumni were born in the 1860's, Harvard's five include one born in the 1840's, two in the 1860's, one in the 1870's and two in the 1890's; Columbia's three include one in the 1850's, two in the 1880's; Indiana's five include three born in the 1860's, one in the 1870's and one in the 1890's.

(b) Graduate work. Table II shows how many starred psychologists had graduate work at each of the more important universities. The leaders are: Harvard 21, Columbia 19, Chicago 12, Clark 10, Hopkins 9, Cornell 8 (33 had some graduate work in Germany).

(c) Doctoral training. List 4 and Tables II and III present data regarding the doctorates of starred psychologists. They reveal that Columbia and Harvard have conferred the doctorate on 15, Chicago on 11, Clark on 9, and Cornell and Hopkins on 8. German universities conferred the doctorate on 20.

When classified as to time, it is seen that for the older starred psychologists, German universities, Hopkins, Clark and Harvard were predominant; for those born in the 1870's and 1880's, Chicago and Cornell were followed by Columbia and Harvard; for those born since 1889, Harvard has led, followed by Columbia.

A third of the 12 born in the 1850's and two of the 33 born in the 1860's obtained no doctorate, and only one born since 1871 has been starred without a doctorate.

LIST 3. INSTITUTIONS CONFERRING BACCALAUREATE DEGREE

Conferred upon three or more:

Amherst: Delabarre, Gardiner, Jones, Pierce, Woodworth; *California:* Brown, Dunlap, Sanford, Stratton, Strong, E. K.; *Chicago:* May, Peterson, Wooley, Yerkes; *Columbia:* Goodenough, Marshall, Wells; *Cornell:* Boring, Ogden, Thurstone; *Harvard:* Allport, F. N., Allport, G. W., Holt, Santayana, Sidis; *Indiana:* Bryan, Lindley, Starbuck, Stone, Terman; *Michigan:* Angell, J., Bolton, Hull, Hurd, Woodrow; *Nebraska:* Bentley, Hollingworth, Pillsbury, Wolfe; *Pennsylvania:* Fernberger, Fullerton, Jastrow, Newbold, Witmer; *Princeton:* Armstrong, Baldwin, M., Farrand, Warren; *Vassar:* Ladd-Franklin, Martin, Washburn; *Wesleyan:* Dearborn, Freeman, Judd, Thorndike.

Conferred upon two:

Haverford: Goddard, Langfeld; *Illinois:* Dallenbach, Kelley; *Ohio State:* Landis, Paterson; *Smith:* Calkins, Howes; *Vermont:* Angell, F., Dewey; *Williams:* Dodge, Hall; *Wyoming:* Anderson, Downey.

Conferred upon one:

Beloit: Bingham; *Brown:* Whipple; *Bucknell:* Poffenberger; *Cincinnati:* Robinson; *Colorado:* Carr; *Dalhousie:* Creighton; *Earlham:* Miles; *Evansville:* Dashiell; *Gustavus Adolphus:* Seashore; *Illinois College:* Wever; *Iowa:* Patrick; *Lafayette:* Cattell; *Leander Clark:* Buchner; *Maryville:* May; *Massachusetts Technology:* Tolman; *Missouri Valley:* Johnson; *Morningside:* Starch; *New York College:* Scripture; *Northwestern:* Scott; *Ohio Wesleyan:* Ferree; *Pacific:* Miles; *Richmond:* Garrett; *Rochester:* Strong; *St. Charles:* Pace; *Swarthmore:* Baldwin, B.; *Texas:* Hunter; *Tufts:* Carmichael; *Valparaiso:* Stone; *Westminster:* McGeoch; *Western Reserve:* Ladd; *West Virginia:* Lashley; *Wisconsin:* Gesell; *Wooster:* Hyslop; *Worcester:* Nichols.

Conferred in Europe:

Klüver, Koffka, Köhler, Leuba, Lewin, MacDougall, McDougall, Meyer, A., Meyer, M., Münsterberg, Pintner, Schurman, Titchener, Wertheimer.

List 4. Universities Conferring the Doctorate

(Numerals indicate edition in which first starred.)

Conferred on eight or more:

Chicago: Bingham 3, Carr 3, Downey 4, Hunter 3, McGeoch 6, Peterson 4, Robinson 5, Thurstone 4, Watson 2, Wooley 3, Yerkes 2. *Clark:* Bolton 1, Bryan 1, Gesell 4, Goddard 3, Leuba 1, Lindley 1, Nichols 1, Starbuck 2, Terman 3. *Columbia:* Brown 4, Dashiell 5, Dearborn 3, Garrett 6, Gates 4, Hollingworth 3, Jones 6, Kelley 4, May 5, Poffenberger 4, Strong, E. 3, Thorndike 1, Wells 2, Woodrow 6, Woodworth 1. *Cornell:* Bentley 2, Boring 3, Creighton 1, Dallenbach 5, Ferree 3, Pillsbury 1, Washburn 1, Whipple 2. *Harvard:* Allport, F. 4, Allport, G. 6, Anderson 5, Baldwin 3, Carmichael 6, Dunlap 3, Hall 1, Holt 2, James 1, MacDougall 1, Pierce 1, Santayana 1, Sidis 2, Tolman 5, Wever 6. *Hopkins:* Dewey 1, Hyslop 1, Jastrow 1, Johnson 5, Lashley 4, Patrick 1, Sanford 1, Warren 1.

Conferred on three or less:

Iowa: Miles 4, Starch 3. *Minnesota:* Landis 6, Stone 5. *Pennsylvania:* Fernberger 5, Newbold 1. *Stanford:* Goodenough 6, Klüver 6. *Yale:* Buchner 1, Freeman 5, Seashore 1.

Conferred in Europe:

Angell, F. 1, Cattell 1, Delabarre 1, Dodge 1, Judd 1, Koffka 5, Köhler 6, Langfeld 3, Lewin 6, Meyer, M. 1, Münsterberg 1, Ogden 3, Pace 2, Pintner 3, Schurman 1, Scott 3, Scripture 1, Stratton 1, Titchener 1, Wertheimer 6, Witmer 1, Wolfe 1.

Endowed universities conferred the doctorate upon 17 times as many of those born in the 1860's or 1870's as did the state universities, upon 5 times as many of those born in the 1880's, upon 7 times as many for those born since 1889. Half of Hopkins' eight starred doctors were born in the 1850's and a fourth in the 1860's. Six of Clark's 9 were born in the 1860's and one in the 1850's. (Four of the 9 were

Hoosiers and graduates from Indiana University.) Columbia's starred doctors are mostly fairly young; half were born in the 1880's and one-fourth in the 1890's. Harvard has had two periods of relatively large output: first, men born in the 1860's and early 1870's who came under the influence of William James; and secondly, men born since 1889—notable is the fact that Harvard conferred the doctorate upon 7 of the 17 born since 1889 who have won stars. Columbia with 4 stands next. Cornell conferred doctorates upon 5 born in the 1870's, 1 in the 1860's, 2 in the 1880's; Chicago on 5 born in the 1870's, 3 in the 1880's, and 1 in the 1890's.

TABLE II

INSTITUTIONS AT WHICH STARRED PSYCHOLOGISTS WERE TRAINED AND AT WHICH THEY ARE OR WERE EMPLOYED

Institution	Trained			Employed		
	Bacca.	Grad. stud.	Ph.D.	1938	retired	dead
California	5	3	0	4	1	0
Chicago	4	12	11	4	1	0
Clark	0	10	9	0	0	2
Columbia	3	19	15	7	3	2
Cornell	3	8	8	3	3	1
Harvard	5	21	15	5	1	3
Hopkins	0	9	8	2	0	3
Illinois	2	2	0	1	0	0
Indiana	5	4	0	0	1	0
Iowa	1	3	2	2	1	1
Michigan	5	2	0	1	0	0
Minnesota	0	2	2	3	0	0
Nebraska	4	0	0	0	0	1
Pennsylvania	5	5	2	1	1	1
Princeton	4	3	1	2	1	1
Ohio	2	1	0	1	0	0
Stanford	0	2	2	3	2	0
Smith	2	1	0	1	1	2
Vassar	3	1	0	0	1	0
Wesleyan	4	1	0	1	0	1
Wisconsin	1	1	1	0	1	0
Wyoming	2	1	0	0	0	1
Yale	0	4	3	5	2	1
Europe	14	33	22			

College graduates only: Amherst 5, Haverford 2, Vermont 2.

There has been a progressive decline in the percentages of the doctorates received in Germany. For those born in the 1850's, German universities conferred nearly one-half of the doctorates; for those born in the 1860's, just a third, for those of the 1870's, just a fourth, for those of the 1880's a sixth, and for those born since 1887, only one out of the 19, and he was born and trained in Germany.

For their doctorates, most subsequently starred psychologists went to a university with an outstanding leader. G. Stanley Hall conferred the doctorate upon the largest number of subsequently distinguished psychologists—6 at Hopkins and 9 at Clark. The fact that during his last 13 years at Clark (1907-1920), while he was 61-74 yr. old, none were graduated presumably illustrates the serious handicap of increasing age with the consequently increasing gap between professor and student.

During a dozen years while he was young, Hall had graduated nearly a dozen men who subsequently became distinguished psychologists.

Employment. List 5 presents the place of employment of most of the starred psychologists employed in 1938, together with the connections of those retired, and for those dead, and place of employment in their later years. This list is summarized in Table IV, which makes conspicuous the leading universities in this respect; namely, Columbia, Harvard, California, Chicago, and Yale. The rank of these leaders is not changed if only the men starred in 1921-1937 are counted, nor are important changes called for if the retired men are also counted. Of the psychologists starred in 1932 or 1937, Minnesota, however, has the most (3), followed by California and Yale, with 2 each.

TABLE III

UNIVERSITY AT WHICH DOCTORATE WAS OBTAINED AND DATE FIRST STARRED

University	Date starred						1921–1937
	1903	1909	1921	1927	1932	1937	
Chicago	0	2	4	3	1	1	9
Clark	5	1	2	1	0	0	3
Columbia	2	1	3	4	2	3	12
Cornell	3	2	2	0	1	0	3
Harvard	5	2	2	1	2	3	8
Hopkins	6	0	0	1	1	0	2
Iowa	0	0	1	1	0	0	2
Minnesota	0	0	0	0	1	1	2
Pennsylvania	1	0	0	0	1	0	1
Yale	2	0	0	0	1	0	1
Europe	13	1	4	0	1	3	8

As to trends in employment, endowed universities have employed more than a half of each age-group; of those born before 1860 or after 1889 the ratio is almost three-fourths. State universities employed nearly one-fourth of each age-group since the 1850's, except during the 1870's when they employed one-sixth. Endowed colleges and commercial work have employed almost the same numbers, from one to three of each decade-age-group, except that none of the youngest group is in commercial work.

Age at starring. List 1 names the psychologists starred in every edition. Table V classifies by ages at starring the psychologists honored at each of the starrings and also by decade of birth. It reveals that the median age at starring has risen from about 40 yr. in 1903 to about 44 yr. for the last three starrings. Since 1921, only one has been starred while under 37 yr.; he was 35.

The lower part of Table V shows the number born in each of the four chief decades of birth and their age at starring. Those born in the 1860's or 1870's were starred at a median age of 39 yr. while those born in the 1880's and 1890's were starred at median ages of 44 and 40 yr. Only two have been starred while under 31 yr.; one of these was Thorndike, the other was Wells.

The eight women who have won stars in psychology were starred at 30, 37, 40, 47, 51, 52, 56, and 58 yr. Thus their median age was several years above that for the men. Three were starred in 1903, 2 in 1909, 1 each in 1921, 1927 and 1937.

TABLE IV

Place of Employment in 1938 and Date First Starred

University	Date starred						1921–1937	Retired
	1903	1909	1921	1927	1932	1937		
California	0	0	1	1	1	1	4	1
Chicago	0	0	2	1	0	1	4	1
Columbia	2	0	2	2	0	1½	5½	3
Cornell	0	1	1	0	1	0	2	2
Harvard	0	0	2	2	0	1	5	1
Hopkins	1	0	1	0	0	0	1	0
Iowa	1	0	0	0	0	1	1	1
Minnesota	0	0	0	0	2	1	3	0
Princeton	0	0	1	0	0	1	2	1
Stanford	0	0	2	0	1	0	3	2
Yale	0	1	0	2	2	0	4	2

List 5. Place of Employment in 1938 of Starred Psychologists

(Numerals indicate edition in which first starred.)

Institutions employing three or more:

California: Brown 4, Dunlap 3, Jones 6, Tolman 5; retired, Stratton. *Chicago:* Carr 3, Freeman 3, Klüver 6, Thurstone 4; retired, Judd. *Columbia:* Garrett 6, Gates 4, Hollingworth 3, Landis (½) 6, Pintner 3, Poffenberger 4, Thorndike 1, Woodworth 1; retired, Cattell, Dewey, Strong, C.; dead, Ladd-Franklin, Fullerton. *Cornell:* Bentley 2, Dallenbach 5, Ogden 3; retired Farrand, Schurman; dead, Titchener. *Harvard:* Allport, G. 6, Boring 3, Dearborn 3, Kelley 4, Lashley 4; retired, Santayana; dead, James, Münsterberg. *Hopkins:* Ferree 3, Meyer, A. 1; dead, Baldwin, M., Buchner, Hurd. *Iowa:* Lewin 6; retired, Seashore, Patrich; dead, Baldwin, B. *Minnesota:* Anderson 5, Goodenough 6, Paterson 5. *Pennsylvania:* Fernberger 5; retired, Witmer; dead, Newbold. *Princeton:* Langfeld 3, Wever 6; retired, Holt; dead, Warren. *Stanford:* Stone 5, Strong, E. K. 3, Terman 3; retired, Angell, F., Martin. *Smith:* Koffka 5; retired, Howes; dead, Gardner, Pierce. *Yale:* Gesell 4, Hull 5, May 5, Miles 4, Yerkes 2; retired, Angell, J., Dodge; dead, Robinson.

Institutions employing one:

Brown: Hunter 3; *Illinois:* Woodrow 6; *North Carolina:* Dashiell 5; *Northwestern:* Scott 3; *Ohio:* Goddard 3; *Syracuse:* Allport, F. 4; *Swarthmore:* Köhler, 6; *Tufts:* Carmichael 6; *Wesleyan:* McGeoch 6.

In commercial work:

Starch 3, Watson 2, Whipple 2.

In governmental work:

Johnston 5, Landis (½) 6, Wells 2.

Summary. Table VI affords a summary of the contributions of the different regions and states to the list of starred psychologists. It reveals a rough correspondence between the number born, trained, and employed. Those regions and states which have employed relatively few have, in general, trained relatively few, and have been the birthplaces of few. The three New England states which employ none yielded, however, seven; and several other states have yielded several times as many as they have employed, *e.g.* Indiana and Ohio. California, Connecticut, and Minnesota have conversely employed three times as many as they have yielded.

Table II reveals that several institutions which gave undergraduate training to relatively many who were subsequently starred, conferred the doctorate on none. Examples are California, Indiana, Michigan, Nebraska, and Vassar. Conversely, some universities gave doctoral training to several, but undergraduate training to none. Examples are Clark, Hopkins, and Yale. Sizable discrepancies between the number of undergraduate and doctoral alumni occur also as to Columbia, Harvard, and Chicago.

TABLE V

Age When Starred

Date starred	1903	1909	1921	1927	1932	1937
Total number	50	12	18	12	13	13
Median age	40	39	42	44	45	43
% between 35–45 yr.	58	44	57	58	61	61
% under 35 yr.	18	27	11	0	0	0
% over 50 yr.	14	17	11	17	8	23
Number above 55 yr.	5	2	0	1	0	1
Maximum age	68	66	55	56	52	57
Next oldest	61	58	54	52	48	54
Number below 40 yr.	23	6	7	2	2	2
Minimum age	29	25	32	37	39	35
Next youngest	30	31	33	37	39	39

Date born (in decade)	1860's	1870's	1880's	1890's
Total number	33	24	26	16
% between 35–45 yr.	85	34	54	94
% under 35 yr.	9	37	13	0
% over 50 yr.	6	12	16	0
Number above 55 yr.	0	1	1	0
Maximum age	55	56	57	47
Next oldest	52	54	54	43
Number below 40 yr.	17	13	6	6
Minimum age	34	29	25	37
Next youngest	34	30	32	39
Median age	39	39	44	40

(One was born in 1902, starred at 35.)

Slight correspondence between the number given the doctorate and the number employed in 1938 exist for Clark, Hopkins, Harvard, and Chicago. A fairly close ratio prevails as to Iowa, Minnesota, Stanford, and Yale.

Conclusions. The distribution of the places of birth, training and employment of starred psychologists supports several tentative conclusions drawn on the basis of earlier studies of the distribution of notables.[2] Indeed, since the validity of con-

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[2] Cf. S. S. Visher, A study of the type of birthplace and of the occupation of fathers of subjects of sketches in *Who's Who in America*, *Amer. J. Sociol.*, 30, 1925, 551-557; Distribution of women sketched in *Who's Who in America*, *Scient. Mo.*, 15, 1922, 443-447; Contrasts among Indiana counties in their yield of prominent persons, *Proc. Indiana Acad. Sci.*, 38, 1929, 217-224; Ecology of American notables, *Human Biol.*, 1, 1929, 544-554; The comparative rank of American States, *Amer. J. Sociol.*,

TABLE VI

CONTRIBUTIONS MADE BY THE DIFFERENT REGIONS TO THE LIST OF STARRED PSYCHOLOGISTS
(Number born, trained, and employed.)

Region or State	Born	Trained			Employed	
		Bacca.	Grad. stud.	Ph.D.	1938	ret. or dead
NEW ENGLAND	23	24	37	27	16	15
Maine	2	0	0	0	0	1
New Hampshire	3	0	0	0	0	0
Vermont	2	2	0	0	0	0
Massachusetts	12	17	31	24	9	9
Rhode Island	1	1	0	0	1	1
Connecticut	3	4	5	3	6	4
MIDDLE ATLANTIC	22	26	37	26	20	19
New York	9	11	28	23	15	12
New Jersey	3	4	3	1	3	3
Pennsylvania	10	11	6	2	2	4
EAST NORTH CENTRAL	29	27	21	22	8	4
Ohio	7	5	1	0	1	1
Indiana	7	7	4	0	0	1
Illinois	10	8	13	11	6	1
Michigan	2	5	2	0	1	0
Wisconsin	3	2	1	1	0	1
WEST NORTH CENTRAL	7	8	5	4	6	2
Minnesota	1	1	2	2	3	0
Iowa	3	2	3	2	2	2
Missouri	1	1	0	0	0	0
Nebraska	1	4	0	0	0	1
SOUTH ATLANTIC	7	4	9	8	4	3
Maryland	0	1	9	8	2	1
Dist. of Columbia	0	0	0	0	1	1
Virginia	1	1	0	0	0	0
West Virginia	1	1	0	0	0	0
South Carolina	2	1	0	0	0	0
Florida	1	0	0	0	1	0
SOUTH CENTRAL	1	1	0	0	0	1
Tennessee	1	0	0	0	0	1
MOUNTAIN	3	3	1	0	0	1
Wyoming	2	2	1	0	0	1
PACIFIC	3	7	3	2	7	3
California	3	6	3	0	7	3
CANADA	4	1	0	0	0	0
EUROPE	17	14	33	22	0	0

One each born in Delaware, Georgia, North Dakota, and Utah.
One employed in Kansas, North Carolina.
One college graduate from Colorado, Oregon, Texas.

24, 1931, 735-757; Social geography, *Social Forces,* 10, 1932, 351-355; Are mineral resources socially detrimental? *Ibid.,* 11, 1932, 51-55; Geography of American notables, *Indiana Univ. Stud.,* 1928 (no. 79), 1-138; Where our notables came from, *Scient. Mo.,* 45, 1937, 172-177; Distribution of the younger starred scientists, especially those in the physical sciences, *Amer. J. Sci.,* 37, 1939, 48-65; The education of the younger starred scientists, *J. Higher Educ.,* 10, 1939, 124-132; Distribution of geologists starred 1903-1937, *Bull. Geol. Soc. Amer.,* (in press).

clusions based upon statistical evidence is much strengthened by correspondence of separate bodies of data, the present study has a significance considerably greater than it would have if it were the only study of its kind. Conclusions which this study alone would not justify, but supports, have some propriety here.

In brief, it appears that most leaders (including distinguished psychologists) come from regions possessing at least a fairly favorable, stimulating climate, a moderate degree of cultural advancement, and many people who are alert, ambitious and persevering. The significance of the quality of the population appears to be so great that effective steps are justified to increase the birthrate of the more capable people. As desirable as relief, medical care, pensions for the aged, etc., may be for the sub-average and average people it seems even more socially desirable to increase the percentage of superior people. The help of the National Youth Administration towards the education of the superior young people is a step in the right direction.

Odin, Cattell, and others who maintain that encouragement ("opportunity") is of prime importance are correct provided unused high ability is abundant. Havelock Ellis and many more recent students of the subject believe, however, that, because of the relative facility with which people of exceptional ability have recently migrated to better opportunities, it is no longer safe to assume that all that is needed is to offer additional opportunities in order to have qualified people promptly appear on the scene to make the most of them. Even if this is still largely so, it will not continue to be true if the dysgenic birthrate long continues. The predominant share of the city in the yield of the younger distinguished American leaders in practically all fields, including agriculture, is ominous when it is realized that almost no city districts but the slums have a birthrate nearly adequate for maintenance.

The great importance of highly stimulating college teachers is so clearly indicated by many data that it appears obvious that such leaders should be widely distributed. Each state university (and as many non-state institutions as possible) should have on their faculty one or more highly enthusiastic research-worker-teacher in each of several great fields of work so as to stimulate various types of young people—some can become enthusiastic over geology but not over psychology!

The evidence indicates that for the doctorate, outstanding leadership and the best of advanced training is highly desirable. Since these can be had in few places, it seems socially desirable that promising students be encouraged to attend one of the few chief centers of the time. In other words, although a widespread distribution of enthusiastic, highly capable research-worker-teachers is highly desirable, it does not seem advantageous that the doctorate be given in numerous institutions. Instead, after receiving the baccalaureate degree and perhaps the master's degree, the promising student should be encouraged and facilitated to attend one of the few best-equipped universities in the land in his subject.

The correlation between the distributions of employment and of birthplaces suggests forcefully that where superior people form a considerable and well-respected fraction of the population not only are relatively many future leaders born, but current leaders are appreciated and employed. Thus, failure to be interested in, and to actively desire, distinguished scholars indicates a serious lack on the part of administrators, trustees, and other influential citizenry.

Indiana University STEPHEN S. VISHER

A PARADOX IN THE USE OF THE NORMAL PROBABILITY CURVE

The fundamental handicap to scientific and social progress lies in a paradox inherent in man's habits.[1] In order to observe effectively, we need principles and techniques to guide our seeing, but, if these are uncritically accepted, they set up resistance to seeing what our habits have prepared us not to see. The paradox is that we are served by our rational formulations and scientific techniques and by the same acts are bound to irrational and non-adaptive principles and techniques that these processes have developed. The aim of this paper is to question subservience to the normal distribution in the quantitative measurement of mental and social traits, and to stimulate the development of statistical techniques applicable to non-normal distributions.

The binding effect of adherence to normal distributions and of techniques which assume its accuracy seems to be attested by our common faith in its validity and by the statistical habits of the profession. Garrett voices the common faith in these words: "Measurements of natural phenomena, as well as measurement of social and mental traits, tend to be distributed symmetrically about their means in proportions which approximate the normal probability distributions."[2] His book is a record of common faith in this generalization, for there is a notable absence of techniques dealing with asymmetrical distributions and *a priori* theories of probability.

It is difficult to reveal a scientist's bias or partiality, but an unbiased or an absolutely impartial scientist rarely, if ever, exists. It may be assumed, however, that bias operates within a limited range and that it probably operates unconsciously, for most scientists display objective measurements, adhere to mathematical and scientific procedures, and assume impersonal attitudes. Bias is, however, to be found in the selection of problems. A preference exists for investigating traits that are unfavorable to predominate loadings, either because of the multiplicity and diverseness of the variables or because of their lack of specificity. Examples of these are found where there is a leaning toward studying traits that are native, or that are general and stylistic. Bias is also indicated by attention to manipulations that influence the normality of distributions by a choice in length of test or step-interval that yields the best fit to the normal curve, and by a restriction of its application to narrow levels of attainment and to samplings that give the desired normality. It appears in the assignment of weights to test-items and, especially, in the use of techniques derived from the normal curve to scale the units. It appears in the general failure to measure and report degrees of goodness of fit to the normal curve.

A motive predisposing to the assumptions of the normal curve lies in the fact that, if the normal hypothesis is accepted, well defined techniques for obtaining quantitatively supported answers are readily available; but, if the assumption of non-normality is made, such techniques await development. For example, coefficients of correlation are available for answering questions of the validity and reliability of tests if normality is assumed, but no such methods exist for the non-normal distributions. If such techniques are applied to distributions which poorly fit the normal curve, the question may well be raised, whether the deficiency is in the methods or in the assumptions. This question needs to be considered, for partiality exists in places where it often is overlooked. Disillusionment regarding the formulations and impartiality of scientists may be very disconcerting, but the risk of that is potentially less dangerous than the failure to appreciate it.

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[1] This paradox is clearly set forth in the contrasting views of habit revealed in the works of James and Coe. James, W., The Principles of Psychology, 1907, Vol. I, 1-689, Vol. II, 1-704. Coe, G. A., The Motives of Men, 1928, 1-265.

[2] H. E. Garrett, *Statistics in Psychology and Education*, 1938, 106.

The question of the adaptiveness of the statistics of normal probability merits serious consideration. The guiding principles of the theory of probability as applied to mental and social measurement are of the *a posteriori* type, *i.e.* the theory can only be utilized with the aid of previously obtained observations of similar phenomena. In the matter of drawing inferences, this type represents a reverse process of the *a priori* theory of probability which rests upon deductive logic and three basic assumptions: (1) the possibilities must be known to be mutually exclusive; (2) they must be known to be equally likely to occur; and (3) the favorable possibilities of frequencies of the phenomena must be known. The normal probability distribution is a graphic representation of the three *a priori* knowns. There seems to be no clear cut reason for expecting that all psychological continuums should satisfy these three assumptions. On the contrary, the logic of the matter is against the principle of equal likelihood. It is implicit in the expression of true, unbiased, or unloaded dice, but why should the ordinary "run of the mill" dice be unbiased in fact as well as in hope? In psychological parlance, the principle of equal likelihood is expected in concepts of nativeness, common environment, and random sampling, but, in the run of the mill products, the fact of biasing or loading is not improbable. True there does exist a fairly large amount of accumulated evidence that obtained distributions approximate the normal symmetry. It seems unwarranted to conclude from this evidence that all psychological distributions must follow the normal pattern.

The assumption of equal likelihood is also quite incompatible with another generalization which is just as widely accepted; namely, that we can figuratively load the dice. We do believe that the efficiency of individuals can be modified by teaching specific skills, and learning may be considered a form of loading. We do believe that efficiency (in part, at least, the product of learning) can be modified by changing incentives, by social approvals and disapprovals, by administrative management, by differences in age, culture, racial stocks, conditions of health and immediate environment. This implies a faith in loading by varying learning, motive, and interest and by manipulating diverse conditions. We frequently find results are significantly modified by such conditions. It would seem difficult to reconcile the idea that loading occurs, with the generalization that we always expect distributions to be normal.

It would seem to be quite an assumption to infer that a curve superimposed on obtained data that is approximately normal after the fashion of *a posteriori* techniques, would also exactly coincide with one that utilized *a priori* theory. Using attitude data and three obtained curves, the writer obtained a satisfactory fit to the normal curve employing chi-square techniques and Fisher's P table when based upon *a posteriori* theory, and clear independence of the normal distribution when the deductive *a priori* probability-theory was made the standard of comparison. This emphasizes the desirability of recognizing that the two types of probability-theory supplement each other, and the need for using both in developing generalizations and drawing inferences.

The frequent non-adaptiveness of the normal probability-hypothesis is revealed by accumulated evidence and the correctives developed to meet its deficiencies. One type of corrective gives us a set of formulae for dealing with skewness or slight departures from symmetrical distributions. In the predictive field, another type of corrective is to certify the qualifications, breadth of instruction, and experience of those who administer and interpret test results. The expert can presumably take the necessary precautions and make the appropriate qualitative corrections for the inappropriateness of the tools 'and techniques and bring forth valid generalizations for the specific purpose desired. It would seem more desirable to develop the appropriate tools and connect the demand for expertness with efficiency in educing generalizations in such complex fields as counselling, personal selection, and clinical diagnosis. The need for more than mere correction is recognized in the field of social phenomena. Here the J-hypothesis is expected to hold for fairly homogeneous groupings influenced by social conforming pressures.[3] The pattern is a highly asymmetrical or non-modal form, resembling the reverse letter J. In spite of the asymmetrical recognition, the techniques and assumptions of normality are often employed in analyzing such data. The writer's critical attitude was enforced by an unpublished study of attitudes in which all of his 176 distributions were found to be completely independent of chance fluctuations to the normal curve. The technique employed was Pearson's chi-square test for goodness of fit and Fisher's "P" table. There are a few critics of quantitative measurement as practiced. Johnson argues that opinions and attitudes do not stand in a continuum.[4] Certainly, if we must define a continuum as it is customarily allied with the chance hypothesis, one must heartily agree. Johnson's conclusion is forthright, for it is in effect that, if quantitative methods are non-adaptive and do not measure, then use qualitative methods. This solution would be much like recommending that a mechanic cease using cutting tools because his tools are unsuited to his purpose. The saner program would be to develop other tools for cutting or for measuring the continuum.

While we have no felt competence for the development of useful techniques applicable to non-normal and asymmetrical distributions, some of our thinking may stimulate their development. The most hopeful line of attack seems to take its point of departure from the neglected *a priori*.probability-theory rather than *a posteriori* development. From the *a priori* normal curve as a point of departure, we should have considerable use for a measure of deviation that would give a numerical description indicative of the amount of loading present in an obtained distribution. Starting from the assumptions of mutual exclusiveness, equal likelihood, and the distribution common to the law of errors, we have considered the following method to have some meaning and usefulness. Superimpose the obtained distribution, over the *a priori* theoretical curve; compute or utilize tables to obtain two chi-square values, one for the obtained distribution from the *a priori* curve, and the other for that giving 1 chance in 100 that the fluctuations can be attributed to chance; then divide the first chi-square value by the second. The deviation value so obtained, if 1, would indicate bare independence of chance as defined by a single probability in 100 chances; values greater than 1, would give a deviation unit, whose distance is defined by a bare independence being equal to 1. Another measure that would seem desirable would be some indication of the degree of extreme "J-ness" in one direction. Starting again from the theoretical *a priori* probability distribution and assigning positive and negative values to the contrasting mutually exclusive directions, we calculated for the positive and negative ordinates the chi-square contributions which were greater than those of the theoretical chance distribution and obtained the algebraic sum; then divided this by the total chi-square value to obtain the percentage so contributed. In a U-type distribution, we obtained the percentages so contributed by both the positive and negative sums independently. When such percentages are given along with the size of the chi-square value, considerable information can be educed from these numerical descriptions. The problem of finding appropriate scaling units for asymmetrical curves in the favored probability units is desirable. We suggest letting the rater assign the probability-values with reference to *a priori* or deductive probabilities. For this, we have asked the rater to assign a total of 10 probabilities to a psychological continuum. For example, if a continuum is defined as extending from being extremely dominant socially to that of being extremely submissive, the 10 probabilities could be allocated in many possible combinations to that defined continuum. The 10 probabilities might be concentrated at a single point, if that point expressed the rater's behavior as consistent under all circumstances, social environments and conditions; or they might be spread over the entire reportable scale, if such would better indicate the spread and inconsistencies of his behavior. A single numerical value for any possible combination can be developed for a scale such as a seven-point double rating scale. We have utilized the chi-square percentage contributions greater than chance as described above, indicating contrasting directions by appropriate signs. While this device puts the task of probability-scaling on the rater, this seems to be an improvement over making normal curve assumptions where they are contrary to fact. It permits a greater flexibility in reporting behaviors and recognizes the fact of behavioral inconsistencies. In starting from the deductive probabilities, it also avoids subservience to the assumptions of normal distributions and *a posteriori* probability-theory. These measures are included here merely as suggestions rather than as developed or useful techniques. The development of useful and meaningful measures would seem most desirable to foster the advancement of investigations of attitudes, personality traits, and predictive problems in such fields as counseling, personnel selection, clinical dignosis, and perhaps in supra-liminal perceptive studies.

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[3] F. H. Allport, The J-curve hypothesis of conforming behavior, *J. Soc. Psychol.*, 5, 1934, 141-183; D. Katz and R. L. Schank, *Social Psychology*, 1938, 35-54.

[4] H. M. Johnson, Pseudo-mathematics in the mental and social sciences, this JOURNAL, 48, 1936, 342-351.

The paradox mentioned in the beginning deepens toward the ridiculous and the tragic when one discovers that those scientists who were most daring in throwing off the fetters of authority by developing quantitative measurement, laboratory controls, objectivity in observations, and academic freedom, have become such worshippers of the normal probability curve that they are bound to its authority and have unconsciously lost the freedom that they earlier obtained at such great effort and cost. It must be in the mingling of faith and distrust that freedom is won and maintained.

Lehigh University JAMES L. GRAHAM

THE EFFECT OF BENZEDRINE SULFATE (AMPHETAMINE SULFATE) ON SATIATION

In a recent paper, satiation was defined by Barmack as "a state of conflict between the tendency to continue and the tendency to get away from a situation which has become unpleasant, principally because of inadequate motivation resulting in inadequate physiological adjustments to it. Accordingly a state of boredom is initiated by *inadequate motivation* during the operation of a task set and results in a tendency to revert back to the *sleep level*."[1] In the attempt to substantiate this hypothesis, Barmack administered 10 mg. of Benzedrine Sulfate (amphetamine sulfate) and equated blank pills to 36 *Ss* in order to determine the effect on the rate and accuracy of adding 6-place numbers for 2 hr.[2] He found that the drug retarded a drop in the rate of addition, and also retarded the development of boredom, sleepiness, and fatigue. Since the accuracy of the performance was not affected, and since the differences in the rate of work were increased with the increased duration of the work period, Barmack believes that the action of Benzedrine (amphetamine) was on the inclination, rather than on the ability to perform repetitive work. It was concluded that the experimental facts substantiate the hypothetical definition of satiation. The findings were confirmed, in a later experiment,[3] with a dose of 15 mg. of Benzedrine (amphetamine) and 120 min. of activity on the Poffenberger pursuit-meter.

This note attempts to indicate some difficulties inherent in the Barmack definition of satiation, and in the experiment which apparently confirms the hypothesis.

When it is observed that an individual is highly motivated, then with continued activity becomes disinterested, and finally rejects the activity entirely, it is plausible to consider that the motivation has somehow disappeared; that in some way the tissue needs, states of agitation, or other conditions which are the basis of motivation (drive), have become appeased, and then saturated to the point of aversion. This is certainly true in certain forms of satiation which we consider primitive; where the satisfaction of a basic need is involved, and is well illustrated by the work of Katz on hunger and appetite.[4] Barmack adopts just this point, but errs when he generalizes this restricted view to all satiation. He does not consider satiation in the wider sense of a dynamic reaction in the behavioral field. He ignores the findings of Lewin and Karsten who showed that satiation may arise suddenly and leave suddenly, *i.e. S* may be satiated at one moment and not in the next.[5] This frequently occurs when the stimulus-field undergoes even a slight reorganization. Under these circumstances it does not seem probable that these motivational states could be resolved, or that a transformation from desire to aversion could be accomplished, in so short a period. In an operational sense, it is known that animals require a relatively long period—within a particular sphere of activity—to satiate a basic need

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[1] J. E. Barmack, The effect of benzedrine sulphate (benzyl methyl carbinamine) upon the report of boredom and other factors, *J. Psychol.*, 5, 1938, 125. (Italics ours.)

[2] *Ibid.*

[3] Unpublished experiment by Dr. Barmack. Personal letter to the writer.

[4] D. Katz, *Animals and Men,* 1937, 1-263.

[5] K. Lewin and A. Karsten, Untersuchungen zur Handlungs- und Affektpsychologie: V. Psychische Sättigung, *Psychol. Forsch.*, 10, 1928, 142-254.

(*e.g.* hunger) and if this satiation were to occur in an instant, it would probably be due to mediating factors and not to the physiological, organic, or vital activities which we think of as the basis of motivation. These mediating factors, which in part determine the continuation or rejection of the activity, seem to involve a higher level of behavior than the physiological, and are of great importance to a theory of satiation. By this we mean to indicate that satiation involves more than a reduction of motivation during the task. There seems to exist a confusion between the recognition of the subjective aspects of fatigue and that of satiation. It is fundamental that a one-to-one correspondence between these basic states and satiation does not seem to hold. Katz has made this the basis of his two component theory for he finds that hunger, and the satisfaction of that hunger, is to a surprisingly great extent independent of the physical state of the organism and largely dependent upon the external circumstances of the event.[6]

If we deny a strict correspondence between motivational states and satiation, then we cannot accept the hypothesis that satiation is a "tendency to revert back to the *sleep level* arising through a *loss of motivation*."[7] The evidence to support this contention seems insufficient—in addition to that presented—on the following grounds.

(1) The criterion of 'sleepiness' was *S*'s report on a 10-point scale, ranging from 1 and 2 (very sleepy), 3 (sleepy), 4, 5, 6 (indifferent), to 7 and 8 (wide awake), and 9 and 10 (very wide awake). We doubt whether any *S* has the ability to discriminate accurately over a 10-point scale. It was found in one of our experiments that responses to a 5-point scale were not sufficiently reliable to be the sole criterion of satiation. Barmack's data seem to indicate that the greatest difference between the two groups on the sleep-wake scale was no more than 1 point on a 10-point scale. The probable error of the scale is such that the small obtained differences might easily be overcome or reversed. A criterion of satiation should involve—in addition to a subjective report—non-resumption of activity, characteristic variability in performance, and the presence of "creative substitution," *e.g.* day-dreaming.

(2) As is well known, the requisite for sleep, or a sleep-like state, is the exclusion of sensory stimulation.[8] An abundance of stimuli in the field, as would be required to satiate an individual, would reduce the tendency to go to sleep.

In certain instances where *S* becomes adapted to identical repeated stimuli, he may restrict the field of attention (as in hypnosis), and where the initial tendency is towards a drowsy state, this may facilitate sleep. Satiation is, however, not restricted to repetitive stimuli—and heterogeneous stimulus-fields tend to a waking state. We have no evidence that those individuals who tend to sleep during repetitive activity, would not do likewise in activities upon which they do not become satiated.

(3) Postulating a 'sleep-level' adds nothing to the explanation of satiation beyond the already apparent fact of the conflict between staying at the task and its rejection; that variegated accompaniments of this conflict appear which may in certain instances be postural, attentive, psychical, visceral, and fantastic.

The pharmaceutical action of Benzedrine Sulfate (amphetamine sulfate) as a relief for exhaustion and for an increased capacity for work,[9] as a vaso-constrictor,[10] as a general activator of behavior,[11] has been demonstrated clinically and experimentally. It may be said in general to produce a restless hyperactive state with increased tensions and tendencies to activities. Skinner and Heron report that "the effect of either drug [Benzedrine or caffeine] is to cause the organism to release energy at a higher than normal rate whether in a state of inhibition or not. The surplus energy is used by the animal in doing what it is accustomed to doing in the situation.[12] It would seem, then, that the action of Benzedrine (or caffeine, ephedrine,[13] and even a stimulus which elicits an intense emotional response—*e.g.* fright) serves to increase the available energy and the extensity and intensity of the tension systems. It is thus possible for *S* to persist longer at the task because of the greater potentiality of these systems. Any drug or stimulus of sympatho-mimetic character thus performs this function to a greater or less degree. This, of course, does not mean that the previously discussed motivational states are necessarily increased under such stimulation, for it is easily shown that such stimuli do not actually increase the desire for activity. In addition, it may be that an increased "vitality" of these *Ss* also introduces a reorganization of the attitude toward the rapidly declining "demand value" of the task.

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[6] Katz, *op. cit.*, 159 ff.

[7] Barmack, *op. cit.*, 125. (Italics ours.)

[8] Katz, *op. cit.*, 34.

In his earlier studies,[14] Barmack seems to recognize the more than motivational character of satiation by recognizing a "feeling of boredom" and an "attitude of boredom," although the distinction is not strictly maintained throughout the paper. No evidence of more than the primitive aspects of satiation is apparent in the later work.

This note is in part directed to just this recognition of the confusion between the motivational and physiological aspects of satiation on the one hand, and the psychical on the other. It does not posit a strict dualism between motivation and satiation (for this is obviously incorrect), but attempts to indicate that all aspects of satiation cannot be explained on the grounds of basic motivational states, and tendencies to revert to a sleep level.

It is quite probable that Barmack would have found somewhat different results had the satiation activity been more intrinsic to every *S* than adding 6-place numbers.[15] His *Ss*, because of strong extrinsic incentives, were probably forced to exhaustion, which is not the same as satiation, and may even have interfered with satiation.

University of California ARTHUR BERMAN

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[9] L. Zieve, The effect of benzedrine on activity, *Psychol. Rec.,* 1, 1937, 393-396.

[10] S. A. Peoples and E. Guttman, Hypertension produced with benzedrine, *Lancet,* 1, 1936, 1107-1109.

[11] L. V. Searle and C. W. Brown, The effect of subcutaneous injections of benzedrine sulphate on the activity of white rats, *J. Exper. Psychol.,* 22, 1938, 480-490; W. J. McNamara, and R. E. Miller, The effect of benzedrine sulphate on mental work, *Psychol. Rec.,* 1, 1937, 78-84.

[12] B. F. Skinner and W. T. Heron, The effects of caffeine and benzedrine on conditioning, *Psychol. Rec.,* 1, 1937, 339-346, esp. 346.

[13] In an unpublished experiment, Barmack found similar effects with ephedrine hydrochloride—although less intense than for benzedrine sulphate.

[14] Barmack, Boredom and other factors in the physiology of mental effort, *Archiv. Psychol.,* 31, 1937, (no. 218), 1-83.

[15] I am indebted to F. Nowell Jones for this suggestion.

INDIVIDUAL PSYCHOLOGY AND PSYCHOLOGICAL VARIETIES

The first term in this title has a long tradition. Individual Psychology derives in part from discrepant items in reports (formerly called 'introspections') of the several observers or subjects serving in a given research. For many years it was common for experimental expositions to conclude with a section on 'individual differences.' Here were added to the generalized results a comparative survey of the several protocols. Another origin lay in measures of variability from central tendency. Again, the testing methods placed their emphasis from the first upon the description (usually in quantitative terms) of the individual's output and upon differences among individuals tested.

Gradually there came to be separated off from the 'general' studies a division of the subject which was concerned, as Binet and Henri explained in 1896,[1] with the determination of those characteristics of mental processes which may vary from individual to individual and which are presumably grouped or constellated in a given organism according to a certain pattern. In 1900 Stern brought together a large mass of this 'differential' material in an important little book which opened with the prophetic exclamation "Individualität, Problem des zwanzigsten Jahrhunderts."[2] Stern stressed the experimental method, as Binet had favored the methods of test and of the questionary, Cattell (closely following Francis Galton) the methods and aims of a gross mensural anthropology, and Kraepelin the Wundtian pattern as applied to the diagnosis of the pathological subject.

In this country Jastrow had assembled apparatus and carried on a testing bureau at the Columbian Exposition of 1893 and the American Psychological Association had appointed, as early as 1895, a committee (Cattell, Baldwin, Jastrow, Sanford and Witmer) to report on "the collection of mental and physical statistics." A series of tests of mental ability was later recommended to the American laboratories and discussed at length at the Ithaca meeting of 1897. The wide program in Individual Psychology which thereupon developed in the United States fused French, German and English views and methods. The subject was ripe for a wider exploitation when pressure to test the individual child was exerted from education and was exerted later from business, industry, and medicine to test the adult. As the pressure-groups were both strong and insistent upon speedy results, the experimental methods of the laboratory were rapidly mutilated or entirely discarded for the quicker and easier methods of test, a change which inevitably brought in its wake the application of statistical devices for refining and interpreting crude and raw data from the testing room.[3]

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[1] A. Binet and V. Henri, La psychologie individuelle, *Année psychol.*, 2, 1896, 411-465.

[2] L. W. Stern, *Ueber Psychologie der individuellen Differenzen* (*Ideen zu einer 'differentiellen Psychologie'*), 1900, 1-146. Upon establishing, as he avers, a new "Wissenszweig mit eigenen, genau zu formulierenden Aufgaben, Zielen und Schranken," Stern proceeds to expound the principles and the facts of this new discipline. Since his study includes differences between and among peoples, social states, races, genera, and so on, he will not regard it as a 'psychology of personality.' It is designed to include "alle psychischen Differenzierungsmöglichkeiten überhaupt" (p. 4), to include all sorts of differentiable varieties of a psychological nature. In his more compendious volume of 1911, *Die differentielle Psychologie in ihren methodischen Grundlagen,* Stern more clearly distinguishes between the study of varieties (*Variationslehre*) and the biographical depiction of the individual (*Psychographie*). Most of the book is devoted to the former, *i.e.* to the study of varieties.

It is noteworthy that, in spite of the tendency for the individual thing (whether measure, trait, organism or 'person') to disappear under the methods of variation, correlation and factoring, the important distinction drawn by Stern and others as between *the strictly biographical* (psychogram, profile, autobiography, analysand, and the like) and *the variational in kind* has been widely ignored—the term 'individual psychology' being illogically used to cover both. This fact is the more striking because the biographical methods themselves—as well as the others—are in very large measure taxonomic, using the individual only as a representative of class, genus, type or grade. Whether, under the present strong domination from practice, most psychologists would join other descriptive sciences in a formal declaration that the strictly individual and unique is never itself the proper object of inquiry in a scientific context is doubtful. Practical responsibility for cases, candidates, wards, and inmates tends to persuade us that these 'persons' offer primary material for study. Besides, ambiguous terms always have their convenient uses. The adjective 'individual' in our title may mean either a unique sample, this thing, quality, or attribute, the unanalyzed organism, the total organism, the organism with all aspects but one ignored, the representative of a kind, a 'type,' or that member of a group who is just now undergoing test and classification. Something of this kind lurks under the current popularity of all sorts of reference to *personality*. The trend of the times may possibly stand related to the threat of individuality from dictators and despotic states; but a more obvious reference is to a recent diversion from the social group to the socialized individual, especially the defective, the aberrant, and the pathological individual.

While all the sciences of living things deal inspectively and descriptively with the individual organism, there seems to be no reason why the psychologist should concern himself any more or less with the single organism *qua* individual than the physiologist or the anatomist does. In all the sciences of life the organism is either a *fair sample* or it is a *variant* of a certain degree or a certain kind within its class. The first context is that of general psychology; the second (the taxonomic) context implies that the organism is representative of some psychological variety or that it takes its place in a variational display. A strictly biographical or personal account ('individual' in a social sense) would seem to find its place in some such subject as clinical medicine, education, economics, or daily living, where it would receive its valuation according to the estimates of its extra-psychological setting. Such a clarification as is here intended is obviously needed not only in the confused and badly designated subject of 'individual' study, but also in social, educational, genetic, and other special psychologies.

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[3] For a discussion of the large part played by this individualizing tendency in the two Great Invasions of the subject during the years 1900-1925, see the writer's The nature and uses of experiment in psychology, this JOURNAL, 50, 1937, 452-469. The fact that the invaders commonly brought for solution a child, a case, a candidate for a position, an alleged medium, or a suspected criminal easily enlarged the belief that it was also a function of psychology to deal with and to resolve *the individual instance.*

THE FIFTH PRINCETON MEETING OF THE SOCIETY OF EXPERIMENTAL PSYCHOLOGISTS

The thirty-fifth annual meeting of the Society of Experimental Psychologists was held at Princeton University on Monday and Tuesday, April 3 and 4, 1939, under the chairmanship of Professor H. S. Langfeld. It was attended by 27 members from 19 institutions, as follows: Brown (Graham, Hunter, Schlosberg); Chicago (Klüver); Columbia (Poffenberger); Cornell (Dallenbach, Weld); Harvard (Beebe-Center, Boring); Montreal Neurological Institute (Jasper); New School Social Research (Wertheimer); New York Psychiatric Institute (Landis); Pennsylvania (Fernberger, Wendt); Princeton (Bray, Langfeld, Wever); Rochester (Culler); Rutgers (Pratt); Smith (Koffka); Swarthmore (Köhler); Tufts (Carmichael); Vassar (Lanier); Washington (Jacobsen); Wesleyan (McGeoch); Yale (Hull, Marquis). That is the widest representation and the largest attendance since the reorganization of the Society in 1929.

After a short business meeting Monday morning, during which Professor S. W. Fernberger of the University of Pennsylvania was elected Chairman of the Society for the coming year, the rest of the sessions were devoted to reports of work in progress or recently completed.

Monday evening, at a banquet given to the Society by Princeton University, Dean L. P. Eisenhart of the Princeton Graduate School extended the University's greetings and welcome. After a response by the Chairman of the Society, Professor Langfeld, the 1939 award of the Howard Crosby Warren medal for outstanding research in experimental psychology was made to Dr. Carlyle Jacobsen "for his description of the function of the frontal lobes of the brain."

The 1940 meeting of the Society is to be held at the University of Pennsylvania. The tentative dates are March 25 and 26.

K.M.D.

William McDougall: 1871–1938

Professor William McDougall died in Durham, North Carolina, on November 28, 1938, in his sixty-seventh year. For some months he had borne with fortitude the knowledge that a cancer would soon end his life.

Probably no other man had better preparation for the study of human nature than McDougall. Born in Lancashire, England, on June 22, 1871, the son of a chemical manufacturer, he was educated in a private school until at the age of fourteen he was sent for a year to the *Realgymnasium* at Weimar. He entered Owens College, Manchester, thereafter and studied for four years, in the latter part of this period working chiefly in biology and geology. In the fall of 1890, having won a scholarship at St. John's College, Cambridge, he began another four-year period, in the last two years of which he specialized in physiology, anatomy, and anthropology. Then followed a medical course at St. Thomas' Hospital, London, and a period spent with Haddon and Rivers on the Cambridge Expedition to Torres Straits and with Dr. Charles Hose among the headhunters of Borneo. In the Straits he helped Rivers investigate the sensory capacities of the natives. There followed still another year of preparation, which he spent a year at Göttingen attending the lectures of G. E. Müller on psychophysics and memory and working on vision.

In 1900 he was appointed Lecturer in Psychology at University College, London. During his tenure of this post he conducted experiments on vision at his country home in Surrey. In 1904, McDougall succeeded G. F. Stout as Wilde Reader in Mental Philosophy at Oxford. Here his work was carried out with difficulty, since Oxford has never given much recognition to psychology. The scientists took him for a philosopher, and the philosophers thought that he was trying to work in an impossible and empty science. He had a few advanced students (W. Brown, Cyril Burt, and H. B. English among others), and Professor Gotch allowed some of the rooms in his physiological laboratory to be used for research. The founder of the Wilde readership objected to experimentation and tried to have McDougall ousted, but in 1912 his position was strengthened by his election to the Royal Society and to a fellowship in Corpus Christi College. In 1913 he made his first trip to America, in the company of Dr. Osler of Oxford, to make an address at the opening of the Phipps Clinic in Baltimore. When the World War broke out, McDougall enlisted as a private in the French army and drove an ambulance. He later offered his services to the medical corps of the British army and was made a major. His work was with military cases of mental disorder and was recognized after the war by his election as president of the psychiatric section of the Royal Society of Medicine. He was called to Harvard University in 1920 and stayed there until he accepted the offer of a professorship at Duke University in 1927.

The scope of McDougall's writings and his intellectual interests is comparable only to that of Wundt and James. He employed almost all methods of psychological investigation, and he made contributions to almost all of the principal fields of the subject. Let us consider some of his dominant interests, which by their variety exhibit the richness of his mind.

Experimental and physiological psychology. Between 1901 and the World War, McDougall published about twenty papers, the most important of which dealt with the following topics: Thomas Young's theory of color vision, physiological factors of the attention process, sensations excited by a single momentary stimulation of the eye, variation of visual intensity with duration of stimulus, the relation between corresponding points of the two retinas, and learning and retention with respect to Bergson's distinction between habit memory and pure memory. His work shows a strong interest in inferring from sensory and perceptual phenomena the nature of processes in the brain. In 1903 he published his drainage theory of inhibition, which received favorable attention from Sherrington. Although this theory, like the idea that learning involves a decrease of synaptic resistance, belongs to that class of neurological theories for which there is considerable plausibility but little or no direct physiological evidence, its author, with characteristic tenacity, never surrendered it. As late as 1929 he claimed that all of Pavlov's phenomena of inhibition were in harmony with the drainage theory. In 1905 the small but extremely influential *Physiological Psychology* appeared, in which psychology was defined as "the positive science of the conduct of living creatures."

Hormic psychology and the theory of instincts. For a decade McDougall's *An Introduction to Social Psychology* (1908) was undoubtedly more successful and influential than any other psychological work. Although written for the general public, it was widely used for many years as a textbook. Largely a synthesis of the views of other writers, it was nevertheless a brilliant work of systematization. In this book McDougall sought to lay a firm foundation for the social sciences to replace the *ad hoc* psychologies previously concocted by many writers in this field. This firm foundation he believed to be the theory of instincts and their derivatives, the sentiments. In his *Outline of Psychology* (1923) he carried this theory farther and named it, after a suggestion of Percy Nunn, the hormic theory. This theory states that purposive striving is the most fundamental category of psychology, and that the springs of purposive activity are the instincts. The instincts are inborn dispositions, which cannot be defined by reference either to definite stimuli or to bodily movements, but only by the kinds of change in the situation which their activity brings about or, when introspection is possible, by the specific accompanying emotional quality. The ascription of a specific emotion to each of the human instincts is one of the most original but at the same time one of the most questionable points in McDougall's system. One of the arguments for the existence of human instincts upon which McDougall most strongly insisted was that from evolutionary continuity; this argument is especially prominent in the *Outline,* where the instincts of both men and animals are discussed in the same sections. His tendency to bring human and animal behavior so close together was somewhat checked in 1933 by his introduction of the term "propensities" in place of "human instincts," a verbal change which called attention to the difference between the relatively fixed behavior to animals and the very variable manifestations of human instinctive activity. These pronounced hereditarian views of McDougall may be illustrated by his idea that in gregarious animals and human beings certain instincts may be aroused, independently of any learning, by the sight or sound of the same instinctive activity on the part of another individual.

One of the central doctrines of McDougall's hormic psychology is that all behavior is purposive and bears six objective marks which distinguish it from mechanically produced movements. Reflexes are not to be confused with "behavior," for they do not show these marks of purposiveness; they are, like habits, devoid of any driving force and may serve, as mere "motor mechanisms," any instinct.

Purposive striving in McDougall's system is said to be accompanied by a unique experience, which cannot be reduced to sensations of muscular contraction. Perception and memory are held to be governed to a large extent by conation. All instinctive striving in men and animals involves some degree of foresight and insight however rudimentary. In *The Energies of Men* (1933) McDougall gave both these factors great prominence, especially in his discussion of animal learning—a section based partly upon animal experiments which he had made in collaboration with his son.

McDougall's theory of sentiments he took, with some modifications, from the work of G. F. Shand. He held that the sentiments are the functional units of mental life; each sentiment is an organization of all the instinctive and cognitive dispositions relating to a particular object. He called the theory of the sentiments "a systematic sketch of the structure of character and a theory of its development from native tendencies under the moulding process of social traditions."

Social psychology. McDougall's strictly social psychology began with *The Group Mind* (1920); his work of 1908 had been largely the groundwork in individual psychology which was to be applied to social phenomena in his later writings. His book of 1920 discussed the concept of the group mind (not to be confused with the rejected concept of group consciousness), the behavior of crowds and organized groups, and the national mind and character. *The Group Mind* was intended to form one part of a *magnum opus,* but McDougall never completed the project, since this part was so unfavorably received. Undoubtedly the book was handicapped by its title, which lent itself to misunderstanding. The verdict of critics was that this work was no more scientific than many others, not written by men of science, on the same subjects.

McDougall developed the theory of innate mental differences between races in a series of Lowell lectures, published under the title *Is America Safe for Democracy?* (1921). This book belongs to the period in which the results of mental tests, especially the army tests given to different racial and national groups, were uncritically accepted as revealers of "innate" intelligence. McDougall used these data as evidence of the innate inferiority of the negro race and for the hereditary origin of intellectual differences found among children from different social strata. He also attempted to show that there were certain important temperamental differences among the three races of Europe which were reflected in various social and cultural phenomena. Later in his autobiography (1930) he said that he did not know that the American people were sensitive about the racial question and that this book is largely responsible for a hostile attitude toward him on the part of the American press.

The mind-body problem. In *Body and Mind: A History and a Defence of Animism* (1911) and its sequel, *Modern Materialism and Emergent Evolution* (1929), McDougall defended the theories of psychophysical interaction and vitalism. Mechanical principles he held to be inadequate to explain purposive activity, organic evolution, morphogenesis, heredity, vital self-regulation, and memory. Furthermore, he thought that the correlation between brain processes and mental processes cannot be carried very far, in fact that there are no complete brain correlates for memory, the unity of consciousness, meanings, or intelligent purposive activity. In the later book he vigorously criticized the efforts of H. C. Warren and R. B. Perry to reduce purposive activity to mechanical activity or to obliterate the distinction between the two. The value of *Body and Mind* as a comprehensive summary of the psychophysical problem and of its sequel as a critique of emergent evolution will remain undiminished for many years to come.

Abnormal psychology. In the field of abnormal psychology McDougall wished to be a mediator between academic and medical psychology. He considered himself a member of a group led by Morton Prince and W. H. R. Rivers. Both of these men had been influenced by McDougall's writings; in fact, McDougall claimed to have converted Prince to hormic psychology. McDougall based his *Outline of Abnormal Psychology* (1926) partly on his military experience. The book is important for its systematic discussion of mental disorders, which made much use of the concepts of instinct, repression, conflict, and dissociation; for its treatment of hypnosis; and for its criticism of Freudian and Jungian psychology. In the last two chapters, McDougall yielded to his speculative tendencies and presented his "monadic theory" of personality.

His criticism of psychoanalysis in this book and *Psychoanalysis and Social Psychology* (1935) is probably the most thoroughgoing and competent that has been made up to the present time by either a medical or an academic psychologist. McDougall preferred not to attack the application of psychoanalysis to individual cases; he chose rather to point out its internal inconsistencies and to criticize its application to other fields, especially social psychology. He was queerly ambivalent toward Freud. After having written in 1920 that he found French writers on abnormal psychology, especially Janet, more valuable to him than those of the German and Austrian school, he said in 1926, "I believe that Professor Freud has done more for the advancement of psychology than any other student since Aristotle." Although he never receded from this opinion, it would be hard to find very much in Freudian psychology of which he approved except its hormic quality.

The Lamarckian experiment. McDougall's belief in the inheritance of acquired characters, which he had expressed in many of his works, led him to begin in 1920 what he has himself called "a fool's experiment" on the transmission of the effects of training in rats. The experiment has now gone on for at least 13 years and 37 generations. The descendants of the trained animals, which learned to escape from a tank of water, made decidedly fewer errors in the latter generations than a control group. Although there has been criticism, principally from Crew of Edinburgh, McDougall believed that all objections could be answered and that he had proved the Lamarckian principle.

Nor was McDougall's versatility limited by even these six fields. As early as 1896 he published a theory of muscular contraction. He contributed several papers to anthropological journals and collaborated with Dr. Hose in 1912 on *The Pagan Tribes of Borneo.* He wrote extensively on topics relating to ethics and world politics. Eugenics and psychic research he called his hobbies; in fact, psychic research became a major interest of his last years, when he actively supported Rhine's work on extra-sensory perception, accepting the results at their face value.

To the last McDougall was a vigorous and indeed an unexcelled polemist. His polemic abilities were a natural outgrowth of his championing a cluster of unpopular causes; he was necessarily belligerent in the face of opposition on so many fronts. Many years ago he decided to write his controversial articles with utter frankness, and he lived up to this policy. His enthusiasm sometimes produced clever phrases which antagonized some readers (he called behaviorism "a most misshapen and beggarly dwarf" and said that parallelism "survives merely as a historical curiosity still hugged by a few intellectual tortoises"), but his informal oral discussions were marked by mildness and even diffidence. Among his last polemic essays were a devastating criticism of Pareto and a series of articles on Gestalt psychology.

McDougall lived and thought in the tradition of British psychology after the decline of associationism. He has said that he was a disciple of no one but Stout and James. His indebtedness to Stout is perhaps not as well known in America as it should be. His system of psychology in its main features is an extension of Stout's psychology, free from certain doctrines which he considered errors, with greater emphasis on hormic forces, and in a less austere literary style. From James, McDougall got a belief in the pragmatic criterion of truth and a great deal of personal inspiration.

A feeling of isolation, due to the almost unanimous rejection by American psychologists of his hormic theory and especially of his views on human instincts, burdened his last years. He deplored the chaotic state of psychology and became skeptical of its present and future progress and of the value of his own life-work, saying in 1935, in spite of his manifest achievements, "Even now, after some forty-five years of sustained effort, I am not sure that I have made any progress, have learnt anything of human nature."[1]

The Rice Institute FRANK A. PATTIE, JR.

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[1] The autobiography of McDougall, in *A History of Psychology in Autobiography*, 1, 1930, 191-223, edited by Carl Murchison, which has been consulted in the preparation of this sketch, is candid and fascinating. It is accompanied by a photograph of McDougall in army uniform. A more recent picture appears in *Human Affairs*, edited by R. B. Cattell and others, 1937. Neither is a very good portrait of McDougall, who was an exceptionally handsome man.

Otto Klemm: 1884-1939

Otto Klemm was born in Leipzig on March 8, 1884, and died there on January 5, 1939. Almost all the important events of his life center about the University of his native city. From 1903 to 1905 he studied at Munich where Lipps turned him in the direction of psychology. Upon receiving his doctorate at Leipzig in 1906, he was offered an assistantship by Wundt which he held until he wrote his *Habilitationsschrift* and became *Privatdozent* in 1909. In 1923 Klemm became *planmässiger ausserordentlicher* Professor of Applied Psychology and Experimental Pedagogy, a post which he held at the time of his death.

In the *vita* prepared for the third volume of the *History of Psychology in Autobiography,* Klemm tells how he became a psychologist.[1] During early adolescence he conceived the "fantastic" notion that trees and buildings always came into being just as he passed them. His first scientific interest appears to have been astronomy, for at the early age of fifteen years he succeeded in publishing a paper on sunspots in a technical journal; the "need to philosophize," however, led him to Lipps' seminar where he was struck with the small collection of apparatus for experimental psychology which stood unused and somewhat dust-covered in a corner of the room. A happy convergence of interests took place and led naturally to his first experiment with the complication-pendulum. Later he spent some time in Rome and Naples writing a prize essay on Vico as a social psychologist, out of which his short but highly successful *History of Psychology* seems to have grown. This book was translated for the American audience by Wilm and Pintner; later editions in Japanese, Spanish, and Italian won him an international reputation. His own translation into German of Titchener's *Textbook* was made during this period.

But Klemm loved the laboratory as well as the library, and his perceptual papers in the pre-war volumes of the *Psychologische Studien* reveal the careful research specialist. His investigation of sensory localization under conditions of disparate auxiliary stimulation demonstrated the law of spatial inducton and showed that localization is affected by the contemporaneousness of minor accompanying stimuli. In a *Mitarbeit* with G. F. Arps (later Dean at Ohio State University) on attention and rhythm, Klemm found that sensitivity to the accented part of a dactyl is greater than to the unaccented. He later discovered that there is a strictly monotic sort of localization and anticipated P. T. Young's famous criss-cross experiment reversing the normal sound paths. His first introduction to applied psychology came when he employed these techniques in training artillerists during the War.

Following the expansion of the activities of the Leipzig Institute under Krueger's directorship, Klemm engaged in a miscellaneous variety of researches and professional duties. He served as a consultant in bureau work for courts, prisons, agriculture, textile factories, sports, and guidance at the same time that he continued his inquiries on illusions and perception. He was the active bibliographical editor for the *Archiv für die gesamte Psychologie* and the *Zeitschrift für Psychologie,* a leading official of the *Deutsche Gesellschaft für Psychologie,* and, after the reorganization of the scientific publications in Germany on an "Aryan" basis, the senior editor of the *Zeitschrift für angewandte Psychologie.*

In his theoretical outlook, Klemm was a Neo-Kantian and a thoroughgoing advocate of the *Ganzheit* position favored by the Leipzig school as distinct from the Gestalt approach of the Berlin-Frankfurt "physicalists." In the paper he read before the International Congress at Yale in 1929 he held that all experience of meaning is an awareness of belonging to a whole, and in the contribution to the Jubilee volume of this JOURNAL (50, 1937, 157-165) the phrase "Glied zu sein eines Ganzes" appears with monotonous and insistent regularity. He adopted a psychology of types in dealing with educational and other "practical" problems, and, although he surmised that psychologists of the future would be less satisfied with "integralism" than he was, for one whose youth had been gravely disturbed by conflicting theories no other attitude was half so congenial.

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[1] O. Klemm, *A History of Psychology in Autobiography,* 3, 1936, 153-180.

Modesty was Klemm's outstanding trait; he was never the pompous overlord to younger students, but always like one of them. A tall, thin, sunburned figure, he engaged in vigorous sports as long as he lived. His writing has a wistful quality; it suggests that what he brought to expression and completion was only a sample of what he had actually lived. He made no pretensions to eminence and suspected that no one else thought of him as a major figure; his was a thoroughly honest mind with a mystical and ethical bent. Nowhere is the mood of the man better revealed than in the few lines with which he ended his autobiography: "How about the answers to those riddles of the Sphinx which ensnared the boy of twenty? The hands of the mature man of today no longer reach for the unattainable; they grasp more firmly that which has become spiritually his own. But his eyes are still fixed upon the stars, which are just as far away now as they were of yore."

Columbia University GEORGE W. HARTMANN

THE CORRECTION OF AN OMISSION IN *Shape as a Function of the Vector-Field*

In my article, *Shape as a Function of the Vector-Field,*[1] Fig. 1, p. 35 of this volume of the JOURNAL, shows the geometrical fields used in the experiments and was intended to show the positions of cohesive equilibrium for the fields, but the lines showing these positions were omitted from the drawings. A corrected figure accompanies this note. In it the fields are reproduced with their respective positions of cohesive equilibrium indicated by the dashed lines. These positions, which Fig. 1 intended to show, are referred to specifically in the article on p. 36, line 1 and footnote 12, and on p, 43, line 3.

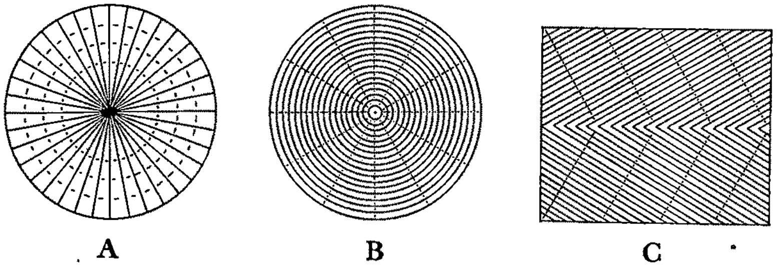

FIG. 1. GEOMETRICAL FIELDS USED IN THE EXPERIMENTS WITH THEIR RESPECTIVE POSITIONS OF COHESIVE EQUILIBRIUM

A, B and *C* are the fields used. The dashed lines, which were omitted in the original figure, show the positions of cohesive equilibrium.

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WILLIAM D. ORBISON

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[1] W. D. Orbison, Shape as a function of the vector-field, this JOURNAL, 52, 1939, 31-45.

BOOK REVIEWS

Psychology and Religion. By CARL GUSTAV JUNG. New Haven, Yale University Press, 1938. Pp. 131.

In this volume, comprising three lectures delivered in Yale University, the distinguished author gives the experiences of certain patients of his, and what he believes to be their significance for psychology and religion. These particular experiences are interpreted in a context of hundreds of others which have come to his knowledge as a specialist in nervous and mental disease. The book is of important interest in its substance, but is at times ill-formed, and to reproduce accurately its chief findings is no easy task. Until later, let me be strictly an expositor, using to a large extent the author's own words. Criticism may come after a patient attempt at understanding.

His intention is "to give a few glimpses, at least, of the way in which practical psychology becomes confronted with the problem of religion" (p. 2); one of the patients here described has a cancer-obsession which in Jung's judgment, expresses a profound discord between the patient's religious impulses and other promptings of his. Jung aims to deal with his data as a scientific psychologist who finds it "indubitable that the real causes of neuroses are psychological" (p. 10); for he has seen, to give but one of many examples, "a case of hysterical fever, with a temperature as high as 102°, which was cured in a few minutes by a confession of the psychological cause" (10). For Jung, "it is an almost ridiculous prejudice to assume that existence can only be physical;" in fact "the only form of existence we know immediately is psychic;" physical existence is merely an inference from this (11).

What are, more distinctly, the observations on which Jung bases his present conclusions? Along with hundreds of others which are merely referred to, three dreams by a particular patient are given in some detail, in which there is religious imagery and feeling. Jung ascribes especial significance to the *fourfoldness* in some of this imagery, and to the feeling connected with it. In one of the dreams, for example, there is a solemn house which people enter, and within the house "are many burning candles arranged so as to form four pyramid-like points" (42). This, the author connects with many other dreams reported to him and others, in evidence for his belief in the importance of the number four as a religious symbol for these patients and for others.

Now the four, which in the particular dream here described appears as an arrangement of lights, appears in other dreams in other forms, such as a town square, a square garden, and so on (79). In a way not clear to the reviewer, Jung seems to include the circle, globe, clock, wheel, and much else, among the dream-images which display fourness, quaternity, and its religious meaning (65). "The four symbolizes the parts, qualities and aspects of the One" (71). The author connects the symbols in his patients' dreams with like symbols in the Middle Ages, Greek antiquity, and more, which signify a world-creative deity (71).

Psychologists generally will be less interested in this than in Jung's theory of the origin of such images and their freight of meaning. For he holds that "certain ideas exist almost everywhere and at all times and they can even spontaneously create themselves quite apart from migration and tradition. They are not made by the individual, but they rather happen—they even force themselves upon the individual's consciousness" (4). Elsewhere he speaks of "forms or images of a collective nature which occur practically all over the earth" (63), which "presumably start from the archetypal patterns of the human mind which are not only transmitted by tradition and migration but also by heredity" (63 f.). Or again, "It seems that" his patients' "unconscious mind has worked along the same line of thought which has manifested itself, time and again, within the last two thousand years" (111). Accordingly Jung assumes "a certain unconscious condition carried on by biological inheritance,"—not an inheritance of "representations," but of "something like a possibility of regenerating the same or at least similar ideas." Such a possibility he calls "archetype" and regards it as something both mental and cerebral (112).

In this attempt to explain the origin of certain ideas, we have also touched upon what he offers about religion; for the ideas and images in the forefront of his discussion are religious. Jung is explicit, however, in denying that such ideas—of the One, of world-creative divinity—are proof of the existence of such objects. They and their dream-contexts, he believes, show the untoward effect of repressing what is religious; and health requires that this living part of the psyche be restored to consciousness and reconciled with it (99).

Religion, as Jung here depicts it, is not mere idea, symbol, dogma, and ritual. It is an attitude peculiar to a consciousness which has undergone a particular kind of experience (5 f.)—an experience which to some may be so disturbing, so distressing that creed and the rest are resorted to as a screen and defense against the experience itself (52 f.). To others, however, the immediate experience is welcome and beneficent; it is characterized by the highest appreciation, and it means everything to the one who has it (75); he "possesses the great treasure of a thing that has provided him with a source of life, meaning and beauty, and that has given a new splendor to the world and to mankind" (113).

All the matters touched thus far are constantly connected by Jung with the unconscious mind. What he means by the unconscious must, as for other meanings of his, be compacted of scattered passages.

"As a matter of fact the concept of the unconscious mind," he tells us, "is a mere assumption" (47). It is "the Unknown" (21). Yet much, it would appear, is known about it; for dreams are its voice (21), and both psychoses and dreams reveal the roots of a patient's obsession, conflicts, and complexes, quite as do the association tests (25). Thus associations, too, reveal the unconscious.

From dreams, associations and other sources the Unknown, the concept assumed, can be described with fair fullness and confidence. The unconscious is "an autonomous power" (17); it has "dangerous tendencies" (21); the male unconscious is different from the female unconscious (34); it is "capable at times of assuming an intelligence and purposiveness which are superior to actual conscious insight" (45). Dreams which, as was said above, are the voice of the Unknown, are elsewhere said to be probably "visible links in a chain of unconscious events" (38). Dreams, it would thus seem, are not mere reports about the unconscious; they probably are parts of the unconscious which have become visible to us.

Conscious and unconscious, it would appear, have contents in common or which pass from the one to the other. Contents may be conscious or may be repressed or isolated from consciousness (93); again, many unconscious contents may seem to be the remnants of historical mental conditions which were conscious several centuries earlier (66).

The relation between the unconscious and the conscious is, accordingly, that of active intercourse. This relation is elaborated, although often in language which is pictorial rather than literal and precise.

By "some uprush of collective forces" a gentle and reasonable being can be transformed into a maniac or a savage beast; indeed "we are always living upon a volcano" with no known human means of protection against an outburst (16). "We may assume," Jung says, "that human personality consists . . . of consciousness and . . . of an indefinite large hinterland of unconscious psyche" (47). But again, it is not assumption but fact—"the fact that an individual consciousness is based upon and surrounded by an indefinitely extended unconsciousness psyche" (99). "The psyche reaches so far beyond the boundary line of consciousness that the latter could be easily compared to an island in the ocean" (102)—the island "small and narrow; the ocean immensely wide and deep" (102). Jung indeed is "not at all certain whether the unconscious mind is merely *my* mind" (46). At times he believes such an appropriation of it to be inaccurate (47). Indeed when he uses the figure of an island in the ocean, this suggests that the unconscious may, in a sense, be shared by many conscious minds, as in actual geography many islands may all be in the one ocean.

Now to the ungracious work of doubt and appraisal—some of which is already done, for in the very attempt to bring together the scattered parts of some thought, to let the reader see it as a whole, the parts have at times refused to fit together. One is left in doubt because Jung, it may be, is himself in doubt, and puts down his thought on the selfsame matter now with hesitation and now with assurance.

Psychologists trained in the laboratory and in statistics will, I fear, be left unsatisfied with Jung's account of the origin of the image-symbols he calls archetypes, particularly with his conclusion that they are natural symbols which recur spontaneously in individuals, who neither influence one another directly nor are influenced by a common social inheritance. His patients, he is convinced by careful investigation, he says, were completely ignorant of what might have come from such influences. The details of this investigation are not given in the present volume, and one is left to doubt whether all possible sources of knowledge by the patients—from long forgotten reading or hearing have been or could be completely examined and excluded. One may be the more cautious because Jung's theory of the origin of these image-symbols goes beyond those fundamental psychological powers which, for Kant and others, have given to our experience its general framework of spatial and temporal relations: According to Jung, there are inborn tendencies to create fairly particular spatial patterns and to give to them a fairly definite and complex meaning by which they are highly significant religious symbols. Until other possible explanations are fully examined and excluded, one can accept all the facts observed by Jung regarding these symbols, and yet remain unready to accept his theory of their origin.

One may be especially unready to believe that the number four with its freight of associations should be counted as a natural symbol. One bears in mind that a considerable array of other numbers—one, two, three, five, seven, eight, ten, twelve, and higher—have each been freighted with significance for religion, morals, magic, science, and practical affairs. Cultural development would seem still to be our most promising source of explanation of each of these numerical ideas, of its image-representation, and of its significance as a symbol generally.

These doubts must not obscure the value of the book. It makes a welcome contribution to our store of observation of abnormal minds, by a very distinguished practitioner and leader in this region. Facts are stated, questions are raised, and answers are offered. The author pleads for nothing but a scientific scrutiny of some important and intricate observations. Jung's manner, while at times sharp against opposing ideas, is usually with dispassionate conviction, with now and then a sentence warm in its sympathy and appreciation. There will be many psychologists ready to study the book no less dispassionately and with no less appreciation.

University of California GEORGE M. STRATTON

Studies in Experimental Phonetics. By Giles Wilkeson Gray, H. Philip Constans, Wesley A. Wiksell, William Hobart Sallee, and Harriett R. Idol. Baton Rouge. Louisiana State University Studies, Number 27, 1936. Pp. 164.

Teachers of speech and voice have emphasized for many years the importance of breathing in voice production. Unfortunately, however, much of the material on this subject which has been included in textbooks of speech has been incomplete, confusing, and, all too frequently, contradictory. Dr. Gray's series of studies on breathing in speech attempts to determine how much of the current literature on this subject is based upon fact and how much upon fancy. Few monographs in the field of speech have accomplished their objective as thoroughly.

The monograph embraces six studies. Mr. Constans shows that the *effusive* and *explosive* "forms of force" which are traditionally used to describe different types of breathing are, as a matter of fact, produced by the action of the articulatory, rather than the respiratory mechanism. Training in breathing as such, therefore, can have little, if any, effect upon these "forms of force." Rather, they may be thought of as terms which describe the type of *articulation* which the speaker uses, not the type of breathing.

Mr. Wiksell, who studied respiration in relation to the intensity of the vocal tones in speech, concluded that there is no "individual relationship between the type of breathing (abdominal versus thoracic) and the intensity of the voice." This is in sharp contradiction to statements found in numerous texts, *i.e.* that the abdominal breather has a more intense voice than the thoracic breather.

Mr. Sallee, studying respiration in relation to audibility of speech, concluded that there is little, if any, relationship between depth of inhalation and audiblity and, further, that there seems to be "no evidence that any one particular type of respiration is superior to any other in relation to audibility, either for individuals or for groups."

Dr. Gray in his investigation of regional predominance (abdominal, medial, or thoracic) concluded that "it is impossible . . . to predict from the type of respiration, or the degree of regional predominance, what any given person's voice is or ought to be, in the particular type of utterance with which these studies were concerned."

Miss Idol, from a study of respiration in relation to speech characteristics, concluded that "no measurable aspect of casual breathing or of breathing during phonation has been found which *per se* affects these vocal characteristics which were considered, namely, audibility, quality, articulation, functional efficiency, general control, and control of force."

Mr. Wiksell, in his study of controlled and uncontrolled types of breathing (an abridgment of his doctoral dissertation) removes still more misconceptions about the relative importance in speech of different types of breathing. His results in general indicate that the type of breathing, whether abdominal, thoracic, or medial, has little effect upon any measurable aspects of speech or voice. It is more important to have an adequate quantity of air available than to have some air available from any specific functioning of the breathing mechanism, *i.e.* if a subject is proficient in getting a fairly large average volume of breath movement through the use of uncontrolled breathing [whatever type he is accustomed to], he will find it of no advantage, and it may even be a disadvantage, to develop the use of a controlled thoracic inflation, and . . . he will be even less successful in increasing the volume of breath movement by using an abdominal type of respiration."

The above summaries are only brief—and more or less randomly selected—highlights from the wealth of experimental data packed into the 164 pages of this monograph. Although the studies in general are largely of a negative nature and may, therefore, not be received too readily by the speech profession at large, it is safe to say that eventually Dr. Gray's work on breathing will have the same effect upon the traditional notions about the rôle of breathing in voice production that scientific studies on personality traits have had upon phrenology.

Purdue University JOSEPH TIFFIN

Problems of Psychology. By H. J. GRUENDER. Milwaukee, Bruce Publishing Co., 1937. Pp. xi, 209.

This book was written as a text for undergraduate courses in the "science and philosophy of our rational life" (p. v). It is intended to serve as a supplement to the author's *Experimental Psychology* and was written because of his view that psychology to become a real science instead of an empirical one must enquire into the ultimate sources of consciousness and thus become philosophical as well as scientific.

He accepts the scholastic interpretation of Aristotle's philosophy given by Thomas Aquinas and in terms of its postulates and hypotheses critically analyzes the following problems: the image theory of thought as interpreted by Titchener; the process of abstraction; free will and modern difficulties against free will; the problem of personal identity and the localization of thought. He shows clearly that the logical conclusions to which sensory associationism leads are untenable and explains the Würzburg hypothesis of imageless thought as consistent with the scholastic doctrine. He rejects Berkeley's view that generalizing abstraction is impossible because it is inconsistent with the scholastic interpretation of thought and the experimental findings of Kuelpe and other investigators of the Würzburg school.

Some of the arguments against free will, he thinks, are due to a misunderstanding about the theory of free will. The scholastic theory of free will does not assume *absolute indeterminism;* that is, that man is not influenced by motives, acquired tendencies, habits, his environment, etc. Free will is defined as freedom of choice between two alternative courses of action by the *active interposition of the Ego* (p. 70). This view is presented by syllogistic reasoning which, because of the abbreviations used in presenting it, becomes very abstract and it would be difficult for a student who had no knowledge of philosophy to follow. The scholastic doctrine explains the Ego not as a duality composed of body and soul but as . . . "the composition of man arising from the union of two incomplete substances" (p. 137). In considering the problem of personal identity, he has given a concise statement of the hypotheses in regard to the mind-body problem advanced by the various schools of modern materialism, all of which he rejects for the Aristotelian concept. The presentation of this problem would have been more effective if the theories had been given with more reference to those who have advocated them and to the historical setting out of which they developed. The argument for the scholastic doctrine is concluded with the explanation that thought and volition are spiritual activities and that the soul of man though conditioned by the body may exist apart from it and so constitutes the noblest part of man.

There is a need at the present for psychologists to formulate more clearly the postulates upon which their investigations and explanations of behavior are based, but many psychologists and philosophers will think that a systematic consideration and evaluation of the validity of those postulates is the problem of the philosopher and not of the psychologist. It is the writer's view that the first course in philosophy should give a student a more general orientation into the field of philosophy with less emphasis on establishing the validity of the concepts of one philosophical system. The book, if supplemented by much reading in the history of both philosophy and psychology, would undoubtedly help those who plan to major in psychology clarify their own conceptions of the metaphysical problems considered.

Georgia State College for Women EURI BELLE BOLTON

Causes of Crime. Biological Theories in the United States, 1800-1915. By ARTHUR E. FINK. Philadelphia, University of Pennsylvania Press, 1938. Pp. x, 309.

The early history of American criminology, penology and theories of crime is more or less hidden in chance articles in biology, medicine, anthropology and psychology. The men making these various contributions were usually busy men engaged in other fields, chiefly medicine. While their contributions are valuable, very interesting historically and often quite essential to any present understanding of the ideology in modern criminology, they have been more or less scattered so that to the average reader in penology there has been a distinct gap in the early literature. We tend to read the European writers, as Lombroso, and to jump from there to the more recent contributions in the United States. It is also usually customary for writers in penology or criminology to refer to the European sources and to neglect the American sources.

What Dr. Fink has done is to carefully edit and digest the essential ideological factors—biological, psychological, medical and social. His own statement as to the reason for the study are pertinent. "The conclusion," he writes, "is inevitable that the historical record of the early labors is bare because social historians have failed to probe into and interpret the nature and extent of these accomplishments. This study undertakes to meet this need." Dr. Fink has done a good job in editing and in simplifying. Anyone who has read the elaborate categories of Spurzheim with 35 sub-headings or Gall with 26 sub-headings knows the tendency for hopeless elaboration if one once gives way to the temptation. The author has carved a very clean-cut path of essentials and classified them under the following headings: Phrenology; Insanity; Moral Insanity (which is still very much alive as evidenced by the symposium in the British Journal of Medical Psychology a few years ago); the part played by Alcohol and Drugs; Criminal Anthropology; Heredity, which is divided into a chapter in which the author deals with the theoretical side, followed by a study of a reëvaluation of the Jukes and the problem of Human Sterilization; and finally, Feeblemindedness.

The author brings this historical period (1800 to 1915) to a close with the introduction of the work of Goddard dealing with the application of the Binet test in this country and the publication of Healy's *Individual Delinquent.* Healy's contribution marks the turn of ideology from a hunt for stigmata and group causes to an emphasis upon individual difference and a vast interlocking directorate of causes of crime.

It hardly needs be said that the book might be very dangerous reading to an individual who read the contribution, let us say, on classificatory anatomy and faculty psychology and was unfamiliar with the critical views that have been added in recent years, such as Lashley's theory of equipotentiality.

The book is a valuable contribution and will save the teacher and the critical student of criminology hours of weary hunting for minor historical threads in less accessible books and references. It will give him the essential gist of a writer or a viewpoint with a minimum expenditure of energy.

University of Pittsburgh W. T. Root

Are We Movie Made? By Raymond Moley, New York, Macy-Masius, 1938. Pp. viii, 64.

This reviewer opened "Are We Movie Made" in full expectation of finding a layman's view of the movies, heavily illustrated by anecdote and worth perhaps a three-line review in this Journal. If others do the same, they may well share the writer's amazement at discovering that the monograph contains a sharply abbreviated but capable summary of Mortimer Adler's critique of the Payne Fund studies on the effects of motion pictures. The original report is made in Adler's "Art and Prudence."

Defending the thesis that "the scientific work that has been done (on the effects of movies) is of little or no practical value to the prudent man," Moley takes up Adler's criticism of each of the Payne Fund reports. The study of the movies as a condition of juvenile delinquency, by Blumer and Hauser, is severely criticized on the grounds of type of material used, of 'loading' these materials by biased instructions, and of drawing conclusions not warranted by their data. Peters' investigation of standards of conduct in the movies is taken to task for employing questionable standards of comparison and for making biased extrapolations from actual results. Dale's report on the content of moving pictures is attacked as arbitrary in classification, faulty in method of inspection, and as providing "discussion wholly unrelated to his elaborate and costly studies."

There is no point in carrying our summary further. Only three of the studies are accepted by these critics as representing "science properly used." The other studies are acridly portrayed as incompetent in technique, or as prejudiced in interpretation or both. Charters' summary is depicted as even less free from these sins of interpretation than the investigations he seeks to integrate, while Forman's "Our Movie Made Children" is flayed as a popularization in which conclusions outrun data by intolerable amounts. The concluding sections of the book present *verbatim*

Adler's conclusions together with an exposition of his thesis that "the first obligation of the moralist is so to train men that they will be able to receive any work of art without harm; only if he fail in that will he have to concern himself with regulating the reception of works of art."

The criticisms of the Payne fund studies are worthy of an answer from the original investigators. If they have been guilty of even a small proportion of the abuses of which they stand accused, psychology will gain by a public investigation of the charges. Until such an investigation has been made, no psychologist who reads this book will be likely to be over-free in quoting the conclusions under discussion.

The careful reader may share this reviewer's regret that the author has committed the reformer's error of trying to paint a uniformly black picture; in seeking to maintain his critical position he has been guilty of straining at certain methodological gnats. One regrets, too, that the acid of criticism so freely applied to the works of others should change to honey whenever Mr. Moley considers Adler's contributions. Others may err, but when Adler speaks "the sharp edge of analytic reason moves through the half truths and prejudicial conclusions of these studies like a keen scythe in a clump of weeds." Such sophomoric adulation ill befits the critical air of the essay. Nor is one overly impressed with the 'solution' over which Mr. Moley labors so lovingly. One feels that it might work in a world of philosophers but that it has little bearing on actual children going to actual movies. So faithfully and so unquestioningly does Mr. Moley label the movies as an art that one wonders how frequently he has gone to observe the nature of run-of-the-mine movies.

In sum, the volume presents in brief compass a critique of methodology and of interpretation in a well-known series of social studies. The critcisms are serious enough to warrant an answer, and the negative contribution is correspondingly large. Its positive contribution and its undeviating aura of hero-worship seem less likely to bulk large in later studies of the effects of moving pictures.

University of Maryland JOHN G. JENKINS

Psychological Aspects of Business. By E. K. STRONG, JR. New York, McGraw-Hill Book Co., 1938. Pp. x, 629.

Those who are familiar with Strong's *Psychology of Advertising and Selling,* published in 1925, will find in this new volume a great many threads of continuity. Not that it is a rehash of the earlier book, for new chapters have been added and the old chapters have been thoroughly revised. The continuity is rather in point of view. In 1925, Strong broke sharply with the traditional, static point of view to write a dynamic psychology of advertising in terms of human wants; in 1938 a study of wants still provides the framework for his treatment.

The volume opens with two sections which set the stage for later chapters by analyzing the acts of buying and selling in terms of wants, obstacles, and solutions. The same sections contain a discussion in general terms of how man comes to have and to attempt to satisfy his wants. A section on "Principles of influencing others" treats such varied topics as consumer research, motivation, and propaganda—all of which are interpreted in terms of wants and solutions of wants. The fourth major section of the volume is concerned with the tactics of selling; here the process of selling is followed from the initial prospecting through to the closing of the sale. A section headed "Research techniques" proves to be an elastic receptacle for such disparate items as questionnaires, rating scales, job analyses, and safety programs. The volume concludes with a section on employer-employee relationships, including the wants of both groups and problems of leadership and morale.

The volume is rich in documentation and there is much evidence that Strong has read widely and thoughtfully. There are many references to his personal contacts with the several fields and a pleasing lack of vague generalizations made in the absence of direct knowledge of conditions. The author writes with conviction and in simple literary style. His case-material has been carefully selected and his sales-interviews have obviously been tempered by actual use.

The shortcomings of the volume suggest that it was written without the benefit of any considerable amount of extra-mural criticism. The author's definition of *want,* for example, is veiled in ambiguity which might have been eliminated if other psychologists had worked over the manuscript before it was published. Thus we find initially that a want "is a drive—a physiological mechanism," only to discover a few pages later that it "is a sensation," while a third section informs us that a want "is comparable to mental set . . . purpose is another synonym." Because a dynamic interpretation is so patently required by current workers in this field, one regrets that Strong was not required to clarify this basic concept before final publication.

Criticism before the manuscript was printed might also have pointed out to the author the irritating frequency with which he employs an unjustified definite article. Surely his exposition would have gained both in force and in dignity if he had spoken of "three factors" or "four conditions," rather than "*the* three factors" or "*the* four conditions." Such criticism might also have called the author's attention to his tendency to relapse into a taxonomic, *a priori* discussion of advertisements in the same volume that contains his review of the very market research methods which have cast suspicion upon this earlier way of 'analyzing' advertisements.

The volume as it stands is a stimulating presentation of the facts and methods in the wide field afforded by psychological problems in business and industry. The better parts of the exposition will take their stand with the best that has been written in this field. The looser parts are reminiscent of the commercial sales manual in language and in their comforting assurance that everything will turn out as predicted. The excellence of the former makes one regret that the latter were not more thoroughly passed through the alembic of alert—if friendly—criticism.

University of Maryland JOHN G. JENKINS

The Development of Children's Concepts of Causal Relations. By JEAN MARQUIS DEUTSCHE. Instit. Child Welfare, Monog. No. 13, Univ. Minn. Press, 1937. Pp. vi, 104.

Employing a group-testing technique, Dr. Deutsche secured the written explanations of various phenomena by 732 children between the ages of 8 to 16 yr. There were 11 questions with demonstration, and 12 questions without demonstration. The answers were subjected to exhaustive analyses, both quantitative and qualitative, and a wealth of results was secured.

The quantitative method used in analyzing the data, however, is reported un-clearly and, as we learned in correspondence with the author, incorrectly. As far as we have been able to understand it, the method was as follows. The children's answers to the questions were evaluated in terms of their adequacy as scientific explanations by 13 graduate students, who were given a set of typical answers to each question plus explanations for each phenomenon by a physicist. The judges rated the answers on a 7-point scale each point of which was carefully defined. So much is clear, but from here on the exposition of the method is inadequate and erroneous. The raw median rating values thus obtained were then, apparently for purposes of weighting, expressed as percentages of the perfect score, *i.e.* of 7. These percentages were then converted into their corresponding sigma values and the sigma values were then *multiplied* by 2 (as we were informed by letter from the author—the monograph reports "divided by 2"); 46 was subtracted from these results, and the remainders were divided by 10. The nearest whole numbers were taken as the "final quantified scores."

Assuming a set of raw median rating values varying between 0 and 7, the reviewer carried through the calculations indicated. The resulting scores were all negative, and inversely related to the raw scores, and, if the nearest whole numbers were taken, the final scores obtained from such raw scores as 1, 3.5 and 5 were all —5. Dr. Deutsche's final results were all positive, ranging from 0 to 8, with the most adequate answers securing the highest scores. In every particular, then, our calculations ended with results different from those of the study. It is difficult to see how the author arrived at her values; and it is even more difficult to find a justification for her method.

In the light of these inconsistencies and errors, it is difficult to discuss the quantitative results. Let is suffice, then, merely to indicate that the author investigated age and sex trends; and the relationship of the "quantified scores" to socio-economic status, intelligence and school grade. Evidence was found to suggest that training and experience as acquired in the schools are the most important factors of those studied in determining the ability of the children to answer the questions on the tests.

In the qualitative analysis, three raters independently classified the answers according to Piaget's division of 'causal thinking' into 17 types. The resulting disagreement made it clear that Piaget's classification was inadequate, dependent upon the individual judge, and that certain of his types are very specific to a certain kind of question. A group conference was necessary before a unanimous classification was reached.

Analysis of the five 'types' which were found in "fairly high percentages," *i.e.* from approximately 6% and up, led the author to suggest that "causality does not develop in definite stages, with types of response characteristic of one age and not another." The kind and level of the answer seems to depend in great part upon the question asked, and not upon the age of the child.

No significant age-differences were found in the number of materialistic or non-materialistic explanations given. When the materialistic answers were subdivided into five categories, however, a direct relationship between increasing age and the percentage of answers using cause-and-effect relationships was found.

The answers secured in this study were compared with the results of other investigations, in which similar questions had been used. The answers from investigation to investigation were highly similar. No evidence was found to support "Piaget's classification of the answers to specific questions according to stages of development." In common with all investigators who have repeated Piaget's questions, there was found "a much higher percentage of naturalistic explanations . . . than Piaget reports," and prelogical answers were "conspicuous by their rarity."

There is an introductory chapter containing a review of the relevant literature; and a chapter analyzing the number of words used. The chapter on "Interpretation of Findings" includes an extensive summary. At the end of each chapter, too, there are convenient and good summaries. There is an index, a bibliography (42 ref.), and many graphs and tables.

Cornell University JOHN IRVING LACEY

The Use of Tests and Rating Devices in the Appraisal of Personality. By ARTHUR E. TRAXLER. New York, Educational Records Bureau, Bull. No. 23, 1938. Pp. 80.

"This bulletin has been prepared to present a brief summary of the field of personality measurement, together with a description of some of the leading contemporary tests of personality and a summary of research dealing with them. The purpose is to provide a simple, non-technical manual on personality measurement for teachers in service who may contemplate the use of personality tests in their work, but who are too busy to survey the whole field of personality evaluation in order to select the tests suited to their situation."

Traxler states that there are between 400 to 500 published personality tests. Of these, 45 are considered among the most useful and are described in detail. His bibliography includes 182 titles, all of which are relevant to the field of personality and of interest particularly to the student who wishes to pursue the subject beyond the limits set forth in the bulletin. Critical comments of personality test-procedures are to be found in all the chapters, but principally in the Introduction and in Chapter V.

In his evaluation of the usefulness of personality tests, Traxler is constructively critical. He definitely points out that most personality tests have been hastily put together and lack validity because of the absence of validating criteria. For these and other reasons, personality tests are seriously limited for purposes of individual diagnosis. Yet, the author is of the opinion that a "large number of good studies dealing with certain personality tests are now available." Moreover, he states that the field manifests satisfactory symptoms of progress in that the present research workers are using improved statistical techniques, are ingenious in the development of new procedures, and are viewing their results with healthy scepticism.

The limitations, nevertheless, remain. One of the more significant is the fact that an unstable individual, may, in a test situation, manifest characteristics typical of him—thus destroying the validity of the data. In any event, the usefulness of a personality schedule, like an intelligence test, depends largely upon the skill with which the psychologist interprets results from the standpoint of the clinical history. It is the psychologist's task to discriminate between the significant and the trivial. In his hands, psychometric measurements, even though imperfect, throw a great deal of light on practical psychological problems and their solutions.

The Training School ANTHONY J. MITRANO
Vineland, N.J.

Psychology in Everyday Living. By F. F. POWERS, T. R. MCCONNELL, W. C. TROW, B. V. MOORE, and C. E. SKINNER. Boston, Heath, 1938. Pp. x, 511.

In the first four chapters of this book one encounters a naïveté seldom found in modern psychology texts. With a few exceptions the remaining chapters must be rated among the best to appear in books attempting to relate psychology to everyday life.

The first four chapters are characterized, in the main, by headings and subheadings with a minimum of enlightening exposition. Moreover, the exposition is often superficial, abstruse, or quoted or paraphrased from other elementary texts. To illustrate: Thought is discussed under the heading, "Difficult problems of psychology" and the subheading, "The nature of thought." The entire first paragraph follows: "Are there such things as thoughts? Does the fact that nobody has ever seen a thought prove that they do not exist? Can thoughts be transferred without sensory intervention?" The remaining short paragraph indicates that attempts to transmit thought by telepathic means have led to ambiguous results. With the exception of a brief consideration of reflective and creative thinking which was written by another author and appears near the end of the book, the above constitutes this book's contribution on the nature of thought. In the third chapter, entitled "Foundations of behavior," one finds something approximating a scrapbook of quotations and paraphrased statements from other texts. The uninitiated might gather that Hunter is an authority on the structure and functioning of the pyramidal tracts, the limits of tonal sensitivity, and the interdependence of taste and smell; Murphy on the functioning of the endocrines; Lund on polarization in neural activity; and so on. As illustrative of this point and also of abstruseness in exposition, take the following example: Of tonal limits, the author says, "Hunter gives the range of ability to receive and transmit sound to the brain for the receptors of the human ear from eighteen complete vibrations per second, to a frequency of 15,000 to 22,000 complete vibrations per second." In the following chapter we learn that "Personality is that portion of the totality of an individual's conduct to which others respond by emotionalized evaluation or the evaluation itself."

Such writing characterizes less than a fourth of the book. As already indicated, the remaining chapters are, with a few exceptions, highly praiseworthy. In a section entitled "Understanding ourselves" are to be found excellent chapters on the psychology of adjustment, human motivation, intelligence and special aptitudes, and individual differences. These discussions are not only written in an interesting style, but they take cognizance of much recent original research. Two other exceptional chapters, in a section on "Adjustment to college and life," concern vocational choice and psychological factors in successful marriage. In a section dealing with "Learning and study" are chapters on the general nature of learning, acquiring knowledge and skill, the acquisition of attitudes, learning to study, and problem solving. These constitute a brief, but sound and well-written summary of present knowledge concerning the learning process. The final section of this volume introduces the reader to various applications of psychology. Business and industry, law, politics, religion, medicine, and education are considered. Each chapter contains questions for further study and a list of references. A long list of references to books on various aspects of phychology appears at the end of the volume.

In the reviewer's opinion, this book will find its greatest usefulness as a supplementary rather than a main text. It is notably weak on sensory processes, neural functions, emotion, thinking, perception, and methodology. If the course is a brief one for students who intend to do no further work in psychology, the chapters mentioned in the above paragraph as providing an excellent survey of psychological contributions to everyday living may serve as an adequate substitute for the more orthodox treatise.

Vanderbilt University NORMAN L. MUNN

Electrical Signs of Nervous Activity. By JOSEPH ERLANGER and HERBERT S. GASSER. Philadelphia, University of Pennsylvania Press, 1937. Pp. x, 221.

This monograph incorporates the lectures delivered by its authors upon invitation of the Eldridge Reeves Johnson Foundation, and is designed to review and evaluate the actual status of nervous electro-physiology. As such it is particularly useful for the psychologist who wishes to gain an authoritative and intelligible account of those neural events which underlie his own problems of behavior. In the first three chapters Erlanger analyzes the action-potential of nerve in its various phases (alpha, beta, gamma, delta, B, C) and its relation to fiber-diameter, speed of conduction and amplitude of potential. In the later chapters, Gasser reviews the correlations of these potentials with heat-production, metabolism and excitability (refractory, supernormal, and sub-normal periods). While some of this material may prove to be rather technical for most psychologists, the whole treatment should be carefully examined by every student of behavior. It cannot be too highly recommended for all who wish a modern and lucid account of the mechanisms which underlie the simpler modes of response.

Recent studies upon conditioning in the spinal and curarized preparation clearly demonstrate that the topics examined in this book must be more seriously considered by the active investigator than has been true heretofore. Instead of contenting ourselves with the gross (intact) animal, we must analyze conditioning in terms of the concepts which have been so ingeniously and laboriously developed by the 'axonologists.' The following preliminary observations in the Rochester laboratory may serve to illustrate this contention. (1) 'Conditioning' in the spinal preparation begins with certain fibers (presumably the larger and faster) and then may or may not spread to other fibers. (2) Under curarization, some fibers (the smaller and slower?) cease early, leaving the others still active; these in turn fail as drug-effects increase. (3) In the spinal preparation, the same fibers which persist longest under curare are as a rule the only ones which show evidence of 'conditioning.'

These few observations indicate that we can no longer work with the gross units of 'nerve' and 'muscle' in our own field; we must begin to utilize the concepts of electro-physiology in the analysis of behavior.

University of Rochester E. A. CULLER

Practice in Preschool Education. By RUTH UPDEGRAFF and HELEN C. DAWE, EVALINE E. FALES, BERNICE E. STORMES, MARY G. OLIVER. New York and London, McGraw-Hill Book Co., 1937. Pp xvi, 408.

Dr. Updegraff and the head teachers in the Preschool Laboratories of the State University of Iowa here present a clear account of the aims and techniques of one of America's first and most influential centers of preschool education and research. The authors begin by describing typical days in the 2-, 3-, 4-, and 5-yr.-old groups, noting both children's and teachers' behavior; subsequent sections detail the child's introduction to the preschool, the manner of providing health and physical care, and provisions for motor activity, eating, resting and elimination. Of special interest to psychologists are five chapters on intellectual development: The General Environment, Books and Pictures as Sources of Information, Nature Study, Social Studies, Language and Speech, and a single chapter entitled Guiding the Child's Personality Development and Social Behavior; a chapter of primarily educational import discusses æsthetic development; the final chapter is devoted to collateral subjects: parents, staff, the use of the laboratories for observation and research, and the training of teachers. The appendix provides a veritable manual of teaching equipment and technique, rating and record forms, as well as bibliographies of children's books and of literature on preschool education.

Enough has been said to indicate that this book will be invaluable to preschool teachers and administrators and to many parents. Of more importance to readers is the function which this volume, with its virtue of taking nothing for granted, can play for research workers in the field. It provides a clearcut and unambiguous background of what happens to a child outside of the experimental situations in a typical, good American nursery school. Thus, of interest to many psychologists, is the expression of aims in social adjustments. Such 'rules' as the determination of ownership by use indicate sharply the kind of cultural conflict that the nursery school must take account of and in whose creation it must inevitably share. Equally clearly presented is the rather 'cool' emotional tone that is set by the teachers, which has been criticized by some psychologists and psychiatrists who consider that nursery school children are at a point of development where real 'mother-substitutes' are necessary to them. Whatever one's views on such topics data for their consideration are explicitly and objectively presented. The publishers have provided a splendid format for this useful book.

Sarah Lawrence College LAWRENCE JOSEPH STONE

Modern Society and Mental Disease. By CARNEY LANDIS and JAMES D. PAGE. New York, Farrar and Rinehart, 1938. Pp. vi, 190.

In a very compact little volume, the authors have enclosed a mass of material ranging all the way from the definition and classification of mental disease, to compilations and interpretations of statistics of mental disease in relation to age, marital condition, urbanization and economic conditions. In addition to all of these, they devote 20 pages to heredity, sterilization, marriage and eugenics. The book is a landmark on the road from clinical impressionism to science in the field of mental disease.

The authors are particularly to be congratulated upon their insight into the variables which must be considered in the interpretation of mental disease statistics. Certainly it will do much to correct many of the current misconceptions which we find today in much of the literature in this field.

Although the volume is encyclopedic and critical, it has many obvious flaws. Its encyclopedism does not signify that it brings together all of the evidence either for or against certain statistical problems in the field of mental disease. The criticalness of the volume is also occasionally marred by certain interpretations of data; for example, that dementia præcox and manic-depressive psychosis have a hereditary basis because "the relatives of dementia præcox patients succumb more frequently to dementia præcox than do other members of the general population." Similar evidence has too frequently been used to "prove" the influence of environment and just as categorically one might question his conclusions regarding the effects of depressions upon hospitalization when based upon one economic depression (the current, or last one, since we are told it ended in June, 1932). Further, a question of technique might be raised as to whether the best way of studying such effects is to compare first admissions in the depression with those in the "pre-plus-post" depression period.

Whatever specific criticism such as the forementioned may be directed against the book, it is a distinct contribution to the field of mental disease whether viewed from a psychological, psychiatric or politico-social point of view. It is so compactly written that it is not easy to read but it is eminently worth studying.

University of Newark FREDERICK J. GAUDET

Indianer-Rassen und Vergangene Kulturen. By R. N. WEGNER. Stuttgart, Ferdinand Enke, 1934. Pp. 320.

This is, at least partly, a report of travels among natives of South America as far away from civilization as the author was able to make contact with them. The book contains a large number of very good illustrations of the people, their art and their social life. The life of many of these tribes is extraordinarily primitive. The weaving of textiles is virtually unknown. Cottonstrings, to make a hammock, *e.g.* are in a few places near the middle of their length tied together with cross strings, instead of weaving. Making fire by friction is an art used by occasional "artists" only. In general, fire is eternally kept up, and glowing wood, carefully wrapped, is carried along even on journeys. Clothing is unknown, refused, despised. The arrows used for hunting and war are often ten feet long, and the bows, made of palmwood, are very powerful and longer than mansize. Medical remedies are virtually unknown. A small amount of pottery is used, but for tobacco smoking rather than for eating and drinking. There are plenty of other tribes, however, which represent every shade of aboriginal civilization and of Mixed-European culture.

The author points out the remarkable fact that the cinchona plants the medical value of which became known to the Spaniards in the early part of the seventeenth century, are nowhere cultivated in South America, whereas they were introduced into Java for cultivation by the Dutch government through the aid of the German botanist Hasskarl in the middle of the nineteenth century, and spread thence over East-India.

The Indian permits little children to play with sharp knives and with fire; and the little ones, from a distance watched lovingly by their parents, soon learn to handle such dangerous things harmlessly. The Indian does not understand the European parent's cruel tendency to keep such things away from little children. Likewise two or three year old little children have already learned how to save themselves without hesitation if the narrow dugout canoe capsizes.

More than two hundred varieties of the potato are known to the highland Indian. Where the climate is too cold to raise maize, it is the chief food on which he depends. The woman of the highland Indian has no civil rights, is totally subject, a slave, to her husband. The llama is treated with the greatest consideration, and its comfort comes before that of the owner. Its flesh is eaten only when it naturally dies. Music is made by means of flutes and drums. Too much alcohol is one of the economic obstacles.

The later parts of the book describe the ancient civilizations of Peru and Bolivia, the life of the present Indians who are descendants of those civilizations, the life of the colonial Spaniards in the sixteenth and seventeenth centuries, the latter also by means of many traditional anecdotes. Finally the Mayan architecture in Yucatan and Guatemala is depicted in a number of well printed illustrations.

Coral Gables, Florida MAX F. MEYER

Mental Health through Education. W. CARSON RYAN. New York, The Commonwealth Fund, 1938. Pp. ix, 315.

This book is an account of how much the movement for mental hygiene has permeated the whole educational process from teacher training institutions to teachers and administrators down to the pupils themselves. With mental health clearly in mind, Dr. Ryan was able to visit these various types of institutions and in this book is set down both the actual findings and his interpretation of their significance. Samples of higher institutions, of school systems and of classrooms whose activities show the influence of this point of view are presented. In general, too few school systems have caught the spirit of the movement because it means more than a few "core" courses, activity programs, etc., but rather a change in the atmosphere in which the whole educational process is carried on.

Progress towards mental health is being made. 'In-service' courses in mental hygiene, pre-service training institutions, and some of the newer experiments in higher education are samples of improvement at one level. Then, too, many school systems and classroom teachers are placing less emphasis on grades and promotions, recitations and home work, examinations and marks and more on balanced emotional development, right attitudes, and a well adjusted personality. Finally, child guidance clinics, the visiting teacher, and psychiatric service for schools while fairly frequent are greatly needed in large numbers.

The book is well written. It smacks, however, too much of the philosopher and propagandist and too little of the scientist. Statements are made about the desirability of change without an iota of evidence in its favor. If one could grant the fundamental assumptions concerning the development of mental health the rest of the book would be more acceptable. One is thus forced to conclude that this book by Dr. Ryan is a careful exposition of opinion and philosophy but not a book of science. It is more a book for the faithful than a treatise for the unbelievers.

University of North Carolina A. M. JORDAN

How to Teach: A Text for Upper Grade and Secondary Teachers. By CLAUDE C. CRAWFORD. Los Angeles, Southern California School Book Depository, 1938. Pp. 512.

The author recommends among other things that the teacher should make notes of all good ideas and apt illustrations as he encounters them in books, lectures, and discussions, and that he should preserve these notes, together with his own comments and reactions, in a carefully classified file. The author has evidently practiced what he preaches for many years, and this book is one of the outcomes. It is not a scholarly book in the usual sense, yet it reflects a lifetime of scholarship. It contains no tables, no diagrams, no graphs, and no pictures. Such experiments as are described are presented sketchily, and are used as illustrations rather than as foundations for the discussion. The illustrative stories are numerous, well chosen, and well told. The book abounds with mis-quotations and errors of fact, particularly in the illustrations, but its discussions are singularly free from fundamental errors, misinterpretations of the genuine significance of the results or research, and systematic biases.

The book is a mine of practical information and sensible hints. The idea of learning by doing, of teaching processes rather than principles, and "how to do it" rather than "which is what," is dominant throughout its pages; yet such words as "dynamic," "functional," and "organismic" are not overworked. The author is progressive and modern in his viewpoint without being too "progressive." In his preface he states, "I haven't made very much effort to use a dignified and conservative literary style because I have been writing to the collegiate crowd instead of to the professors." He means what he says. His book is easily the most interesting and readable one on its subject that has been published in recent years. If a good many of the suggestions offered seem to be such as should occur naturally to any bright teacher, we may perhaps be overestimating the number of teachers who are bright. If Gestalt psychology is worked overtime—"A unit [meaning a teaching unit] will be a whole, or a configuration, or a pattern, in the Gestalt sense," and "The Gestalt view of psychology puts tremendous emphasis on the fact that details have meaning only as parts of a total larger pattern; yet much informational teaching is still done on the isolated basis"—why perhaps the end may justify the means.

Those instructors who feel that the course in Principles of Teaching should serve principally to illustrate the progress in recent years of the science of experimental education will condemn this book. A goodly number of those who feel that such a course should serve primarily to show alike the applications of educational psychology, educational philosophy, and old-fashioned horse sense to the modern classroom situation as it actually exists, will welcome it as an important addition to educational literature.

Alabama Polytechnic Institute EDWARD E. CURETON

Krankheit und Tod im Schicksal bedeutender Menschen. By ADOLF BRAUN. Stuttgart, Ferdinand Enke, 1937. Pp. 104.

Medizinisches aus der Weltliteratur von der Antike bis zur Gegenwart. By ADOLF BRAUN. Stuttgart, Ferdinand Enke, 1937. Pp. 152.

These two books seem to have resulted from the author's endeavor to write the first. The material collected which did not strictly fit into the first, the collector, it seems, then put into the second book. The chapters of the first are constituted by the causes of death, such as general senility, violence, cancer, etc. The material should make good practice reading for a degree candidate who has to fulfil a language requirement. The data presented are interesting. Here are a few examples:

Tolstoy, at the age of 82 years, decided to be a monk. On his way to the monastery, at a railway junction, he got chilled and died of pneumonia. Mendel, age 62 years, died of obesity, from which he suffered many years as the result of heredity aided by the excellence of the chef of his wealthy cloister in Brno, Moravia. Hutten, the poet of the Reformation, knighted by the Emperor Maximilian, after suffering from syphilis during 17 years, died at the age of 35 in exile in Switzerland, leaving nothing but a writing pen and numerous debts. Zola, age 62, was killed by carbon-monoxide, after having closed, on a chilly night in Paris, the windows of his bedroom in order to keep warm. Mozart, age 35, very poor, died from a kidney disease, was buried in a blizzard in a potters field, with no possibility of identifying the place. His wife lived fifty years longer. Poe, age 40, tuberculous, died in delirium tremens. Koerner, famous Prussian poet-patriot-soldier, age 22, was shot 1813 by a captured French officer whom he insulted. Tizian painted two masterpieces at the age of 99 and was killed by the bubonic plague. The corpse of Sterne was stolen from the cemetery and by a mutual friend identified in the dissection room of the anatomy professor in Cambridge who had bought it and had already cut it up.

Large is the author's list of those to whose death a syphilitic infection made a contribution. He mentions among them Erasmus, Cellini, Mirabeau, Donizetti, Lenau, Nietzsche, Smetana, Lenin, Wilson. Many of those thus suffering were mentally active, even extravagantly so, till the very end, in spite of pronounced symptoms of another kind.

The second book has a less unitary aim; and its style is more rambling. Among the famous personalities who are discussed from the standpoint of inner medicine, prevail those of antiquity and of German nationality. But it, too, makes entertaining reading.

Coral Gables, Florida MAX F. MEYER

A Laboratory Manual in General Experimental Psychology. By NORMAN L. MUNN. Revised edition. New York, Prentice-Hall, 1938. Pp. viii, 286.

In this manual the author aims "to acquaint students with the experimental investigation of outstanding current psychological problems." The distribution of the 65 experiments by subject matter may be roughly classified as follows: nervous system and sensory organs, 3; motor and condition responses, 5; sensation and perception, 23; learning and memory, 13; animal, 5; feeling and emotion, 4. There are one or two experiments each on such topics as thinking, personality, aptitude, imagery, suggestion, testimony, and attitudes. Each experiment has an annotated bibliography. In the appendix are suggestions on equipment.

Certain criticisms of the manual suggest themselves. Although the author, in many instances, suggests alternate and less expensive equipment, a large number of the experiments call for apparatus which is not available in most laboratories. Because directions to the student are frequently meager, both as to experimental procedures and treatment of data, a heavy teaching load is placed upon the instructor. Many will question the advisability of basing the discussion questions, which accompany each experiment, principally upon the reference reading rather than upon data collected by the student. Norms or class data for comparison with individual results are not included. This text emphasizes content much more than method and leans heavily upon traditional psychology (*i.e.* much on sensation, perception, memory and little on personality, vocational and applied). The binding is such that the book will not lie open flat upon the table.

The reviewer believes that the most satisfactory laboratory manual is one that emphasizes methodology, reflects to an adequate degree the contemporary trends in psychology, reduces the teaching load to a minimum, requires only simple apparatus, and is handy for the student to use. This manual cannot be rated as entirely adequate on any of these points.

Like most recent texts, this is the work-book type. The list of experiments is sufficiently numerous and varied so that the instructor has a wide range of choice. The book will find favor with those instructors who are able to select a pattern of experiments which is fitted to their laboratory facilities and objectives.

University of Minnesota MILES A. TINKER

Studi Sulle Precognizioni. By LEONE VIVANTE. Florence, Vallechi, 1937. Pp. 218.

Vivante began these studies of the phenomena of precognition because such phenomena appeared to him to contradict a doctrine of originality of thought to which he had always adhered. He wishes, he states, to investigate the genuineness of such phenomena before accepting conviction. He asks: "What roads must be followed in order to explain the phenomena of precognition?" He selects as his criteria the methods and principles of modern science.

A large share of the book is given over to a critical consideration of historical data and case studies of the last fifty years. Vivante draws rather generously from the works of Bozzano, Richet, F. W. H. Myers, and Osty. He reviews the more important theories of those who have published on the subject of precognition and also presents the traditional, popular interpretations of precognition as well as the interpretations of mediums. The final chapter is a discussion of the relationship between normal and abnormal phenomena.

Vivante is guarded in his evaluation, but suggests that phenomena of precognition are but aspects of behavior which have become exaggerated through retrospection; the manifestations are emphasized in disregard of the law of probability.

The book is carefully documented, scholarly in presentation, and worthy of translation. There are, however, some notable omissions. Murchison's *The Case For and Against Psychical Belief* is not cited nor is any reference made to the controversies raised by the Duke experiments in America.

The emphasis, although creditably psychological, is philosophical. Vivante comes to no hard and fast conclusions. He is neither "for" nor "against" the concept of precognition, preferring to urge that more research be conducted.

Rochester, New York ANTHONY J. MITRANO

Over Polariteit in de Zielkunde. By C. A. J. HARTZFELD. Bussum, Van Dishoeck, 1937. Pp. 40.

This really valuable little study considers more generally the problem of polaric conception in psychology as it has become more and more applied in the last 30 years only to mention C. G. Jung, Kretschmer, and Janet. Hartzfeld brings up the question about the value of such kind of psychologies in general and sees it in the differentiation which becomes not so evident in unitaric viewpoints. The "double viewpoint" enlightens not only different members of psychic behaviour but gives too a better opportunity to value the totality and unity of psychic facts and psychic life itself.

Washington, D.C. ERNST HARMS

Durrell-Sullivan Reading Capacity and Achievement Tests. By DONALD D. DURRELL and HELEN BLAIR SULLIVAN. Yonkers-on-Hudson, N.Y., World Book Co., 1937.

These tests, designed for grades 3-6, are constructed on the assumption that reading disabilities are revealed by discrepancies between the understanding of spoken and of printed words. The first test, a Reading Capacity Test, seeks to discover the subject's potential reading ability. In this test the subject's vocabulary is checked by having him, in response to an oral question regarding 'word' meaning, select from a group of eight pictures the one that best answers the question. Following this part of the test, the subject's 'paragraph' meaning is checked by five questions in answer to each of which he is required to select one of three pictures—only one being correct.

The second test, a Reading Achievement Test, measures the subject's vocabulary by a five-item completion-test and his comprehension of 'paragraph' meaning by tests of his ability (1) to understand sentences, (2) to understand words in their context, (3) to note detail, (4) to grasp the central thought, and (5) to locate specific details of information.

By the means of age and grade norms given in the manual, it is possible to determine whether the child's deficiency is due to his lack of understanding of the spoken or printed word, to his lack of vocabulary, or to his lack of any of the five items measured by the reading achievement test.

Spelling tests and written recall tests are given as optional. High reliabilities were obtained by the authors in all the tests.

Ohio State University. ROBERT Y. WALKER

Love, Hate and Reparation. By MELANIE KLEIN and JOAN RIVIERE. London, Hogarth Press, 1937. Pp. vii, 119.

This second volume in the Psychoanalytical Epitome Series presents two lectures given by the authors in a course which described the emotional life of civilized men and women. Many of the ideas and impulses in the adult mind are traced to mental functions unconsciously remaining from infantile and childhood experiences.

The first lecture, by Riviere, is entitled, "Hate, Greed and Aggression." These mental attitudes are destructive, while love is unifying in nature: they stem from the great primary emotional patterns of hunger and biological love respectively. Later in life the projection of these inwardly felt emotions develops into a two-fold interest in other people. The former destructive group, centering around hate, seeks an external outlet for the discharge of our meaner traits, while the latter strives for objects of satisfaction.

The second lecture, on "Love, Guilt and Reparation," by Klein, stresses the contributions which love makes toward a more stable society from the early parental object to the friendships and marital partnership of maturity. Contentment and satisfaction thus gradually supplant the earlier feelings of guilt and resentment connected with frustrations of natural impulses.

The work is interestingly written but would have gained value in this direction if the two lectures had been recast in systemtically condensed form as one comprehensive essay on the subject.

Minneapolis, Minn. CHRISTIAN A. RUCKMICK

Recent Experiments in Psychology. By LELAND W. CRAFTS, THEODORE C. SCHNEIRLA, ELSA E. ROBINSON, and RALPH W. GILBERT. New York, McGraw-Hill Book Co.; 1938. Pp. xiv, 417.

There are twenty-eight chapters in this book and each one is written around one or two clear-cut and, in the majority of instances, crucial experiments. The experiments are intensively treated and particular emphasis is placed on the exact procedure and apparatus employed. Many excellent figures of apparatus clarify verbal descriptions. Although the reader is given an intimate account of the operations performed by psychologists in the laboratory, each chapter also contains a careful statement of the general significance and practical importance of the experiments discussed. The reviewer is impressed by the skillful and dignified manner of exposition.

One could have asked for a wiser selection of topics in only three instances. The chapters on the relation between character traits and handwriting, the judgment of vocational aptitude and achievement from photographs and the representative and expressive effects of music are interesting and well set forth but they do not measure up to the general significance of the remaining twenty-five chapters. These chapters are all so skillfully handled that it is difficult to single out any one or a group for special commendation. The reviewer is gratified by the discussion of the migration of birds and salmon, Honzik's important experiment on sensory control of the maze habit, brain waves, occupational ability patterns, the conditioning of vasomotor responses, and the motor theory of thinking. All of the important concepts in elementary psychology are represented by one or more chapters.

Western Reserve University CALVIN S. HALL

The Abbe du Bos: His Advocacy of the Theory of Climate. By ARMIN HAJMAN KOLLER. Champaign, Illinois, Garrard Press, 1937. Pp. 128.

The theory that climate is responsible for art was a contribution of Abbe du Bos in the early 18th Century. The theory is now revived by Koller who presents the evidence and arguments for it. Even if one were to agree with the author that 'real art' is limited to a few countries of Western Europe (a proposition that many will challenge) it does not follow that the primary cause of that excellence is climate. Koller's conclusions are not based upon a comparison of the climates of Western Europe with other parts of the globe, nor upon a demonstration that climate has any specific physiological or psychological effect upon man. They are for the most part non sequitur.

Washington University JOHN PAUL NAFE

Het Domein Der Godsdienst: Psychologie. By F. J. TH. RUTTEN, Nijmegen, N.V. Centrale Drukkerij, 1937. Pp. 51.

The chairman of the department of psychology of the Roman Catholic University of Nijmegen surveys in this little treatise the most acknowledged theories of psychology of religion of our time as Girgensohn, Schjelderup, Thouless, Janet and C. G. Jung in their consideration of the psychology of worshipping. He distinguishes general, typological and genetic psychology of worshipping. His own is the characteristical Catholic one, nevertheless he maintains that the psychology of worshipping should be outside of the dogmatical confessions.

Washington, D.C. ERNST HARMS

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PAIN: HISTORY AND PRESENT STATUS

By KARL M. DALLENBACH, Cornell University

The doctrine of the five senses, which is common sense today, is ascribed to Aristotle, although it is probable that it was also common sense in his day and generation. However that may be, the doctrine descends to us from antiquity, and its does not include, nor does it make any provision for, a sense of pain. Though Aristotle states that touch, the fifth of the senses enumerated by him, "discriminates several sense-qualities,"[1] pain is explicitly not one of them; for he, like Plato before him, places pain with pleasure among the passions of the soul.

It is not surprising that the Ancients should have set pain apart from the senses, or that they should have considered it as the antithesis of pleasure. Unlike sight, hearing, smell, taste, and touch, pain does not possess an obvious sense-organ; nor is it restricted to any part of or locus in the body, but rather pervades the whole. Moreover, it is not a quality of external objects as the qualities of the five senses were supposed to be; and it does seem to be inherently unpleasant, as most pains (probably more than 99% of them—indeed, a recent writer, Fröbes,[2] says *all*) may thus be characterized. Once pain, identified with unpleasantness, is set in opposition to pleasure, it transcends the limitations of the sensory field and extends into the other fields of mental life.

These considerations, together with the dominant influence of Aristotle's pronouncements upon subsequent philosophical and scientific thought, long delayed the recognition of pain as a sensory quality. The affective classification of pain and the doctrine of the five senses prevailed until comparatively recent times. All through the intervening years, however, some intrepid souls have considered and variously answered the question tacitly raised by Aristotle regarding touch: "Is it a bundle or group of many special senses?" Since pain was turned into a special sense as a result of the attempts to analyze touch, it may be profitable as well as interesting to review the answers that have been given to that question.

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* Presidential address of the Eastern Psychological Association, given at Bryn Mawr College, March 31, 1939.

[1] Aristotle, *Treatise on the Principles of Life,* Eng. trans., W. A. Hammond, 1902, bk. ii, chap. 6; bk. iii, chap. 1.

[2] Joseph Fröbes, *Lehrbuch der experimentellen Psychologie,* 1, 1923, 149.

Aristotle's commentators were divided. Themistius [317-387] adopted the view that touch was a plurality of senses.[3] He held, though he did not specify, that there were as many separate senses in touch as there were different tactile qualities. Alexander, in the 2nd century, Simplicius, in the 6th, and Philoponus, in the 7th, were, however, of the contrary opinion,[4] and, since the 'master' had not definitely spoken, but had merely mooted the point, the contrary opinion that touch was a single sense prevailed for their day and generation.

The question was raised again in the 11th Century by the Schoolmen. Themistius' doctrine, that touch was a plurality of senses,[5] was adopted by Avicenna [980-1038] and Averroes [1126-1198] among the Arabian Schoolmen, and by Appollinaris [12th century], Albertus Magnus [1193-1280], Aegidius [1247-1316], Jandunus [d. 1328], and Marcellus [14th century] among the Latin Schoolmen. They were not, however, in agreement regarding the kind or the number of senses into which it was to be divided. Avicenna, like Themistius, divided it into as many senses as there were tactile qualities. He did not, however, specify their number, though he did distinguish two from touch proper; namely, the sense of pain from a wound and the sense of titillation.[6] Averroes, his compatriot, accepted titillation; but he disagreed regarding pain, and added hunger and thirst.[7] Aegidius increased the number of senses by two: one for temperature (hot and cold) and the other for humidity (dry and moist).[8]

Among the more modern philosophers, Cardano [1501-1576] divided touch into four senses: one for the tactile qualities of hot, cold, wet, and dry; a second for light and heavy; a third for pleasure and pain; and a fourth for titillation.[9] These were denied by Scaliger [1484-1558] who himself distinguished sexual appetite as a sixth sense. Francis Bacon[10] [1561-1626] distinguished sex and so also did Voltaire[11] [1694-1778] and Buffon[12] [1707-1788]. Locke [1632-1704] added hunger and thirst;[13] and Kant, [1724-1804] a vital sense (*sensus vagus*) into which he placed heat, cold, shudder, quake, thrill, hunger and thirst.[14]

Beginning in the 2nd century A.D., the physicians and scientists also tried their hand at analyzing touch. Galen [131-200] thought that in addition to touch proper there was a sense of heat and cold.[15] Pechlini, in 1691, divided the fifth sense into touch, cold, and heat.[16] In the 18th century, Erasmus Darwin, [1731-1802]—who had the misfortune to be the grandfather of Charles Darwin, since otherwise he would have been great in his own right—added seven senses to the traditional five: the sense of heat, of muscular extension, of hunger, of thirst, of suffocation, of sexual appetite, and of lactiferousness.[17]

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[3] Cited by William Hamilton, *Lectures on Metaphysics and Logic,* edited by H. L. Mansel and John Veitch, 2, 1870, 155.

[4] *Idem.* [5] *Idem.* [6] *Idem.*

[7] *Ibid.,* 156. [8] *Ibid.,* 155 f. [9] *Ibid.,* 156.

[10] *Idem.* [11] *Idem.* [12] *Idem.*

[13] *Idem.* [14] *Idem.* [15] *Idem.*

[16] J. N. Pechlini, *Observationum physico-medicarum libri tres,* 3, 1691, 410; quoted from Torsten Thunberg, Physiologie der Druck-, Temperatur- und Schmerzempfindungen, in W. Nagel's *Handbuch der Physiologie des Menschen,* 3, 1904, 648.

In support of his statement that "nature has provided us with a set of nerves for the perception of heat,"[18] Erasmus Darwin cites an interesting observation made on a patient by one of his relatives, Dr. R. W. Darwin. This patient, as a result of fever and violent cramps in his legs, had lost sensitivity to touch in one of his feet. "Though pricked with a pin in five or six places," Darwin reports, "the patient declared he did not feel it in the least, nor was he sensible of a very smart pinch. I then held a red hot poker at some distance, and brought it gradually nearer, till it came within three inches, when he asserted that he felt it quite distinctly. I suppose some violent irritation of the nerves of touch had caused the cramps, and had left them paralytic; while the nerves of heat, having suffered no increase of stimulus, retained their irritability."[19]

Some of Darwin's observations regarding the other senses that he proposed are also worthy of quotation. In reference to the sense of extension, he writes: "the whole set of muscles, whether they are hollow ones, as the heart arteries and intestines, or longitudinal ones attached to bones, . . . may be considered as one organ of sense, and the various attitudes of the body, as ideas belonging to this organ."[20] The sense of hunger, he writes, "is most probably perceived by those numerous ramifications of nerves that are seen about the upper opening of the stomach;" and thirst "by the nerves about the fauces and top of the gula."[21]

At the end of the section on the organs of sense, Darwin adds that "there are many more senses than have been here mentioned . . . which have not acquired the name of senses."[22] Pain, however, was not one of these, for he regarded pain and pleasure as central effects of sensorial motions. Pain results, he says, "whenever the sensorial motions are stronger than usual. . . . A great excess of light . . . of pressure or distension . . . of heat . . . of cold produces pain."[23] He thus anticipates the intensive theory of pain that was introduced several generations later.

About a decade after Darwin, Bell [1774-1842] posited,[24] without reference to earlier writers, a sixth sense—a sense of movement—in addition to the traditional five. This new sense, whose organs are the muscles of the body, was, he believed, "the most important of all . . . for it is by a sense of motion that we know many of the qualities of outward things, as their distance, shape, resistance, and weight[25] . . . hardness, softness, figure, extension, and motion."[26]

Bell also writes of hunger and thirst as sensations. "We are," he states, "solicited to take food by the uneasy sensation of hunger," which is "the effect of the attrition of the sensible coats of the stomach upon each other by the peristaltic motion of the stomach and compression of the viscera."[27] "Thirst is a sensation seated in the tongue, fauces, esophagus, and stomach. It depends on the state of the secretions which bedew these parts and arises either from a deficiency of secretion or from an unusual acrid state of it."[28] He took no account of either of them, however, in his classification of the senses; nor did he specifically mention pain.

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[17] Erasmus Darwin, *Zoonomia*, 1, 1794, 76-90, sec. xiv.

[18] *Ibid.*, 86.

[19] *Ibid.*, 87.

[20] *Ibid.*, 87.

[21] *Ibid.*, 89.

[22] *Ibid.*, 90.

[23] *Ibid.*, 90.

[24] Charles Bell, *Anatomy of the Human Body*, 3, 1803, 224-372; John and Charles Bell, *The Anatomy and Physiology of the Human Body*, 5th Amer. ed., revised by Charles Bell, 2, 1827, 154-290, esp. 158, 290.

[25] John and Charles Bell, *op. cit.*, 158.

[26] *Ibid.*, 290.

[27] *Ibid.*, 326.

Approximately 100 years ago, in 1840, Johannes Müller [1801-1858] presented his theory of the specific energies of nerves. Though this theory was anticipated by Bell, as has frequently been pointd out, it was independently formulated by Müller and its scientific significance was not recognized until Müller had published it. In accordance with the traditional doctrine of the senses, Müller posited five kinds of sensory nerves.[29] The fifth class, the nerves of feeling (*Gefühlsnerven*), yielded, he believed, a number of different sensations: tickle, itch, shudder, pleasure, pain, fatigue, suffocation, warmth, cold, touch, and movement. He did not, however, posit different nerve-specificities for those qualities but attempted to account for them by differences in their mode of arousal and in the state of the organism at the time of their arousal. There is, therefore, as far as the analysis and classification of the senses are concerned, little in Müller beyond Aristotle. Müller is, nevertheless, important to us for two reasons: first, because his doctrine of nerve-specificity effectively destroyed, in the realm of scientific thought at least, the point of view, against which the philosophers have inveighed since Descartes and Locke, that sensory nerves conduct to the brain properties or incorporeal copies of the objects perceived. He showed definitely that that was not the case, as different objects produce the same effect with the same nerves, and the same object produces very different effects with different nerves. He is important to us, secondly, because his doctrine provided a sanction for analysis and a systematic basis for classification. If a given nerve, however affected, gives rise to a sense quality that depends upon the specific character of that nerve, may it not be then that the various nerves giving rise to different qualities of touch or feeling have different specificities? The question raised by Aristotle has now obtained systematic significance.

Before Müller, there was no unifying doctrine to shape analysis nor any accepted principle of classification. The efforts of the various inquirers who have been mentioned were without lasting significance. Their individual insights were merely eddies in the current of thought. The gains made by them were not consolidated. Every author wrote, against the background of the traditional doctrine of the five senses, as if he were proposing something new. Lapses of centuries and differences in language may in part account for the lack of continuity, but even Bell, Darwin's contemporary in England, wrote of the 'muscle sense' as if he were discussing something new. He gave no indication of knowing Darwin's work.

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[28] *Ibid.*, 327.

[29] Johannes Müller, *Handbuch der Physiologie des Menschen,* 2, 1840, 249-502.

The testimony of the ages proved ineffectual in refuting the Aristotelian doctrine of the senses. Negative evidence usually is ineffectual. Doctrines pass from the scientific stage, not because they have been discredited but because they have been superseded—pushed off the stage and replaced by others. What had been lacking all along to consolidate the gains as they were being made was a positive principle; and Müller, though he himself, as we have seen, accepted the traditional doctrine of the five senses, gave us such a principle. His work may consequently be reckoned as the dividing line between the ancient and the modern history of the psychology of the senses.

Under the impetus of the Müllerian doctrine, scientific thought was tremendously quickened. The principle of the dependence of qualitative difference upon specific nerves, though denied by some,[30] was immediately extended by others to the different sense departments, and Müller's "sense of feeling," which is of particular interest to us, was divided into several senses.

For example, in 1844, Natanson divided the sense of feeling into touch, temperature, and resistance.[31] Weber, in 1846, divided it into touch and common sensibility (*Gemeingefühl*).[32] Touch was restricted by Weber to the skin; and common sensibility, which stems from Kant's *"sensus vagus,"* was defined to include all the qualities common to the skin and the other organs and tissues of the body. These two classes were then further divided. "Touch," he wrote, "provides us with two kinds of sensations which are peculiar to it, pressure sensations (*Druckempfindungen*) and temperature sensations (*Temperaturempfindungen*); at the same time the organ of touch and its nerves are so constituted that the same sensations may be differentiated from one another when they are aroused at two different places on the skin. We may, therefore, distinguish the sense of locality, the sense of pressure, and the sense of temperature."[33] These senses were restricted to the skin because he had failed to elicit them from the internal organs and deep tissues exposed in operations and wounds.

The sensations of common sensibility (*Gemeingefühlsempfindungen*) were even more varied in kind. They derive, as he thought, from all parts of the body possessing nerves (the bones, cartilage, tendons, enamel of the teeth, nails, hair, and epidermis being the exceptions); and are characterized by the fact that "we cannot relate them to outer objects."[34] From internal stimuli (such as chemical and nutritive changes in muscles, organs, and blood) derive fatigue, hunger, thirst, nausea, feelings of well-being and of sickness, etc. From external stimuli and the movement of muscles derive pain, sensations of movement, shudder, tickle, itch, sting, etc. The category of *Gemeingefühl* is Weber's scrap basket into which he cast a miscellany of sensory remnants. It is, as Henle [1809-1885] wrote of it, "the sum, the unsorted chaos of sensations from all the sensitive parts of the body, which leads to the consciousness of self."[35]

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[30] Cf. R. H. Lotze, *Allgemeine Pathologie und Therapie,* 1842, 164; also A. W. Volkmann, Von der specifischen Reizbarkeit der Nerven, in Rudolph Wagner's *Handwörterbuch der Physiologie,* 2, 1844, 521-526.

[31] — Natanson, Analyse der Funktionen des Nervensystems, *Arch. f. physiol. Heilkunde,* 3, 1844, 515-535, esp. 517 f.

[32] E. H. Weber, Der Tastsinn und das Gemeingefühl, in Rudolph Wagner's *Handwörterbuch der Physiologie,* 3, 1846, 481-588. Reprinted separately as *Die Lehre von Tastsinne und Gemeingefühl,* 1851.

[33] *Op. cit.,* 511.

Common sensibility, as Weber believed, was finest in the muscles and skin, and pain was its most marked characteristic. Like Darwin, Romberg, Henle, and Volkmann before him,[36] he anticipated the intensive theory of pain, as he wrote that pain "is aroused in all organs of sense by external stimuli which are so strong that they affect not only the ends of the nerves, but also their trunks."[37]

From Weber on down to the present, numerous analyses of the sense of touch have been made and numerous classifications proposed. In all of them, *Gemeingefühl*—or *coenaesthesis,* as Weber also called it—has played an important though varying rôle, serving ever as the classificatory term for the residue of unsorted sensations and feelings.

The divers classifications are, however, no longer of concern to us. The principle of analysis having been accepted, we may now turn to our special interest, pain, and follow its rapid unraveling—and snarling—from the skein of theory.

Several investigators, as we have seen, anticpated the intensive theory of pain. It was first explicitly formulated, however, by Erb, in 1874,[38] who held that pain was not a sensation specific in kind, but one of high degree. Every sensory stimulus, he maintained, was capable of producing pain if it reached sufficient intensity. This theory, as we shall see, was accepted and supported by many writers.

The theory that pain was a separate and distinct sense, first suggested by Avicenna in the 11th century, was intimated for a second time by Lotze in 1852.[39] It was first definitely formulated, however, by Schiff in 1858, following his analgesic experiments on animals.[40] Noting the effects of various incisions in the spinal cord, he found that pain and touch were independent. When he cut thrugh the gray matter, pain could not be aroused below the level of the incision while touch was left intact; when he cut through the white matter, but not the gray, touch was lost but pain remained. The results of these vivisections were immediately corroborated by clinical evidence, as physicians reported numerous pathological cases of diseased or injured spinal cords with similar sensory defects.[41] Cases of tabes dorsalis with a marked differentiation in the time of arousal of touch and pain—the pressure from the jab of a needle preceding the pain by several seconds—were also placed in evidence.

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[34] *Ibid.,* 495.

[35] Henle, *Allgemeine Anatomie,* 1841, 728; quoted from Weber, *op. cit.,* 563.

[36] Volkmann, *op. cit.,* 520.

[37] Weber, *op. cit.,* 495.

[38] Erb, *Krankheiten der peripherischen cerebrospinalen Nerven,* 1874. Quoted from G. W. A. Luckey, Some recent studies of pain, this JOURNAL, 7, 1895, 109.

[39] R. H. Lotze, *Medicinische Psychologie,* 1872, 247.

[40] Schiff, *Lehrbuch der Physiologie,* 1, 1858, 228. (Cf. O. Funke, Der Tastsinn und die Gemeingefühle, in L. Hermann's *Handbuch der Physiologie der Sinnesorgane,* 3, (2), 1879, 297.

This theory of pain, which may be designated as the sensory theory, was reaffirmed by Funke in 1879 after a thorough examination of the evidence pro and con.[42] It did not, however, come to full bloom until after the classical experiments of Blix and Goldscheider in the early '80s. Blix,[43] in 1882, and Goldscheider,[44] in 1884,—in experiments undertaken to extend the Müllerian doctrine of specific nerve-energies to touch —independently discovered in the skin separate spots for warmth, cold, pressure, and pain, which—no matter how stimulated—yielded, if stimulated, their own specific quality. These results were accepted by many as conclusive evidence for the theory that pain is a separate department of sense.

The intensive theory and the sensory theory stood in opposition to one another, and they both stood in opposition to the traditional pleasure-pain theory which represented pain as an affective *quale*. In the decade between 1886-1896, these different opinions clashed and a three-cornered controversy ensued, the like of which has never before, nor since, appeared in the scientific literature. The proponents of the contending views had to defend themselves from two directions and at the same time lead a divided attack. The controversy was intense; no quarter was asked and none given. Writers were dogmatic and caustic, offering the apology that this manner "made for brevity and explicitness of exposition."[45]

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[41] Cf. O. Funke, *op. cit.*, 299.

[42] Funke, *op. cit.*, 289-309.

[43] Magnus Blix, Experimentelle Beiträge zur Losung der Frage über die specifische Energie der Hautnerven, *Zsch. f. Biol.*, 20, 1884, 141 ff., 160 ff. This was a translation of a paper read in Sweden in November 1882 and first published in *Upsala läkarefören. förhandl.*, 18, 1883, 87 ff.

[44] Alfred Goldscheider, Die specifische Energie der Temperaturnerven, *Monatshft. f. prak. Derm.*, 3, 1884, 198-208, 225-241; Die specifische Energie der Gefühlsnerven der Haut, *ibid.*, 283; Die specifischen Functionen der Nerven der Haut, *Proc. 8th Internat. Cong. Kopenhagen*, 3, 1884, 25-27, also *Vtjsch. f. Derm., Wien*, 11, 1884, 313-316; Nachtrag zu den Mittheilungen über die specifischen Energieen der Hautnerven, *Monatshft. f. prac. Derm.*, 4, 1885, 5; Ueber Wärme-, Kälte- und Druckpunkte, *Verhandl. d. Physiol. Gesellsch., Berlin*, 1885, 000. All of these studies may be found in Goldscheider's *Gesammelte Abhandlungen*, 1, 1898, 53-107.

The traditional point of view, that pain was an affective *quale,* was supported by the philosophers and by psychologists of philosophical bent: notably by Hurwicz, Lehmann, and Wundt in Germany; by Bain, Bradley, Spencer, Sully, Stout, and Ward in England; and by Baldwin, Dewey, James, and Marshall in America. Of all of these, the American, Henry Rutgers Marshall, was the most active and articulate. He wrote one book and numerous articles in behalf of the theory[46] and though, as it turned out, he fought a losing battle, he fought determinedly and vigorously. The "weight of authority" was with him, as he himself pointed out; but that proved of slight advantage as the authorities were widely divided among themselves. There was not *one* pleasure-pain theory but many; indeed, there were almost as many different theories as there were writers on the subject; and, as a rule, the theories proposed were satisfactory only to the men proposing them. Concerted effort and the maintenance of a common front were, under those conditions, impossible. It was almost a case, among the pleasure-pain theorists, of every man for himself. Though Marshall did yeoman's service in behalf of the traditional point of view, the particular theory that he advocated was as unsatisfactory to most of the members of his own camp as it was to his opponents in the other two camps. The internecine quarrels among the pleasure-pain theorists probably did more to advance the theories of the opposition than all their criticisms did to check them.

The situation was very different in the opposing camps. The intensive and sensory theories were supported by physiologists and by younger psychologists with psychophysical interests. These men were untrammeled by tradition; they put their trust in experiment and observation, and followed where their results led, even though this meant, as it frequently did, change of allegiance. For example, when Blix discovered the pain spots in 1882, he was inclined to believe that pain was specific; but by 1885 later experiments moved him to conclude that it was not. "There are," he writes in his second study, "three specific kinds of nerve apparatus in the skin, one for warmth, one for cold, and one for pressure. No specific organs for the sense of pain have been proved to be in the skin."[47] Goldscheider also shifted front. At first he did not believe that pain was specific; but by 1885, just when Blix was shifting in the other direction, he came to the conclusion that the evidence did speak in favor of specificity.[48] He held to that view until about 1891,[49] when he shifted once more—this time to the theory, which he fully developed in 1894,[50] that pain was mediated by the tactile nerves and resulted from an intensive summation of their excitations. He turned to that theory because of an experiment of Naunyn's,[51] in 1889, in which pain was aroused through the summation of weak stimuli rapidly applied. Naunyn found, in cases of tabes dorsalis, that a mechanical stimulus (a hair for example) that was below the threshold for touch or pain, when applied repeatedly from 60-600 times a second, yielded in a few seconds (from 6-20) a pain which soon became unbearable. Since he obtained similar results with other stimuli (electrical for example), he concluded that pain was the result of summation. This conclusion and Goldscheider's extension of it must be counted as variants of the intensive theory. According to that theory as originally formulated, pain arises from the intense stimulation of any sensory organ. Bright lights, loud sounds, strong smells and tastes, severe pressures, and extremes of temperature, all, it was thought, arouse pain if they are intense enough. The ultimate extension of this doctrine is exemplified in the writings of Külpe and Titchener. Külpe wrote in his *Grundriss* in 1893: "pain has been held to be a special quality of cutaneous sensation, and pain nerves put alongside of the nerves subserving heat, cold, and pressure. The hypothesis has found its chief support among physiologists. We cannot accept it, as it stands: for pain is produced in all cases where the stimulation of a sensory nerve passes a certain limit of intensity."[52] Titchener wrote in the first edition of his *Outline* in 1896:

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[45] Herbert Nichols, The origin of pleasure and pain, *Philos. Rev.,* 1, 1892, 404.

[46] H. R. Marshall, The classification of pleasure and pain, *Mind,* 14, 1889, 511-536; The physical basis of pleasure and pain, *ibid.,* 16, 1891, 327-354, 470-497; Pleasure-pain and sensation, *Philos. Rev.,* 1, 1892, 625-648; *Pain, Pleasure, and Aesthetics,* 1894, 1-364; Pleasure-pain, *Mind,* 19, 1894, 533-535; Are there special nerves for pain?, *J. Nerv. & Ment. Dis.,* 21, 1894, 71-84; Pleasure and pain, *Proc. Amer. Psychol. Ass.,* 1894, 24; Pleasure-pain and emotion, *Psychol. Rev.,* 2, 1895, 57-64; Emotion versus pleasure-pain, *ibid.,* 166-168; *Mind,* 4, 1895, 180-194; Physical pain, *Psychol. Rev.,* 329-347.

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[47] Blix, *op. cit., Zsch. f. Biol.,* 21, 1885, 160 ff.

[48] Goldscheider, Neue Thatsachen über die Hautsinnesnerven, *Arch. f. Anat. u. Physiol.,* 1885, Physiol. Abth., Suppl. Bd., 1885, 1-110 (*Gesammelte Abhandlungen,* 1, 1898, 197 ff.).

[49] Goldscheider, Ueber die Summation von Hautreizen, *Arch. f. Physiol.,* 1891, 164-169 (*Gesammelte Abhandlungen,* 1, 1898, 397-432).

[50] Goldscheider, *Ueber den Schmerz in physiologischer und klinischer Hinsicht,* 1894, 1-66.

[51] B. Naunyn, Ueber die Auslösung von Schmerzempfindung durch Summation sich zeitlich folgender sensibler Erregungen, *Arch. f. exper. Pathol. u. Pharm.,* 25, 1889, 272-305.

[52] Oswald Külpe, *Outlines of Psychology,* 1893; Eng. trans., E. B. Titchener, 1905, 90.

"excessive stimulation of any sense-organ, or direct injury to any sensory nerve, occasions the common sensation of pain."[53]

The sensory theory, that pain is a sensation mediated by specific nerves, found its chief support, as Külpe remarked, among the physiologists. Blix and Goldscheider were among the pioneers holding this view. Though they later shifted position, as we have seen, their places were taken by others—notable among whom was Max von Frey. This theory, it should also be noted, received almost unanimously the support of the clinicians, the most prominent among them being S. Wier Mitchell,[54] in America, and Henry Head,[55] in England. Psychologists were long in joining this camp. The first and for a time the only psychologist to uphold this point of view was Herbert Nichols[56]—a former student of G. Stanley Hall and a man who had come to psychology with training in engineering and sixteen years of successful service with the Pennsylvania Railroad.[57] Unprejudiced and not indoctrinated with psychological lore, he was possessed of sufficient self-confidence to espouse and to take a leading rôle in a minority movement. He opined, that "the prejudice of science . . . ran in favor of specific end-organs,"[58] and, as it turned out, he was right; for, when Head published his clinical observations[59] and Von Frey his experimental conclusions[60] correlating warmth with Ruffini cylinders, cold with Krause end-bulbs, pressure with hair follicles and Meissner corpuscles, and pain with free nerve endings,[61] psychologists moved over to the sensory view almost in a body. Here was something definite to teach, and textbooks changed over night. For example: Titchener, who had written in 1896 from the point of view of the intensive theory, wrote in his *Primer* in 1898 "of the sensation of pain,"[62] and placed it alongside of the sensations of pressure, heat and cold; and in his *Instructor's Manual* he said: "investigation of the cutaneous sensations has moved so rapidly during the past five years that there is no adequate account of them to be found in the textbooks."[63]

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[53] E. B. Titchener, *An Outline of Psychology,* 1896, 65.

[54] S. W. Mitchell, Precision in the treatment of chronic diseases, *Med. Rec.,* 42, 1892, 721-726; Post-hemiplegic pain, *Med. News,* 62, 1893, 421-423, Wrong reference of sensations of pain, *ibid.,* 66, 1895, 281 f.

[55] Henry Head, On disturbances of sensation with especial reference to the pain of visceral disease, *Brain,* 16, 1893, 1-133; 17, 1894, 339-480; 19, 1896, 153-276.

[56] Herbert Nichols, The origin of pleasure and pain, *Philos. Rev.,* 1, 1892, 402-432, 518-534; Pain nerves, *Psychol. Rev.,* 2, 1895, 487-490; The feelings, *Philos. Rev.,* 4, 1895, 506-530; Pain nerves, *Psychol. Rev.,* 3, 1896, 309-313.

[57] K. M. Dallenbach, Herbert Nichols: 1852-1936, this JOURNAL, 49, 1936, 320-321.

[58] Nichols, Pain nerves, *Psychol. Rev.* 3, 1896, 311.

[59] Head, *opp. citt.*

[60] Max von Frey, *Die Gefühle und ihr Verhältnis zu den Empfindungen,* 1894, 1-24; Beiträge zur Physiologie des Schmerzsinnes, *Ber. ü. d. Verhandl. d. k. sächs. Ges. d. Wiss. z. Leipzig,* math.-phys, Kl., 46, 1894, 185-196, 288-296; Beiträge zur Sinnesphysiologie der Haut, *ibid.,* 47, 1895, 166-184; 48, 1896. 462-468; Untersuchungen über die Sinnesfunctionen der menschlichen Haut: I. Druckempfindungen und Schmerz, *ibid.,* 49, 1897, 169-266.

[61] *Op. cit., ibid.,* 47, 1895, 181-184.

[62] Titchener, *A Primer of Psychology,* 1898, 45.

Evidence for the sensory theory extant in 1896 was derived from many sources.

(1) A large share was drawn from pathology, as clinical evidence indicated that the skin possessed four distinct forms of sensibility: warmth, cold, pressure, and pain; and that any one or any combination of them may be lost or impaired without involving the others.

(2) Experiments with intense stimuli also showed that pain was specific; for, contrary to the views of the intensive theorists, excessive stimulation of the special senses does not yield pain when the effect is limited to the organ concerned. For example: if the retina is stimulated, however intensely, it yields not pain but light. If proximal regions are stimulated simultaneously, pain results, but only because those extra-retinal regions are themselves capable of yielding pain. As with vision so also with audition, smell and taste. Excessive stimulation of any of the special senses does not yield pain unless the effects involve tissues that are themselves supplied with pain nerves. Even the warm and cold spots were found to be specific. No matter how excessive their stimulation—whether they are prodded with a probe, pricked with a needle, or burned with a red hot wire—they yield, when stimulation is restricted to the proper area, not pain, but their own specific quality.

(3) The long latent period, the comparatively slow rise of pain after stimulation, which is exaggerated in cases of tabes dorsales, was set in evidence of specificity.

(4) So also were the results obtained with anesthetics. Under light chloroform anesthesis, the patient is sensitive to the slightest touch, but to pain he is totally insensitive. Again, if an ice pack is applied to the elbow, pressure, warm, and cold, and finally pain lapse from the lower arm in that order. Similar results were obtained with cocaine and menthol. These findings, that pain disappears under the influence of anesthetics while the other senses remain and that it remains while the other senses disappear, spoke for specificity.

(5) Mapping the distribution of pain spots on the body with punctiform stimulation and graded esthesiometers.

(6) Liminal determinations of pain in different areas.

(7) The uniqueness of certain areas in yielding pain alone (*e.g.* the cornea of the eye) and the insensitivity of certain other areas (*e.g.* Kiesow's area, the mucous membrane of the cheek opposite the second lower molar).

The evidence for the sensory theory in hand in 1896 was impressive. It convinced many, but it did not place the theory in the realm of fact. There were still many unsolved problems. Outstanding among them were: (1) the definite identification of the pain-receptors; (2) the establishment of the peripheral pathways; (3) the localization of the cerebral centers; (4) the interpretation of the non-adaptability of pain; and (5) the explanation of its unique relationship with unpleasantness.

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[63] Titchener, *Experimental Psychology: I. Qualitative Experiments: ii. Instructor's Manual,* 1901, 81.

Solutions of these problems were, to be sure, proposed in the '90s, but none were final. For example, Von Frey thought that the free nerve-endings were the organs of pain; but, his direct experimental evidence being meager, he relied upon circumstantial evidence. The free endings are the only nerve structures widely scattered throughout the body and existing in sufficient profusion in the skin to correlate with the numerous pain-spots localized upon the surface. Again, the non-adaptability of pain was explained biologically. Pain is deleterious; adaptation would be of anti-survival value, as organisms that become adapted to pain would, in the long run, not survive; hence pain is non-adaptable. A false explanation, to be sure, but one that satisfied the exigencies of the occasion.[64]

The sensory theory was widely accepted. When Sherrington[65] in Schäfer's *Textbook* and Thunberg[66] in Nagel's *Handbuch* wrote of pain as a specific sensation coördinate with the other cutaneous sensations, the theory was 'in,' and it was presented and taught in most textbooks of physiology and psychology as established fact. The other theories were eclipsed. The traditional pleasure-pain theory underwent a complete reorganization. After vain attempts to discover nerves of pleasure—Nichols, for example, searched for them throughout the body and finally decided that they must be in the organs of sex and alimentation, the two systems vitally concerned with the preservation of the species[67]—the theory was placed in a new perspective. Pain was removed as a term in the dichotomy, unpleasantness was substituted, and the dichotomy was changed to one of pleasantness-unpleasantness and then relegated to the affective sphere where it would no longer be of concern to those chiefly interested in sensation.

The intensive theory in its more general form was definitely disproved. Pain positively is not a common sense-quality that is aroused by excessive stimulation in all sensory nerves. The pains of dazzling lights, of shrill tones and intense noises, of pungent tastes and smells, of extremes of cold and heat, are mediated by the co-excitation of other nerves than those of the specific senses. So much we may accept as established fact.

Goldscheider's modification of the intensive theory was, however, untouched by these considerations. It was, nevertheless, at the textbook level at least, completely ignored; it was put in the discard along with the general form of the intensive theory with which it was incorrectly identified. At the frontiers of research, on the other hand, the theory was very much alive. There it took its place as a theory contending on equal footing with Von Frey's; and many have been the studies undertaken to decide between them!

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[64] Cf. also G. W. A. Luckey, Some recent studies of pain, this JOURNAL, 7, 1895, 108-123; W. von Tschisch, Der Schmerz, *Zsch. f. Psychol.*, 26, 1901, 14-32.

[65] C. S. Sherrington, Cutaneous sensations, in E. A. Schäfer's *Textbook of Physiology*, 2, 1900, 920-1001.

[66] Torsten Thunberg, Physiologie der Druck-, Temperatur- und Schmerzempfindungen, in W. Nagel's *Handbuch der Physiologie des Menschen*, 3, 1905, 647-733.

[67] Nichols, *op. cit.*, *Philos. Rev.*, 1, 1892, 414.

Goldscheider partly prejudiced the issue by his adherence to the term *Gemeingefühl,* which is the context into which he set his theory. It is, nevertheless, clear from his treatment that he, like Von Frey, regards pain as a specific sensation subserved by a special class of nerves. From there on, however, their theories diverge. Goldscheider holds that pain is mediated by the tactile nerves and that it results from the summation of their excitations in the gray substance of the spinal cord. Von Frey, as we have seen, holds that pain is unique, that it is a separate modality subserved by discrete receptoral structures—the free nerve-endings. In writing of these theories, Kiesow says: "the differences between them goes so deep that a reconciliation is hardly conceivable . . . if one view is correct, the other must necessarily be false. Consequently, we cannot expect the conflict to end until one of the views attains general recognition and so flict to end until one of the views attains general recognition and so vanquishes the other."[68] But how to set the experimental stage so that the contending theories could meet? That question was long in answering.

As we have seen, pain, for biological reasons, was not supposed to show adaptation.[69] This point of view was held for many years. It was not until comparatively recent times (1919) that Straus and Uhlmann discovered in the Cornell laboratory that pain *did* show the phenomenon of adaptation.[70] One of the outstanding problems regarding pain, *i.e.* the explanation of its none-adaptability, was solved, and pain, as far as adaptation is concerned, stepped into line with the other cutaneous senses. All that is required for its adaptation is a relatively constant, unvarying stimulus—the very conditions necessary for adaptation in all the other departments of sense. When this result was obtained, the reason that pain had heretofore been regarded as non-adaptable immediately became clear. The stimulus-conditions in injuries, tooth-aches, and headaches are constantly varying.

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[68] Friedrich Kiesow, The problem of the condition of arousal of the pure sensation of cutaneous pain, *J. Gen. Psychol.,* 1, 1928, 199-212.

[69] Titchener (A Textbook of Psychology, 1909, 154) says, in 1909, that "the pain sense does not appear to show the phenomenon of adaptation;" and Fröbes (*op. cit.,* 151) in 1923, writes, "der Schmerz zeigt keine Adaptation," although numerous experiments had by then been reported showing the contrary.

[70] H. H. Straus and F. R. Uhlmann, Adaptation of superficial pain, this JOURNAL, 30, 1919, 422-424.

Straus and Uhlmann were content to rest their study at that point, but the experiment, as it turned out, was destined to play a larger rôle. As an *experimentum crucis,* it led to a decision between Von Frey's and Goldscheider's theories. If Von Frey's sensory theory were correct, pain should, in the course of its adaptation, simply become progressively weaker and disappear without undergoing any qualitative change. If Goldscheider's theory, on the other hand, were correct, then, as pain becomes less intensive through adaptation, it should also undergo a qualitative change, *i.e.* it should change into pressure. Goldscheider,[71] accepting the logic of the adaptation-experiment, claimed that the excitation of the pain-spots arose and disappeared through subpainful experiences ("die Erregung der Schmerzpunkte unterschmerzlich beginnt und unterschmerzlich aufhört").[72] He found, in his studies of its adaptation, that pain not only became weaker under continuous stimulation but also that it underwent a qualitative change, *i.e.* before completely adapting and disappearing, it passed into a subpainful sensation (*unterschmerzliche Empfindung*) of contact (*Berührung*) or pressure (*Druck*).

In an experiment on pain[73] in 1922, Von Frey tested Goldscheider's subpainful sensations. In order to avoid the concomitant pressures that are aroused when an object is brought to bear upon the skin, Von Frey elicited pain by focusing the sun's rays upon the skin by means of a hand lens. Because he was able to obtain pure pressureless pains by this as well as by other means, he concluded that Goldscheider's summation theory was untenable.

In refutation, Goldscheider, in 1926, pointed out that Von Frey had used strong stimuli, and that the course of experience, under those circumstances, is telescoped and the subpainful pressures are obscured by the intense pains.[74] He laid down two necessary conditions for the subpainful pressures: first, the pain should not be intense; and secondly, it should be produced slowly, not quickly or suddenly. His conclusion was that "the doctrine of specific peripheral nerves of pain is unwarranted."[75] So the situation was back again to where it was before! Opinion set against opinion, and observation against observation.

In 1931, Wells and Hoisington reported a phenomenological study of pain-adaptation which, they thought, settled the controversy by reconciling the disparate points of view.[76] They found that the end-quality in the course of pain-adaptation

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[71] Goldscheider, *Das Schmerzproblem,* 1920, 20, 71-73.

[72] *Op. cit.,* 20.

[73] Von Frey, Versuche über schmerzerregende Reize, *Zsch. f. Biol.,* 76, 1922, 1-24.

[74] Goldscheider, Beiträge zur Physiologie der Gemeingefühle, *Zsch. f. Sinnesphysiol.,* 57, 1926, 1-14.

[75] *Ibid.,* 14.

"is neither a weak pain (Von Frey) nor a pressure (Goldscheider), but a non-pressury 'bright contact'."[77] Upon the basis of this finding, they suggested that the difference between Von Frey and Goldscheider was principally terminological. Though recognizing that "interpretation and comparison of qualitative terms is always dangerous," they, nevertheless, ventured the opinion that Von Frey's "weak pains" and Goldscheider's "subpainful pressures," which those investigators obtained just before complete adaptation was reached, corresponded to their own "bright contacts." If such is the case—"if Von Frey's insistence that isolated pain spots give nothing but pain is . . . due to a confusion of 'weak pain' with non-painful 'bright contact'," and "if Goldscheider's contention that isolated pain spots give 'pressure' is the result of a failure to differentiate between true pressure and the non-pressury contact-like experience"—then, they conclude, "the controversy will be settled." Yes, settled by the elimination of Von Frey! For Von Frey to admit "that the final phase of pain subsidence is not really painful"—as Wells and Hoisington would have him do—would be for him to accept Goldscheider's theory. Wells and Hoisington reconcile the controversy by corroborating Goldscheider!

It was at this point that I undertook, in coöperation with several of my students, a series of studies on the problem. The first study (with Burns) was performed, following Goldscheider, with needle esthesiometers.[78] We found, in confirmation of Goldscheider, that pain was replaced by pressure in every instance before complete adaptation was secured. The normal and typical course of adaptation was "a gradual subsidence from a maximal pain through pressure to indifference."[79] At which point or points in the curve, however, was complete adaptation attained? When pain disappeared and pressure emerged? Or only when pressure disappeared? Or have we here two cases of complete adaptation: one of pain, and another of pressure? Our answers to these questions will depend upon our point of view, but it should be recalled that our stimuli—needle esthesiometers—were applied at superliminal pressure values; they were, therefore, capable of eliciting pressure as well as pain. The pressures reported after the subsidence of pain may very well be nothing more or less than the residual effect of our own stimuli. The only way of deciding the matter was to arouse pain with stimuli, like radiant heat, that do not give rise simultaneously to pressure.

We therefore devised, in the second study (with Stone),[80] a thermo-esthesiometer that yielded pain in conformity with the strict conditions laid down by Goldscheider for the observation of his subpainful pressures.

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[76] E. F. Wells and L. B. Hoisington, Pain adaptation: A contribution to the Von Frey-Goldscheider controversy, *J. Gen. Psychol.*, 5, 1931, 352-366.

[77] *Ibid.*, 364.

[78] Maryland Burns and K. M. Dallenbach, The adaptation of cutaneous pain, this JOURNAL, 45, 1933, 111-117.

[79] *Ibid.*, 115.

[80] L. J. Stone and K. M. Dallenbach, Adaptation to the pain of radiant heat, *ibid.*. 46, 1934, 229-242.

Goldscheider said that there should be no telescoping of experiences and, as the typical reports of our observers showed, there was none. Warmth, the first quality elicited, passed slowly through intensive gradations to heat; this, in turn, passed through intensive gradations to weak pain which grew in turn in intensity, remained constant at a high degree for a short time, and then gradually declined, and disappeared into heat, which in turn subsided into warmth. Pressure was noticeable by its absence. This result might lend support to a new theory that pain is a summation of warmth; but we preferred the explanation that the accompaniments of pain, whether pressure or warmth, are functions of the type of stimuli employed. Since a pressure stimulus yields residual pressure on its way to complete adaptation, and a warmth stimulus residual warmth, we hazarded the opinion that a cold stimulus would yield residual cold—and thus set the problem for our third study.

This study (with Edes) yielded the expected results.[81] A dry-ice stimulator elicited pain without telescoping the experience. From slowly increasing degrees of cool and cold, pain emerged gradually, grew in intensity, remained at a high degree for a time, and then gradually disappeared, subsiding into decreasing degrees of cold and cool. At no time during the course of adaptation to the cold stimulus was pressure reported by our observers.

Did our observers, in the two studies with temperature, miss Goldscheider's pressures? Were those pressures so weak that they escaped notice? We think not. Our *O*s were highly trained in cutaneous observation; it is hardly possible that all of them would have failed all the time to report those qualities if they occurred, particularly as our experimental conditions fulfilled all the requirements that Goldscheider laid down and should, therefore, have been highly favorable for their observation.

All the pain-adaptation studies reveal, as residual effects, the qualities that are inherent in the stimuli. A pressure stimulus yields residual pressure; a warmth stimulus, residual warmth; and a cold stimulus, residual cold. Surely, these experiments justify us in concluding with Von Frey that the nerves of pain are separate and distinct from the tactile nerves, and that Goldscheider's summation theory is untenable. This conclusion is corroborated by experiments during the past decade in the field of neurophysiology, demonstrating that pain is subserved by specific nerve fibers.[82]

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[81] Barbara Edes and K. M. Dallenbach, The adaptation of pain aroused by cold, *ibid.*, 48, 1936, 307-315.

[82] E. D. Adrian, *The Mechanism of Nervous Action*, 1932, 42-60.

H. S. Gasser and J. Erlanger, The rôle of fiber size in the establishment of a nerve block by pressure or cocaine, *Amer. J. Physiol.*, 88, 1929, 581-591.

Though the investigators disagree among themselves regarding details, they agree in general that separate nerve fibers subserve the function of pain.[83] Further support for this point of view is provided by the experiments in sensory chronaxy which demonstrate significant and reliable differences in the chronaxies and in the strength-duration curves of pressure and pain.[84] Thus we see that Nichols was right when he opined that the prejudice of science ran in favor of specificity.

Of the five outstanding problems at the turn of the century, I have touched upon two. One of these, as we have seen, has definitely been solved. The discovery that pain was adaptable removed the explanation of its non-adaptability from the list of problems. The other, the establishment of the pathways of pain, is well on the way to solution, if, indeed, it is not settled now. Of the three problems that I neglected, only one is nearer solution now than it was a third of a century ago. The identification of the pain-receptors is bound up with the establishment of the structural pathways, but the evidence for the free nerve-ending is only a little more convincing now than it was when Von Frey formulated his theory. The other two problems, the localization of the cerebral centers, and the explanation of pain's unique relationship with unpleasantness, are practically untouched. We still are, as Waterston recently observed, "entirely ignorant of any 'cortical area' associated with pain perception, nor do we know whether such an area exists or whether the thalamus alone . . . is sufficient for the pain sensation."[85]

The uneven advance since the turn of the century characterizes the history of the subject from the beginning.

P. Heinbecker and G. H. Bishop, The mechanism of painful sensations, *Proc. Ass. Res. Nerv. & Ment. Dis.*, 15, 1935, 226-238.

P. Heinbecker, G. H. Bishop, and J. O'Leary, Fibers in mixed nerves and their dorsal roots responsible for pain, *Proc. Soc. Exp. Biol. & Med.*, 29, 1932, 928-938; Analysis of sensation in terms of the nerve impulse, *Arch. Neurol. & Psychiat.*, 31, 1934, 34-53.

S. W. Ranson, Cutaneous sensory fibers and sensory conduction, *Arch. Neurol. & Psychiat.*, 26, 1913, 1122 f. 1 Cutaneous sensation, *Science*, 78, 1933, 397 f.

S. W. Ranson and H. K. Davenport, Sensory unmyelinated fibers in the spinal nerves, *Amer. J. Anat.*, 48, 1931, 331-353.

David Waterston, Observation on sensation: The sensory functions of the skin for touch and pain, *J. Physiol.*, 77, 1933, 251-257; On pain, *Lancet*, 224, 1933, 943-946; Pain and the mechanism of its production, *Brit. Med. J.*, (no. 3857), 1934, 1087-1089.

[83] The outstanding exceptions are Heinbecker, Bishop, and O'Leary (*opp. citt.*). These authors report results which agree with Goldscheider's theory.

[84] Cf. W. S. Neff and K. M. Dallenbach, The chronaxy of pressure and pain, this JOURNAL, 48, 1936, 632-637, and the references cited by them.

[85] Waterston, *op. cit.*, *Lancet*, 224, 1933, 943.

THE SPECTRAL COMPONENTS OF PSYCHOLOGICALLY UNIQUE RED

By FORREST L. DIMMICK and MARGARET R. HUBBARD, Hobart College

The determination of the spectral location of the seven 'psychologically unique' colors is important if the four-variable color equation is to become a valid means of description.[1] That is, the color equation acquires exact meaning only when the points of reference have been precisely standardized. In a previous paper we have discussed this question at length, and the standards for yellow, green, blue, black, white, and gray have been reported.[2] There remains, therefore, the definition of the remaining 'unique,' red. The majority of investigators who have studied the correlation of wave-length with hue have agreed that all spectral reds are yellowish. In the preceding experiment on the spectral location of the 'psychologically unique' colors we verified this observation.[3] Because the apparatus used in that experiment—a wavelength spectrometer—was suitable for work only with single wavelength bands, the present study was undertaken to determine experimentally the amount of 'unique' blue (476 mμ) that must be added to spectral red (635 mμ) to obtain the 'psychologically unique' red, *i.e.* a red which is neither bluish nor yellowish.

METHOD AND PROCEDURE

Apparatus. A colorimeter, designed and built in the Hobart laboratory for the purpose of obtaining well saturated colors by combining filtered light, proved satisfactory for our problem. (See Fig. 1) It consists of three optical systems A, B, C, all of which obtain light from a single source, S. The three systems converge and are focused upon a viewing screen coated with MgO. Light from the source falls directly, or is reflected by first-surface mirrors (M), upon three windows covered

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[1] F. L. Dimmick, A reinterpretation of the color pyramid, *Psych. Rev.,* 36, 1929, 83-90.

[2] F. L. Dimmick and Margaret R. Hubbard, An experimental determination of the spectral location of the psychologically unique yellow, green and blue, this JOURNAL, 52, 1939, 242-254.

Our attention has been called by Dr. Elsie Murray to an article not included in our previous review of the literature, in which an experimental determination of the yellow region is reported. J. G. Hailwood and H. E. Roaf (The sensation of yellow and anomalous trichromatism, *J. Physiol.* 91, 1937, 36-47) using a method of adjustment, determined the position in the spectrum of the points at which yellow becomes noticeably orangish and greenish. Their mid-yellow value is 583 mμ, which agrees with our results. Their range of variation is larger than ours, a fact which is undoubtedly a function of the difference in method.

[3] *Ibid.*

with fine grain ground glass (G), the image of which appears evenly illuminated on the MgO screen. The arrangement of lenses, (L), is such that the introduction of a variable diaphragm, (D), into the filter compartment, (F), between them, cuts down the illumination of the image on the viewing screen evenly over its whole area without altering the focus.

The observer views the central portion of the MgO screen through two reducing apertures (V_2V_3) by way of a first surface mirror (M_2) and a third aperture (V_1) which controls the line of vision. The field surrounding aperture V_2 is a middle gray whose reflectance is 0.200 which gives 5 foot lamberts brightness at an illumination of 25 f.c.;[4] those of V_1 and V_3 are black. The source of illumination (S) is a 300 w. T-10 projection bulb with a coiled coil filament rated at 115 v. and operated at 110 v. to give a color temperature of 3000° K. The lamp housing is cooled with a blast of air through an opening in the base (x).

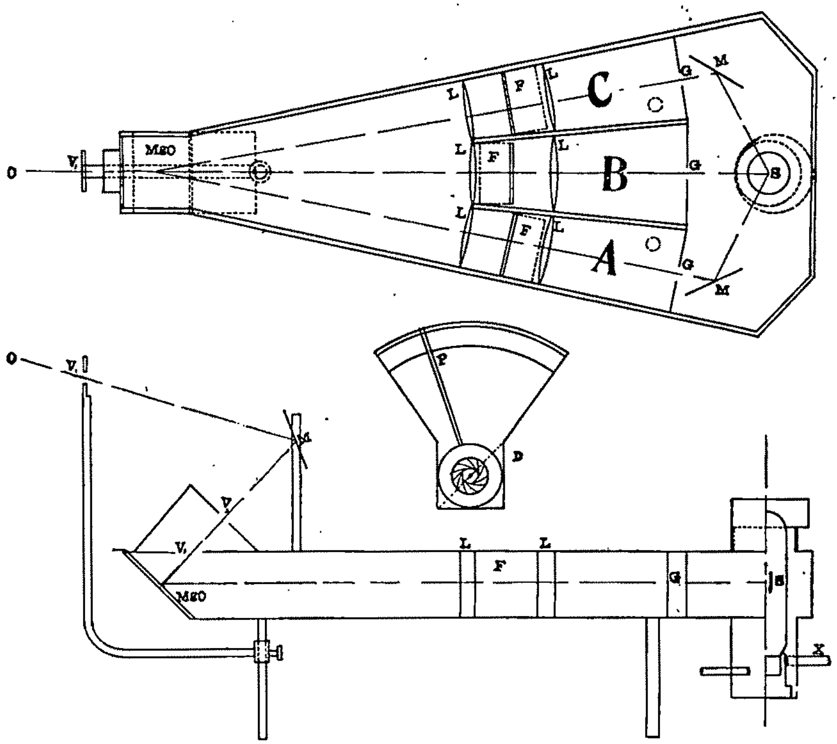

FIG. 1 HOBART COLORIMETER

The iris diaphragms (D) are calibrated in foot candles of illumination falling upon the MgO screen for every setting of the pointer (P). Since the apparent reflectance of the MgO screen is 1.00,[5] the brightness of the screen as viewed from O may be read in foot lamberts.

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[4] Dimmick and Hubbard, *op. cit.*, F. L. Dimmick and G. McMichael, A psychophysical determination of the limits of pure gray, this JOURNAL, 45, 1933, 314.

A Corning glass filter (No. 243, 2.19 mm.) which gives the equivalent of spectral red (635 mμ, purity 100%, brightness 9%) was placed in the filter compartment (F_a) of optical system A. Diaphragms B and C were set at zero and A was opened until there appeared on the viewing screen a well saturated, slightly yellowish red of medium brightness. The foot candle reading for this setting without the filter was 10.3. A blue color filter (Corning No. 554, 4.01 mm., 471 mμ, purity 100%, brightness 1.3%) was placed in filter compartment (F_c). As the diaphragm (D_c) in that system was opened, the color on the screen became less yellowish, passed through red, and appeared bluish red. At the same time, the total brightness increased somewhat. When the brightness change was sufficient to cut across the required judgment of hue, diaphragm A was decreased as C was increased, in a proportion that kept the brightness of the color constant. In the final series, sets of fixed apertures were used to give identical values in successive presentations.

Procedure. The colorimeter was set up in a dark room illuminated by a 500 w. Ivanhoe Northlight lamp, and the entire apparatus was screened from the observer except as much as could be seen through the viewing aperture (V_1). A shutter closed that opening save for the one second intervals of observation. A head-rest kept *O*'s head in a constant position. Observations were made entirely with the right eye which was maintained by the foregoing conditions in a constant state of moderate light adaptation. Before every experimental period, *O* remained in the experimental room for 5 min. to become adapted to the general illumination.

As in the preceding work, the *O*s were given a sufficient period of training with the method of limits to stabilize their judgments and to locate the region of the limens on the yellow side and on the blue side of red. The final values were derived from 100 series of 7 stimuli in each of the two regions, presented according to the method of constant stimuli.

Instructions. The *O*s were instructed as follows: "You will be shown a color. Judge whether the color is yellowish (bluish) or not-yellowish (not-bluish)." *O* was not asked to make any decision as to the purity of the red. Limens were determined for 5 of the *O*s who served in the previous experiment on the determinations of unique colors; namely, *C.D.*, *F.D.*, *F.*, *L.*, and *M.*

RESULTS

Table I shows the limits within which the red is neither yellowish nor bluish. Fig. 2 shows the location of the limiting values on the I.C.I. diagram. All *O*s agree so closely that the region is clearly delimited. Since there are no data on the number of J.N.D. within the limits of red, we have designated the mean of the two limits as 'red.' This gives 493.6c mμ for a representative red which is neither yellowish nor bluish. This value lies close to the one given by Exner; namely 494c mμ, and to the dominant

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[5] D. B. Judd, Preparation and colorimetric properties of a magnesium oxide reflectance standard, *Nat. Bur. Stand.* LC-395, 1933, 3.

TABLE I

LIMITS OF THE REGION OF RED FOR ALL *O*s

Judged	*O*	Trichromatic coefficients λ	x	y	h	σ	Purity (%)	Brightness (ft. lam.)
Yellow in red	C.D.	493.4 c	.631	.253	(3.34)	(.212)	86	.95
	F.D.	492.7 c	.662	.264	(2.46)	(.287)	87	.94
	F.	493.1 c	.645	.258	(2.61)	(.271)	86	.95
	L.	493.5 c	.627	.250	(2.34)	(.302)	86	.97
	M.	492.8 c	.657	.262	(2.32)	(.305)	87	.94
	Av.	493.1 c ± .3						
Blue in red	C.D.	495.7 c	.550	.222	(2.86)	(.247)	83	.96
	F.D.	494.1 c	.600	.240	(1.56)	(.453)	85	.96
	F.	493.7 c	.618	.248	(1.65)	(.429)	85	.96
	L.	494.8 c	.577	.234	(4.31)	(.164)	83	.99
	M.	493.7 c	.619	.247	(1.24)	(.570)	86	.96
	Av.	494.4 c ± .7						

Median Value for Red 493.6 c

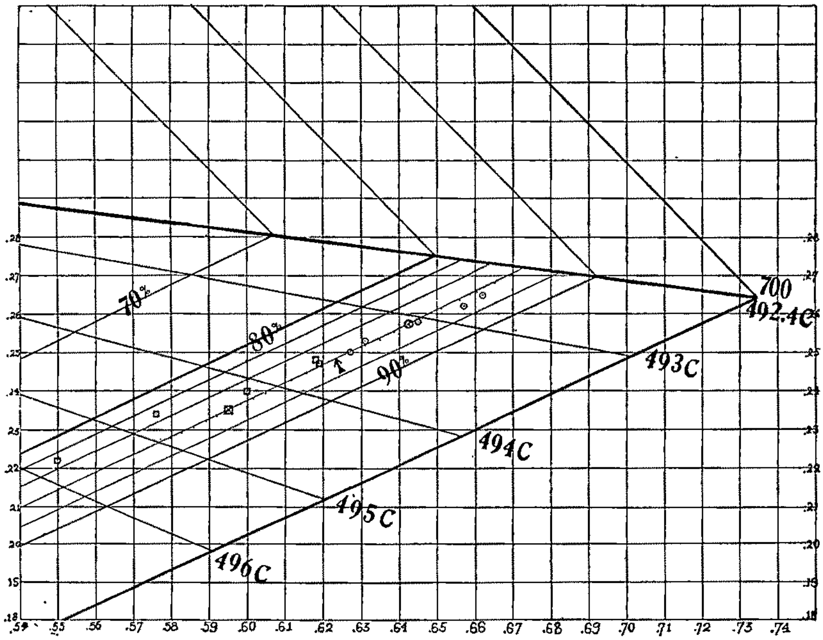

FIG. 2

FIG. 2 LOCATION OF THE LIMITS OF RED ON THE I.C.I. DIAGRAM
Small squares = 'just-not-blue' limits for all *O*s. Large square = Average 'not-blue' limit. Small circles = 'just-not-yellow' limits for all *O*s. Large Circle = Average 'not-yellow' limit. Arrow = Median value.

wave length of the Munsell sample 10-RP 5/10, though the latter is somewhat less saturated.

With the determination of 'unique' red as the complement of 493.6 mμ,

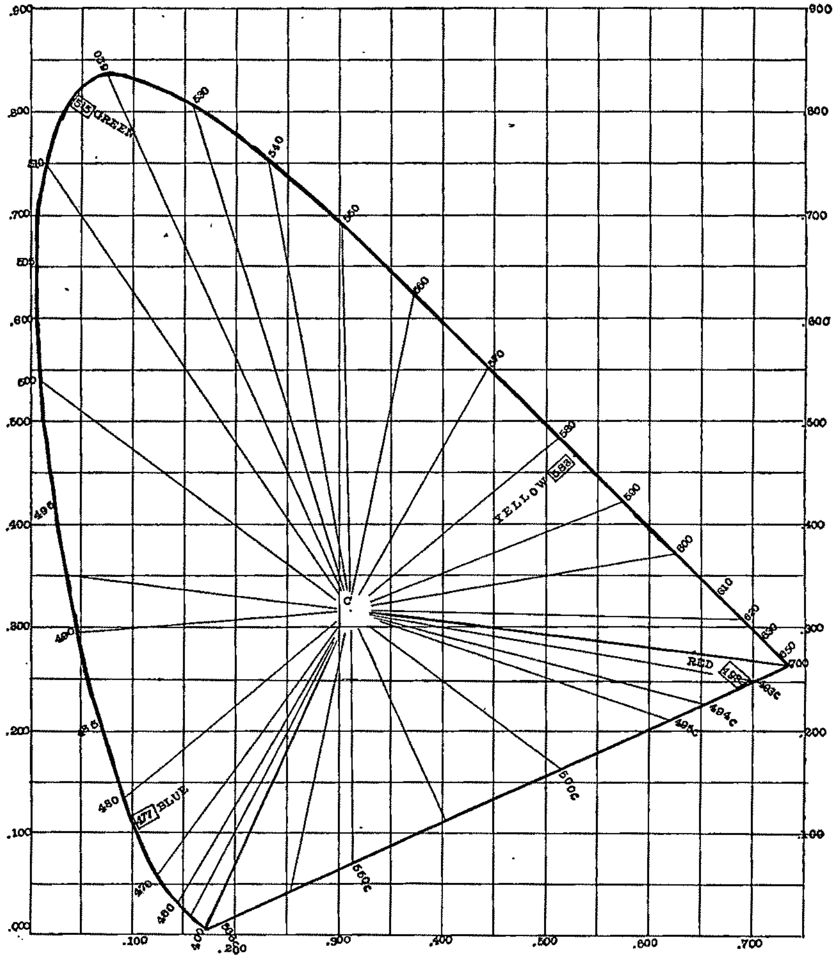

FIG. 3

FIG. 3 LOCATION OF PSYCHOLOGICALLY UNIQUE RED, YELLOW, GREEN, AND BLUE ON THE I.C.I. DIAGRAM

we now have the physical specifications of the seven psychologically unique colors (see Fig. 3). In terms of these, it is possible to put into quantitative form the four-variable color equation, x (red or green) ± y (yellow or blue) ± z (black or white) + G (Gray) = C.[6] Since the specifications are given in trichromatic coefficients, transformations can be made from one notation to the other and comparisons can be made with other systems of notation which have been or can be reduced to trichromatic coefficients. Full quantification of the psychological color equation now requires the determination of the J.N.D. constants relative to the seven points of reference.

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[6] Dimmick, *op. cit.*

SUMMARY

The amount of 'unique' blue (476 mμ) that must be added to spectral red (635 mμ) in order to produce a red which is neither yellowish nor bluish was determined. The just-not-yellow and just-not-blue limits of red were obtained with 5 *O*s, and a representative red was calculated as the mean between these two limits, yielding the value 493.6c mμ. The seven points of reference in the four-variable color equation have now been experimentally established.

CHANGES IN TENSION-PATTERN AND TOTAL ENERGY EXPENDITURE DURING ADAPTATION TO 'DISTRACTING' STIMULI

By G. L. Freeman, Northwestern University

The problem for this investigation was suggested by two seemingly incompatible results on the energy costs of mental work. Morgan,[1] testing the effect of distraction upon reaction-time, reported increased performance and increased muscular tensions. Davis,[2] however, showed that repetition of noise-stimulation produced a fairly rapid decrease in the magnitude of the action-potentials recorded from the right forearm. Harmon,[3] in a preliminary examination of the same question, found increased metabolism per unit of work done when noise was first introduced, but noted that in a relatively short time the worker tended to return to the metabolic rate characteristic of work under conditions of quiet. Commenting on several possible explanations of these results, Poffenberger says "one might accept the increased tensions noted by Morgan as authentic and interpret the reduced tensions demonstrated by Davis as a *shift of tensions* to new muscles with a reduction of tension only in those muscles on which measurements were at the time being made. The decreasing metabolic cost as adaptation to the distraction progressed would indicate merely that the later tension patterns functioned more economically than the earlier ones."[4]

Evidence for such shifts in tension pattern were found in experiments by the present writer both during the acquisition of a new habit and under changes in instruction.[5] In the present report, we have applied our method of recording muscle tensions from the four limbs to the problem of adaptation to distraction and have added measurements of total energy expenditure in terms of oxygen consumption. The results obtained may be regarded as a preliminary analysis of the compensatory changes in tension pattern

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* Accepted for publication September 6, 1938. Studies in Muscular Tension; Series 2, No. 9.

[1] J. J. B. Morgan, Overcoming distraction and other resistances, *Arch. Psychol.*, 5, 1916 (no. 35), 1-84.

[2] R. C. Davis, The muscular tension reflex and two of its modifying conditions, *Indiana Univ. Pub., Sci. Ser.*, no. 3, 1935, 1-23.

[3] F. L. Harmon, The effect of noise upon certain psychological and physiological processes, *Arch. Psychol.*, 23, 1937 (no. 147), 1-81.

[4] A. T. Poffenberger, Some unsolved problems in human adjustment, *Science*, 87, 1938, 124-129.

[5] G. L. Freeman, The spread of neuro-muscular activity during mental work, *J. Gen. Psychol.*, 5, 1931, 479-492.

which act to maintain the economy of adjustment even under conditions of distraction.

THE EXPERIMENTS

Two adults (*F* and *G*) performed addition problems under conditions of quiet and noise stimulation of approximately 50 db. on 12 consecutive days. Work periods of 20 min. were divided equally between the two conditions, and were preceded by 40 min. of rest. Total oxygen consumption and action potentials from the four limbs were recorded during the last 10 min. of rest and also during the mental work. All experiments were done in the morning with the *Ss* in the post-adsorptive state (without food for 12 hr.). While the conditions of quiet or noise stimulation were alternated in a random order so that records obtained in the basal state of rest would not reflect specific anticipation of noise or quiet, half of the time noise stimulation preceded quiet work and half the time followed it. (In preliminary series it was found that if *S* expected noise stimulation to precede quiet work, his neuro-muscular set or preparation differed from that which accompanied expectation of quiet working conditions.) During the experiments *S* lay in a semi-prone position on a cot. The addition problems (10 three-place numbers) were printed on cards and *S* was instructed to put his sums upon a long strip of paper attached to his writing board. The Jones motor-basal apparatus[6] was used for the metabolic measurements and the muscle action potentials were obtained from small (6 mm.) gold-plated disks cemented to the dorsal surface of the upper arms and legs. A common 'indifferent' lead was attached to the abdominal region, and a commuting arrangement permitted serial recording from the four areas at the rate of 2 per sec. The common amplifier used with these leads was built after the model described by Davis[7] and was connected to a crystal oscilloscope (type S_1) with optical level arranged for photographic recording. The physiological measurements made during each condition of rest, 'quiet' work and 'noise' work were, therefore, 2 separate determinations of the metabolic rate and 10 momentary records of action potentials from each of the four muscle groups.

RESULTS

In Table I we present the addition scores under conditions of quiet and noise distraction. As in the study by Harmon,[8] we find only a temporary reduction in work output upon the introduction of noise distraction. The percentages of correct solutions were practically identical in the two situations, the average for quiet being 80.4% and for noise 78.8%. The critical ratios of the experimental differences were low for all days and decreased progressively in magnitude with adaptation of the subject.

Since the *Ss* attempted to work at top speed in both conditions and were unaware of their scores, it is possible that compensatory effort was utilized during distraction. Records of oxygen consumption, given in Table II, do indicate a considerable increase in total energy expenditure, especially during the first few days of work under noise stimulation. Its gradual reduction to somewhere near the level originally established for quiet-work is shown by the Q-N scores. All metabolic measurement in the table has been reduced to the calories consumed per min. and the values given for conditions of basal rest, quiet-work, and noise-work are the average of two tests made daily under each condition. Records taken during the basal rest have a reliability coefficient of 91.4± .12, while records taken under conditions of quiet-work and noise-work have a test-retest correlation of 70.5 ± .15 and 81.0 ± .13.

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[6] The Jones motor-basal apparatus, *J. Amer. Med. Ass.*, 103, 1934, 1620.
[7] R. C. Davis, *op. cit.*
[8] F. L. Harmon, *op. cit.*

TABLE I

NUMBER OF CORRECT ADDITIONS DURING THE DIFFERENT 10-MIN. PERIODS

Day	Quiet			Noise		
	F	G	Av.	*F*	G	Av.
1	30	28	29.0	26	29	27.5
2	30	35	32.5	28	27	27.5
3	37	54	45.5	34	45	39.5
4	40	38	39.0	43	41	42.0
5	45	40	42.5	41	36	38.5
6	40	50	45.0	38	48	43.0
7	41	42	41.5	40	45	42.5
8	43	48	45.5	42	46	44.0
9	40	56	48.0	44	55	49.5
10	41	48	44.5	41	52	46.5
11	38	55	46.5	36	55	45.5
12	40	47	43.5	42	49	45.5

The slight but consistent increase in metabolic activity which accompanies mental work is in accordance with the expectation derived from studies by Benedict and Benedict,[9] Harmon[10] and others.[11] Such increments are insignificant when compared with the demands of physical work, and it is probable that no surplus store of fats or carbohydrates is drawn upon and that little oxygen debt is incurred.

The introduction of noise-distraction in such a situation causes at first a marked increase in metabolic activity. With continued stimulation, however, this effect is obscured by the readjustment of the individual to the new working condition, and distraction *per se* has little far reaching effect so far as general bodily economy is concerned. For an analysis of internal reorganizations which occur in the adaptive process we turn to action potential records from various muscle groups.

It will be recalled that we obtained action potential records from the upper arm and leg muscles at 2-sec. intervals during the conditions of basal rest, quiet-work, and work under noise distraction. In Table III these records are treated as if they were simultaneous and are scored in terms of the total deflections per sec. according to the method advocated by Davis.[12]

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[9] F. G. Benedict and C. G. Benedict, The energy requirements of intense mental work, *Proc. Nat. Acad. Sci.*, 16, 1930, 438-443.

[10] F. L. Harmon, *op. cit.*

[11] F. C. Becker and O. Ollson, Metabolism during mental work. *Skand. Arch. f. Physiol.*, 31, 1914, 80-197.

TABLE II

THE EFFECT OF BASAL REST, QUIET-WORK, AND WORK UNDER NOISE DISTRACTION UPON TOTAL EXPENDITURE OF ENERGY
(Calories consumed per min.)

S	Condition	Days of tests											
		1	2	3	4	5	6	7	8	9	10	11	12
F	Basal	1.61	1.87	1.64	1.50	2.13	1.92	1.54	1.61	1.87	1.82	1.63	1.70
	Quiet	1.86	1.91	1.64	1.62	2.10	2.00	1.63	1.68	1.82	1.80	1.78	1.73
	Noise	2.24	2.10	2.18	2.25	2.20	2.16	1.72	1.52	2.01	1.78	1.93	1.73
	Diff.$_{N-Q}$	.37	.18	.54	.63	.10	.16	.08	—.17	—.19	—.02	.14	.01
S	Basal	1.78	1.60	1.60	1.82	1.77	1.67	1.62	1.78	1.60	1.75	1.82	1.82
	Quiet	1.72	1.71	1.69	1.80	1.79	1.84	1.75	1.72	1.65	1.52	1.83	1.76
	Noise	2.43	2.53	1.82	2.41	1.92	1.83	2.24	2.06	1.65	1.73	2.02	1.80
	Diff.$_{N-Q}$	.71	.82	.12	.61	.13	—.01	.45	.33	—.00	.10	.18	.04
Av.	Basal	1.69	1.74	1.62	1.66	1.95	1.80	1.58	1.70	1.74	1.78	1.72	1.76
	Quiet	1.79	1.81	1.67	1.71	1.95	1.92	1.69	1.74	1.74	1.71	1.31	1.78
	Noise	2.34	2.32	1.99	2.33	2.06	1.99	1.98	1.79	1.83	1.75	1.97	1.77
	Diff.$_{N-Q}$	.54	.50	.33	.62	.11	.08	.26	.08	.09	.04	.16	.02

TABLE III

MAGNITUDE OF THE ACTION POTENTIALS IN THE FOUR LIMBS ON SUCCEEDING DAYS, DURING BASAL REST (R), QUIET-WORK (Q), AND WORK UNDER NOISE DISTRACTION (N)
(Total deflections per sec., average of 2 Ss)

Days	Right arm			Left arm			Right leg			Left leg			Average		
	R	Q	N	R	Q	N	R	Q	N	R	Q	N	R	Q	N
1	120	361	525	97	225	742	102	200	904	165	215	835	121	250	751
2	85	228	444	109	287	801	150	225	504	154	291	520	124	258	569
3	125	312	518	150	265	628	85	254	475	152	247	515	128	269	534
4	110	276	432	67	192	485	107	194	312	175	163	388	115	206	404
5	254	295	449	285	217	527	318	225	350	280	297	291	284	258	404
6	148	224	421	95	210	433	205	212	288	227	238	245	169	221	347
7	72	200	344	65	172	405	188	175	183	105	189	192	107	184	281
8	103	287	370	97	154	375	150	142	180	174	170	178	131	188	276
9	125	195	362	136	188	291	207	217	231	159	143	140	157	186	256
10	96	193	394	114	177	215	227	225	212	248	220	187	171	204	252
11	50	225	345	87	103	200	135	243	154	127	151	163	100	180	215
12	200	219	337	182	185	282	212	177	170	181	188	175	194	192	241
Av.	124	251	412	124	198	449	174	207	330	179	209	320	150	216	377

Daily averages are given under each condition. Mean deviations of individual averages ranged from 84 to 324. In spite of this variability, the data still appear to be highly consistent for a given *S* in all three situations; that is, there was relatively less electro-myographically recorded activity shown during basal rest than in quiet-work and noise-work conditions.

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[12] R. C. Davis, *op. cit.*

Table III indicates that while the right arm muscles were usually most active in the condition of quiet-work, their activity is exceeded in the condition of noise-work by other muscles groups, especially during the first few introductions of the distracting stimuli. This shift in the pattern of muscle tensions is not open to a 'fatigue' interpretation, for in half of the cases the noise condition actually preceded the quiet condition without producing any marked change in the general trend. What is apparently exhibited is an habituation effect to a new and novel situation, the condition of quiet work presumptively occasioning a more usual or normal performance for the subject.

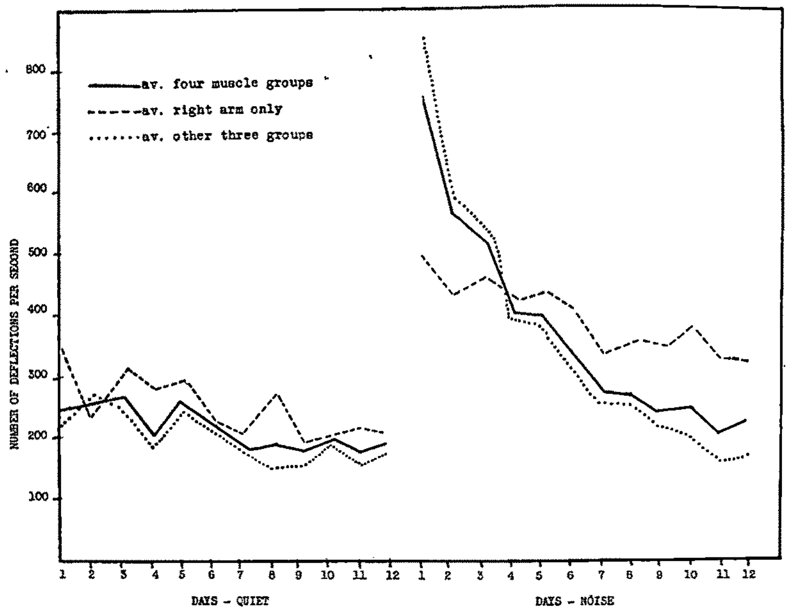

FIG. 1. AVERAGE MAGNITUDE OF ACTION POTENTIALS ON SUCCESSIVE DAYS UNDER QUIET WORKING CONDITIONS AND UNDER NOISE DISTRACTION

Fig. 1 shows clearly the redistribution of the tension pattern upon the introduction of noise and also indicates the progressive change which this pattern undergoes as *S* adapts to the distraction. The solid line represents magnitude of action potential deflections when the activity of all four muscle groups is averaged; it shows little change from day to day during quiet work while for noise it falls rapidly from an initial peak so that toward the end of the tests total activity is on the level established for quiet work. As might be expected, these values correlate positively with the determinations of total energy expenditure in the same situations (0.51 for quiet-work and 0.74 for noise-work).

Because both *S*s used the right arm in totaling sums and because there was a tendency in all conditions for muscular activity to be more concentrated there than in other bodily parts, we have separated these action potential scores from the totals and indicated their comparative magnitudes by means of a broken line. The dotted line, also appearing on the same graph, is the average magnitude of action potential activity occurring correlatively in the other three limbs. Taken *in toto,* the graph suggests that in working adaptation to noise, activity in the focal part of the supporting tension pattern (right arm) is slightly increased (over the quiet work control) and remains so during the period of test. The more obvious change under distraction is in what might be called the non-focal parts of the tension pattern. Activity in the other limbs, while normally less than that of the right arm, is markedly increased at the first introduction of distraction. An interpretation of the parallelism between progressive changes in these parts and in total energy expenditure is that such activities are compensatory and tend to drop back to a minimum as the focal part of the tension pattern reëstablishes a more economical function.

Discussion

The results obtained in this study suggest that the initial effect of distracting stimuli is to temporarily unbalance a bodily economy in which, for habituated acts at least, supporting processes operate at an optimal level. The added effort which *S* applies to compensate for the distraction expresses itself not only by a rise in total energy expenditure, but also by a shift in the pattern of supporting processes. There is a marked spread of neuromuscular activity to parts but remotely connected with the task at hand, and the original focus of the pattern tends to be lost. Under repetition of stimulating conditions, however, this 'irradiation' effect becomes less prominent and the more economical concentration of supporting processes in limited areas begins to reassert itself. Such redevelopment of a focalized response-pattern may involve a slightly greater degree of supporting tensions, but these changes apparently are not extensive enough to have much effect on the economy of total energy expenditure.

The fact that we obtained records showing a concentration of neuromuscular activity in the right forearm during addition under quiet work may well be an artifact of our conditions;[13] that is, utilization of some other effector part for setting down the sums might have developed a greater degree of activity in that part. Even were this supposition true, it would seem not to weaken the principle of redistribution in tension-load under distracting stimulation.

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[13] R. C. Davis (*op. cit.*) obtained records from the right forearm showing marked reduction under noise distraction. However, it should be recalled that Davis was not dealing with the problem of working distractions; that is, noise was introduced upon a condition of rest, in which differential tension patterns might not have been as prominent as in the present experiment.

In an earlier study it was found that habituation of an act (though not learned under noise conditions) was accompanied by a reduction in tension in remotely related muscle groups.[14] This work has subsequently been confirmed and developed by others.[15] The theory and practice of differential relaxation for various tasks recognizes similar principles.[16] One research in which simultaneous records were taken from four muscle groups showed that habituation produced a concentration of tensions in muscles primarily involved in the task.[17] It would seem that the present experiment is an extension of this observation to the problem of work under distracting conditions, the supporting processes at first outbalance the distraction by intensive and extensive increments. During protracted work under these conditions, however, a more economical pattern is reëstablished. Work output during the entire readjustment process may remain fairly constant, and according to present indications at least, the net effect upon total energy expenditure may also be minimal.

While these results are suggestive, it must be obvious that they leave many related problems untouched and unsolved. Especially we would call attention to the fact that interpretation of the *shift in tensions* under habituation is not yet understood in terms of neural dynamics. Also, it is not known how effectively such shifts would operate either for heavier working loads or under more intense distraction. The *S*s in this experiment were perhaps not as seriously disturbed by noise as many others, or they may have possessed more stable or efficient neuro-muscular systems. To hypothecate such limitations is at present both unwarranted and unwise; but that these may exist even in young children is suggested in studies by Johnson,[18] Duffy[19] and others.[20]

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[14] G. L. Freeman, *op. cit.*

[15] J. Stroud, The rôle of muscle tensions in stylus maze learning, *J. Exper. Psychol.*, 14, 1931, 606-631; M. S. Clites, Certain somatic activities in relation to successful and unsuccessful problem solving, *ibid.*, 18, 1935, 708-724. E. Ghiselli, Changes in neuro-muscular tension accompanying the performance of a learning problem involving constant choice time, *ibid.*, 19, 1936, 91-99.

[16] E. Jacobson, Electrophysiology of mental activities, this JOURNAL, 44, 1932, 677-794.

[17] G. L. Freeman, *op. cit.*

[18] B. Johnson, Changes in muscular tension in coördinated hand movements, *J. Exper. Psychol.*, 11, 1928, 334.

[19] E. Duffy, The measurement of muscular tension as a technique for the study of emotional tendencies, this JOURNAL, 44, 1932, 146-162.

[20] G. L. Freeman, and E. T. Katzoff, Muscular tension and irritability, *ibid.*, 44, 1932, 789-792; M. A. Wenger, Some relationships between muscular processes and personality, *Child Dev.*, 9, 1938, 261-276.

PERSISTENT ALPHABETICAL SYNESTHESIS

By H. L. HOLLINGWORTH, Columbia University, and VIRGINIA WEISCHER,
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An eight-year-old child was heard to remark, upon first hearing the name of an unfamiliar person, "That name is brown, isn't it?" Further inquiry revealed that all names were colored in her experience and that she supposed that this was the experience of others also. A series of verbal stimuli brought out the promptness of her color responses, and these were recorded. A year later the same series was repeated, and the responses were so similar to the earlier ones that it appeared desirable to make systematic record of them, and to make some study of their consistency and permanence.

The initial observations were made 12 yr. ago (1926) when *S* (the junior author) was 8 yr. old. The initial stimuli were presented again at the ages of 9 and 10 yr., without previous notice. Thereafter nothing was said of the matter for a period of 10 yr. In December 1938, *S* was 20 yr. old, a college senior, and an opportunity occurred to repeat the initial experiment, and also certain extensions that had been added at the trial 10 yr. previous.

Since the main interest of the earlier repetitions of the stimulus-series lay in the question of the persistence of the color phenomena, no very systematic exploration was outlined. In the light of the later findings it would have been better had a more thorough going alphabetical analysis been made in the beginning. But two sets of responses, 10 yr. apart, subsequent to two earlier annual tests, seem to bring out the essential features of the phenomena, even if they do not suggest a wholly adequate explanation of them.

The available data are as follows. Stimuli presented 4 times, over a period of 12 yr., from childhood to maturity, were the nine digits, three two-number combinations, the days of the week, seven proper nouns, an abbreviation, and inquiries concerning the sound of two musical instruments. The color responses to these are shown in four columns of Table I.

In 1928, additional stimuli were included in the series, and all these were repeated at the final test, 10 yr. later. These included nine words whose meanings were unknown to the child (Table II), eight pairs of words sounding alike but having different meanings (Table III), and fifteen reversed questions, in which the color was named by the experimenter and *S* asked to state what names have that color (Table IV).

In the 1938 examination it appeared that the key to the results was to be found in the fact that each digit and each letter of the alphabet had from the beginning (and for some time previous to the examination at 8 yr. of age) had a specific color, the colors of words being those of the dominant or initial letters. At this time the color of each letter of the alphabet, in random order, was asked for, and a few days later this alphabet series was repeated (Table V).

In addition *S* was asked various questions about the nature of the experience, and in 1938 wrote out a brief account of its character. In these accounts it appeared that

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* Accepted for publication January 9, 1939.

TABLE I

SYNESTHETIC RESPONSES FROM CHILDHOOD TO ADULT LIFE

Stimulus-word, letter, or number	Color responses on following dates			
	Nov. 1926	Nov. 1927	Nov. 1928	Dec. 1938
One	white	white	white	white
Two		white	yellow	white and yellow
Three	yellow	dark yellow	brown	yellow
Four	pink	pink	pink	pink
Five	blue	blue	blue	blue and yellow
Six		black	black	black
Seven	yellowish brown	light olive	light olive green	green
Eight	reddish brown	brown	reddish brown	brown
Nine	dark green	now black often changes	black	black and purple
Monday	blue and white	blue and brown	blue	blue and white
Tuesday	yellow and black	yellow and black	yellow	yellow
Wednesday	gray	gray blue	gray and blue	bluish gray
Thursday	yellow and brown	yellow	yellow	yellow and blue
Friday	gray	most gray some blue	blue-gray	purple
Saturday	gray	gray	gray	black, I guess
France	yellow or purple	yellow and purple	purple	purple
Greece	green	green and gray	gray	green, then whitish
New York	white and gray	gray and white	gray and blue	gray and dark
London	yellow and green	yellow	yellow	yellow and white
Montrose	Mont—brown rose—pink	Mont—brown rose—pink	Mont—brown rose—pink	brown
Virginia	pink	pink	pink	pink
Hollingworth	Holl—purple ing—yellow worth—white and black	purple, yellow orange gray	purple yellow and pink gray	"blue and black—the worth is light yellow or light"
Mrs.	M—lavender r—green s—red	brown green red	"red and yellow"	brown brown red
"the sound of a violin"	purple (Says "Maybe because 'violin' and 'violet' are similar.")	purple	purple	"green, but can't be sure. The word is brown."
"the sound of a piano"	red or green or brown	high—yellow, pink low—brown, black or dark red	"None"	white
47	4 pink	pink	pink	pink
	7 green	yellow green	olive green	green
86	8 red	brown	reddish brown	brown
	6 gray	black	black	black
35	3 yellow	light tan	yellow	yellow
	5 blue	blue	blue	blue

the precise alphabetical character of the synesthesis was not recognized by *S*. The colors just "came," or they did not. A discussion of the results follows the tabulation of the responses.

The most striking things about these results are their remarkable consistency and systematic character, and their persistence from early childhood well into adult life (from at least as early as 6 to later than 20 yr). The description of the experience in itself also has some interest.

When first observed, *S* was 8 yr. old, with an I.Q. of about 150. She had been from infancy interested in colors, and her mother reports that colors were the first things she knew the names of. She was fond of working with colors, in drawings and decorative designs, and had a good appreciation of color effects.

TABLE II

Synesthetic Responses, 10 Years Apart, to Stimulus-Words Whose Meanings Were Unknown

Stimulus-words	Nov. 1928	Dec. 1938
synapse	red and green	*red and yellow
Fechner	purple and green	pink and yellow
psyche	red	*pink and white
glossopharyngeal	orange and purple	yellow and last part bluish gray
neurilemma	green and yellow	green and yellow and gray
Babinsky	brown	brown
cortex	white	*white and brown
aorta	blue and yellow	blue and white
Kegan	purple	green and orange

* Meaning of the stimulus-word now known.

TABLE III

Synesthetic Responses, 10 Years Apart to Stimulus-Words Sounding Alike but Whose Meanings Are Different

Stimulus-words	Nov. 1928	Dec. 1938
tea	yellow	yellow
"t"	yellow	yellow
sea	red and white	red
see	orange	red and black
fast (swift)	blue and black	lavender
fast (not eat)	blue and black	lavender
post (of fence)	pink	pink and white
post (a letter)	pink	pink and white
blue	gray	blue and gray and white
blew	blue and yellow	blue, gray and white
sew	red and brown	red and black
so	red and white	red and white
chews	white and yellow	reddish and light
choose	white and yellow	reddish and light
Greece	gray	"Don't know,—they keep changing."
grease	gray	

She sees colors only in connection with letters, words, and numbers. The symbols themselves appear, apparently "in her head." They are not as if printed on a page, but there is a vague light background, and the symbols appear in color or in a spot of color. At the age of 20 yr. she says:

> "As I hear the word it immediately seems to be printed in front of my eyes—and yet inside my head. Sometimes a word will appear to be all one color and sometimes it will have several colors."

"Colors do not appear when I read silently or aloud nor when others read or speak, usually. I never notice colors in words unless I *think of* a particular word by itself. In conversation an unfamiliar word sometimes occurs to me as having color because I know nothing else about it."

"Usually the first letters and the letters which have the most prominent sounds stand out from the rest and seem to have much more color. Often a great many of the letters in a word may be grayish or nondescript and a few of the important letters will give their color to the whole word."

TABLE IV

SYNESTHETIC RESPONSES, 10 YEARS APART, TO SPECIFIC COLORS

Stimulus—situation	Nov. 1928	Dec. 1938
What day is yellow?	Tuesday and Thursday	Tuesday
What day is gray?	Saturday ("Fri" would be gray but "day" not)	Friday
What day is gray-blue?	Wednesday	Wednesday and Monday
What day is red and yellow?	Sunday	Sunday
What number is blue?	5	5
What number is brown?	none	8
What number is white?	1	1
What number is black?	6 and 9	6 and 9
What number is yellow?	2	2, 3, 13, 30
What number is pink?	4	4
What cities are gray?	Athens and New York	New York
What cities are yellow?	Berlin	London
What cities are green?	Rio de Janeiro	"don't think of any"
What instruments sound purple?	violin	"violin might"
What instruments sound brown?	"none I think of"	drum

TABLE V

SYNESTHETIC RESPONSES, A FEW DAYS APART, TO THE LETTERS OF THE ALPHABET GIVEN IN HAPHAZARD ORDER

Letter	1st trial	2nd trial	Letter	1st trial	2nd trial
A	black and blue	black	N	green	green
B	brown	brown	O	white	white
C	white	white	P	pink	pink
D	green and brown	green and brown	Q	light gray	gray
E	black	black	R	green	green
F	bluish black	purplish gray	S	red	red
G	orange	orange	T	yellow	yellow
H	black	bluish gray	U	gray	gray
I	white	white	V	pink	pink
J	yellow	yellow	W	blue	grayish white
K	purple	gray	X	brown	brownish
L	yellow	yellow	Y	white	white
M	brown	brown	Z	gray	gray

Voices, musical tones, tastes, odors, and words in foreign languages do not ordinarily have color. Concrete objects have their own colors but their names, including proper names, have distinctive colors, not the colors of the objects of which they are the names. Color names are not exclusively the colors which they name, though mainly so. The color of 'blue' is "grayish or whitish and blue." But 'purple' is "purple."

Detailed examination of the tabulated responses brings out a number of interesting things. Over the period of 12 yr. the digits maintain their characteristic colors, with only trivial variations. Even the variations are consistent—thus 7 is yellowish brown, light olive, light olive green, green. The variation is apparently in the momentary choice of names, rather than in the color tone itself.

The days of the week also have their consistent colors from childhood to adult life; mainly the colors of their initial capital letters, as may be seen by referring to the alphabet list. As for other words, "France" is mainly purple (the color of F), and "Virginia" (her name) is always pink (the color of V). She says she always wished the color of her own name to be blue but "It always stayed pink." It is not so easy to see where the greenish-gray of "Greece" comes from (perhaps from the R, C and EE, green, white, black) but the initial letter (G, orange) does not count here.

In some words each syllable has its color; in the abbreviation "Mrs." each letter is separately colored. The two-place numbers are invariably two-colored, the colors of their respective digits. Even words she cannot precisely spell, and whose meaning she does not know, have characteristic color. Thus, at 10 yr. of age, "neurilemma" is reported as "green and yellow" (N—green, L—yellow). "Babinsky" is of course brown, from its capital B.

Words which sound alike but have different meanings, do not vary in color. This might be due either to the identity of sound or to the identity of initial letter. But it is apparently not phonetic elements that have color for this *S*, it is instead the letters of the alphabet and the single digits. Nevertheless it is of some interest that all the vowels are achromatic—white, gray or black (including also the Y which is often a vowel sound). It is the consonants that have the spectral colors.

Explanatory conjectures readily occur, but exceptions are easily found to most generalizations that have been tried out. Thus it might be surmised that the associations are mediated by the letters of the color names. It is true, for example, that B is *b*rown; but so also are D,M,X. Wh*i*te is the color of I and Y; but also of C and O. It might seem "natural" for L to be ye*ll*ow and for P to be *p*ink. But J and T are also yellow and 4 and V are pink too. The name of a color does not itself have that color exclusively. Hence this theory falls short.

The usual sensory and cortical theories that have been applied to the miscellaneous synestheses do not fit well with the fact that in this case sounds other than letters and numbers lack color.

Did *S*, as an infant, learn the letters and numbers from blocks or pages in which each had its fixed color, and in which all the vowels were achromatic? This theory has a reasonable ring, but neither *S* nor her mother has any recollection of any such playthings, and none are to be found in the collection of things she has preserved from earliest childhood. She is said to have learned to read at a very early age, by memorizing stories read to her from the ordinary printed page and then tracing these stories on the page, thus first learning the words, rather than beginning with the alphabet.

Nevertheless some such explanation fits well with the fact that the colored hearing in this case appears to go back fundamentally to the characteristics not of sounds as heard but of letters as "thought of" or "imaged." Such a genesis would articulate well with the most reasonable explanations offered for more casual and transient synestheses.

Summary. An eight-year-old girl reported that all words had their characteristic colors. She had apparently had this experience for at least two years or so. A list of stimulus-words was presented and the color responses recorded. One year and two years after the same list was presented. After a lapse of ten years from the last annual test, twelve years after the first examination, the observations were repeated, with a duplication also of certain new stimulus-words that had been added ten years before. The reports were surprisingly consistent and systematic. The key to the phenomena was found to lie in the fact that each digit and each letter of the alphabet had a special color, and these elements, or prominent or initial elements, gave the general color tone to words and two-place numbers. Unsuccessful attempts were made to trace the genesis of this alphabet synesthesis. *S* was not clearly aware of the very systematic character of the associations, which have persisted well into adult life.

POST QUANTIFICATION OF INTROSPECTIVE DATA

By Douglas Fryer, New York University

Descriptions of conscious activity during short tests of simple performance have been rated upon a scale of degrees of motivation and correlated with changes in performance from test to test as an indicator of the validity of this method of post quantification of introspective data.

Instructions. The general description of conscious activity for the period of the performance was undirected by the experimenter and was secured by means of the following written instructions: "At the conclusion of each test you are asked to dictate an introspection of your awareness of speed, or movement, or push, or drive, or motive. Start with the last awareness as you finish the test and work backward toward the beginning, recalling these awarenesses." Occasional promptings, "Is that all you recall?", were employed.

Conscious motivation was maintained at a high level, for a series of approximately 10 tests, throughout each of several days' observations. This was accomplished by administering systematically various new incentives, including knowledge of own record from test to test and knowledge of slightly superior records.

Observers. Four *Os* (*F, G, H, J*), worked in the major part of the experiment with tests of 1-min. duration.[1] One *O* (*I*) worked with tests of 4-min. duration.[2] All the *Os* were trained in reporting conscious activity. No specific training in the task preceded the work. The task consisted of Fryer's *Speed Addition Test*,[3] in which the problems are a chance distribution of totals of less than nine, arranged in rows of thirty.

Rate of performance is used as the criterion of validity. As indicated elsewhere,[4] performance has been found to follow conscious stimulation wherever intention can be specifically determined—in fact, far more closely than environmental stimulation and perhaps exactly within the range of capacity of the performer.

The descriptions of conscious stimulation averaged approximately 100 words, with very few totaling less than 50 words, which appeared to be as full undirected descriptions as could be gathered. There were 237 introspective descriptions distributed as follow: *F*, 46; *G*, 41; *H*, 32; *J*, 62; and *I*, 56 (4-min. tests)

Rating scales. The introspective descriptions were rated by *E* on two scales, Scale

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[1] Acknowledgement is made to Professor F. C. Bartlett, Drs. P. E. Vernon and M. G. Vernon and Mr. G. C. Grindley of the Psychological Laboratory of the University of Cambridge, England, who worked as the observers in the one-minute tests.

[2] The author, who wrote his own introspective reports. This work was performed prior to the main part of the experiment with the 4 *Os*.

[3] Douglas Fryer, *op. cit.*, 1936. Published by the Department of Psychology, New York University.

[4] Fryer, A genetic study of motivation under changing auditory situations, *Brit. J. Psychol.*, 24, 1934, 408-433; Motivation effects of auditory timing upon repetitive mental work, *ibid.*, 25, 1934, 140-169; Specific conscious intent and its correlates in performance, *ibid.*, 27, 1937, 364-393; Conscious activity in coördination of repetitive mental work and rhythmic timing, *ibid.*, 28, 1937, 150-166; Variability in automatic performance with uniform intent, *J. Appl. Psychol.*, 21, 1937, 528-545.

A providing for a general estimate of conscious motivation and Scale B including several specific estimates of factors thought to exist in conscious stimulation. These scales are given below as used in the experiment.

In application of these scales a judgment upon the part of the rater was involved between what is irrelevant in the introspective report of awareness and what is a part of the working situation.

A. Rating Scale of Efficiency of Intention in Realization of Goal

To decrease performance				To increase performance		
tremendous	definite	slight	neutral	slight	definite	tremendous
3—	2—	1—	0	+1	+2	+3
1	2	3	4	5	6	7

A generalized rating of efficiency in operation of the various factors of conscious motivation (including focus of cues, clearness of goal, amount of intentional effort, etc.) in realization of goal.

B. Rating Scales of Conscious Factors in Intention

I. Scale for conscious effort or degree of intentionally exerted motor pressure (tension). Rating of the amount of intended effort or energy, as reported in verbalizations or kinesthetic and organic sensitivities, or in generalized statements of motor attitude or set. Restraining effort (motor inhibition) is rated as minus.

no information	none	slight	definite	tremendous
X	0	1	2	3

II. Scale for degree of focus of intentionally used cues. Rating of the clearness of an intentionally used cue, or cues, as reported in imagery, verbalizations, or in generalized statements. Rating includes any quantification of the cue, or cues, consistently used.

no information	none	vague	intermittently or fairly clear	consistently or completely clear
X	0	1	2	3

IIa. Scale for degree of focus of exerted motor pressure (tension). This additional rating is of the clearness with which motor pressure as a cue is a part of conscious intention.

no information	none	vague	intermittently clear	consistently clear
X	0	1	2	3

III. Scale for frequency (or number) of cues occurring in working situation. Rating of number of cues during a test, as reported in imagery or in verbalizations, or in generalized statements. Cues need not be used, but must be related to working situation.

no information	none	few (1 or 2)	several (3 or 4)	exceptional number (more than 4)
X	0	1	2	3

Results. Table I shows all correlations (rho) by *O* and observation between the criterion of validity, rate of performance, and the various ratings of conscious motivation in the rating scales. Performance was computed for this analysis as percentage change from previous test based on Test 1 of each observation as 100%. Table II gives the arithmetic means and Table III the absolute means of correlations. The absolute means indicate the extent of correlation in either direction and the arithmetic means the extent of correlation in the direction accepted by the experimenter as positive conscious motivation.

Only fair validity is indicated for the method of post quantification of the undirected introspective description, as used here. The average (arith.) correlation of the one-minute tests for Scale A, the general scale, is about 0.30. This correlation is raised only a few hundredths to 0.36 with the best assembling of specific ratings of Scale B, with 0.44 as the highest average (arith.) coefficient for any one rating.

TABLE I

CORRELATION (RHO) BETWEEN QUANTIFIED INTROSPECTIONS AND PERCENT CHANGE FROM PREVIOUS PERFORMANCE BY DAILY OBSERVATIONS

(Daily observations of approximately 10 tests.)

O	Observations	Quantified introspections						
		A	B_{I}	B_{II}	B_{IIa}	B_{III}	$B_{I+II+IIa}$	$B_{I+II+IIa+II}$
F	I	0.30	0.48	0.57	0.14	0.10	0.43	0.36
	II	0.10	−0.11	0.40	−0.06	0.28	−0.02	0.04
	III	0.44	0.05	0.78	0.05	−0.37	0.36	0.27
	IV	0.25	0.03	0.45	0.78	−0.17	0.50	0.38
G	I	0.61	0.32	0.50	0.39	−0.40	0.38	0.34
	II	−0.15	−0.02	0.75	0.41	0.00	0.52	0.50
	III	0.25	0.65	0.78	0.58	−0.37	0.78	0.85
	IV	0.48	0.43	0.61	0.42	0.26	0.57	0.89
H	I	0.23	0.21	−0.26	−0.14	0.10	−0.12	−0.22
	II	0.48	0.45	0.67	0.33	−0.08	0.46	0.45
	III	0.53	0.63	0.32	0.30	0.05	0.44	0.37
J	I	0.02	0.35	0.10	0.01	−0.36	0.13	0.04
	II	−0.18	−0.02	0.01	0.28	0.06	−0.15	−0.20
	III	0.06	0.34	0.05	0.29	0.08	0.26	0.16
	IV	0.42	0.27	0.44	0.39	−0.24	0.46	0.30
	V	0.81	0.43	0.83	0.68	0.50	0.79	0.80
	VI	0.34	0.35	0.46	0.10	0.50	0.32	0.32
I	I	0.47	−0.07	0.47	0.50	−0.29	0.31	0.12
	II	0.49	0.35	0.11	−0.11	0.33	−0.12	0.03
	III	0.11	0.17	0.08	0.34	0.41	0.04	0.24
	IV	0.13	−0.55	0.10	0.68	−0.04	−0.13	−0.17
	V	0.27	0.16	0.61	0.37	0.31	0.69	0.73
	VI	0.06	−0.16	0.12	0.19	0.20	−0.06	−0.01
	VII	0.26	0.22	−0.38	0.55	−0.14	0.24	0.01

It seems doubtful that further statistical treatment of these data would offer additional information as the conditions of validity in such an experiment usually lie within the data where no statistical treatment can correct them.

Certain improvements in method are suggested in the experiment which may raise the validity of post quantification of introspective data to a degree satisfactory for exploratory purposes in the study of conscious motivation. It is suggested that the descriptions of conscious activity for one-minute tests are more valid than for four-minute tests. In recent unpublished research by the author it has been found possible to secure reliable measures of 10 second performance with the same test material as used in this experiment, but the observers must be trained in accurate starting and stopping of work. A further trial of method should be worth-while with 10 second tests where more detailed descriptions of conscious activity are possible.

The author is interested in this method because of its possible application in the technical occupations. Intelligent workers may be able to report conscious activity for correlation with parallel objective measures, such as motion and time-study records. In such applied situations intensive training in introspective technique is impractical and the undirected report offers a possible source of as yet unaccessible psychological knowledge of a variety of skills.

TABLE II

MEAN (ARITH.) OF CORRELATIONS (RHO) FOR DAILY OBSERVATIONS BETWEEN QUANTIFIED INTROSPECTIONS AND PERCENTAGE CHANGE FROM PREVIOUS PERFORMANCE

O	No.	Quantified introspections						
		A	B_I	B_{II}	B_{IIa}	B_{III}	$B_{I+II+IIa}$	$B_{I+II+IIa+III}$
F	4	0.27	0.11	0.55	0.23	−0.04	0.33	0.26
G	4	0.30	0.35	0.66	0.45	−0.13	0.56	0.65
H	3	0.41	0.43	0.24	0.16	0.02	0.26	0.20
J	6	0.25	0.29	0.32	0.29	0.09	0.30	0.24
Mean		0.31	0.30	0.44	0.28	−0.01	0.36	0.34
I	7	0.26	0.02	0.16	0.36	0.11	0.14	0.14

Suggestions. Several interesting suggestions in the study of conscious motivation are indicated in this analysis of method while the data are insufficient for a satisfactory statistical treatment of them.

(1) The B_{II} rating, which is the "Scale for degree of focus of intentionally used cues," has the highest correlation with the 1-min. performance and may carry the full load of the ratings. This is interesting as it points to conscious focus of intentionally used cues as an important factor in increasing performance. This suggestion is supported in a previous study where observers were working with intention to maintain rate without change.[5] The correlation between estimated degrees of focus of intentionally used cues and variability in performance in reverse order was 0.84 ± 0.08.

TABLE III

MEAN (ABSOLUTE) OF CORRELATIONS (RHO) FOR DAILY OBSERVATIONS BETWEEN QUANTIFIED INTROSPECTIONS AND PERCENTAGE CHANGE FROM PREVIOUS PERFORMANCE

O	No.	Quantified introspections						
		A	B_I	B_{II}	B_{IIa}	B_{III}	$B_{I+II+IIa}$	$B_{I+II+IIa+III}$
F	4	0.27	0.17	0.55	0.26	0.23	0.33	0.26
G	4	0.37	0.36	0.66	0.45	0.26	0.56	0.65
H	3	0.41	0.43	0.42	0.26	0.08	0.34	0.35
J	6	0.31	0.29	0.32	0.29	0.29	0.35	0.30
Mean		0.34	0.31	0.49	0.32	0.22	0.40	0.39
I	7	0.26	0.24	0.27	0.39	0.25	0.23	0.19

(2) The B_{IIa} rating, which is the "Scale for degree of focus of exerted motor pressure (tension)," has a higher correlation with 4-min. performance than with 1-min. performance, which suggests that intentional motor pressure becomes more essential in highly motivated activity during lengthy periods of performance. The

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[5] Fryer, *op. cit.*, *Brit. J. Psychol.*, 27, 1937, 364-393.

literature as to the importance of motor effort in performance is contradictory and it may be that the time factor has been neglected.

(3) The $B_{II\ I}$ rating, which is the "Scale for frequency (or number) of cues occurring in the working situation," offers the lowest correlation with the criterion. Seven of 17 coefficients for the 1-min. tests and 3 of 7 for the 4-min. tests are negative. Here is a suggestion that a large number of conscious cues is a disturbing factor in immediate performance.

THE EFFECT OF THE IMMEDIATE TEST ON VERBATIM AND SUMMARY RETENTION

By ALLEN L. EDWARDS and HORACE B. ENGLISH, Ohio State University

Striking systematic differences in retention have been found, in a series of studies of substance learning,[1] which seem to depend in the first instance upon the method of measurement employed. With the passage of time, recognition of sentences which are drawn verbatim from the text studied shows a significant loss similar to the Ebbinghaus curves of forgetting, while recognition of statements which give the substance of paragraphs shows little or no loss—in many cases a net gain or reminiscence effect.

The divergence between the curves of retention is a fact, and can be produced at will, though by no means without considerable effort in devising the measuring instruments. The persistence of the divergence under variation of a considerable number of factors has been investigated with approximately 3,000 *Ss* and in eleven separate experiments. This report deals with the effect of an immediate test upon the retention as measured in the two ways previously referred to. That is, does the rehearsal afforded by a test immediately after learning influence the two types of items in the same way and to the same extent, or does it operate differently upon the two?

Experimental procedure. A total of 155 college students were divided into 6 groups, 5 experimental and 1 control, each group having from 20-30 students in it. These groups were fairly homogeneous in respect to the learning abilities involved, being randomly selected from a single class.[2] Until much more is known about these abilities and their correlations, any rigorous attempt at matched groups is more likely to introduce, than to eliminate, systematic errors.[3] Each of the five experimental groups read and heard read an unfamiliar passage. Group A, given the immediate test after the reading, was retested at 60 and again at 90 days. None of the other experimental groups was given the immediate test. Instead one group, Group B, was tested at 5 days after the reading, another, Group C, at 10 days, Group D at 15 days, and Group E at 20 days. The Control Group which had not heard the passage read at all was given the test twice with an interval of 30 days separating the first and second tests.

The test, with slight modifications, was the same as that used by English, Welborn, and Killian in some of the earlier reports of *verbatim* and *summary* learning at the college level[4] and is therefore not reproduced here. The split-half reliability (corrected) is 0.82 ± .02.

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* Accepted for publication August 12, 1938. This study was presented at the Madison, Wisconsin, meeting of the Midwestern Psychological Association on April 23, 1938. This is the twelfth of a series of studies in substance learning and retention.

[1] H. B. English, E. L. Welborn, and C. D. Killian, Studies in substance memorization, *J. Gen. Psychol.*, 11, 1934, 233-260; H. B. English, Further data on reminiscence, *Psychol. Bull.*, 32, 1935, 688.

[2] Acknowledgement is made to Professor J. E. Janney of Western College who administered some of the tests of his classes.

[3] Cf., on this point, English, The procedure of matched cases—a caution, *J. Educ. Psychol.*, 30, 1939, (in press).

[4] English, Welborn, and Killian, *op. cit.*, 238.

Results. Fig. 1 shows the trend of retention in the two kinds of items. The V's stand for mean verbatim scores; the S's for corresponding mean summary scores. The mean scores of the A Group for immediate, 60-, and 90-day tests are connected by a *straight line;* the mean scores for the two tests of the Control Group are connected by a *dotted line;* and the mean scores of the 5-, 10-, 15-, and 20-day tests (Groups B, C, D, and E) are connected by a *broken line.*

On the assumption that our groups are equivalent—which is subject to the previously mentioned qualifications—the points connected by the broken line represent a retention curve with *elimination of the rehearsal element* involved in successive testings. It can be easily seen that there is a sharp drop in the V-scores and that this is not paralleled at all in the S-scores. For the V-scores, the decline from A (the immediate test) to B (the 5-day test) is statistically significant; the other successive declines, B to C, C to D, and D to E, do not meet the test of significance, perhaps because the populations are small and the P.E. correspondingly large. The total trend is, however, unmistakable and is wholly in line with other experimental data in this series and in other studies.

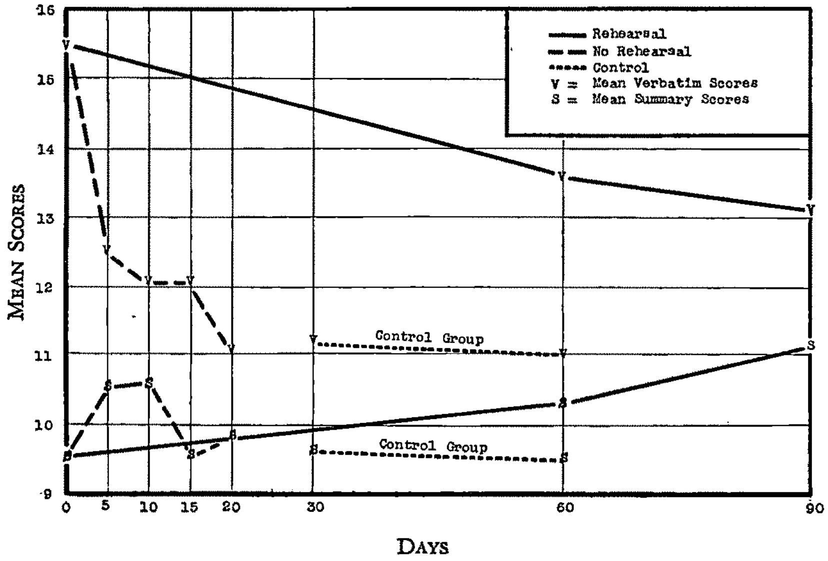

Fig. 1. Retention Curves of V- and S-Items with and without the Immediate Test

Turn now to the same comparison of the S-scores. In all cases there is found a gain in the more remote tests over the immediate. While these gains do not meet the requirements of significance, the important point is the difference between the trend in the V-scores and the S-scores—one definitely downward and the other upward, or at least not down. This difference is obviously significant statistically.

Neither the mean S-score nor the mean V-score for the Control Group showed any significant fluctuation or practice effect from the first to the second test. There is thus one answer: the divergent trends found in the previous experiments are not due wholly to the rehearsal involved in testing immediately after the learning period. There is better net retention—less forgetting, more reminiscence, call it what you will—in *summary* than in *verbatim* even without the immediate test.

None the less the rehearsal afforded by the immediate test is not without its effect. Compare the two *verbatim* curves, the one with and the other without the rehearsal. Both trends are unambiguously downward, but the trend is much sharper where the immediate test is lacking. Or take the two *summary* curves; the trend is slightly upward without the rehearsal, but with it it is somewhat more so.

Table I gives the essential statistics on this point. The difference between the means of the immediate and the 60-day test for the V-scores has a critical ratio of 4.1; for the 90-day loss it is 4.9. The S-scores, on the other hand, show a gain; instead of forgetting there is reminiscence. The gain from the immediate to the 60-day test is not significant but for the entire 90-day period the gain of 1.6 has a critical ratio of 3.5.

TABLE I

Comparison of the Means of V and S Items Over 90 Days
(Group A: N=30)

Mean Scores	Test periods			
	Immediate	60-day	90-day	Diff./P.E.$_{diff.}$
V	15.5±.25	13.8±.32		4.1
		13.8±.32	13.2±.40	1.2
	15.5±.25		13.2±.40	4.9
S	9.5±.33	10.3±.32		1.7
		10.3±.32	11.1±.32	1.8
	9.5±.33		11.1±.32	3.5

These findings indicate rather conclusively that rapid forgetting takes place in the V-items, but is somewhat retarded by an immediate test. In the S sub-test, forgetting is largely replaced by reminiscence. It is difficult to say from the data just what the effect of the immediate test is on the S-items. The S-curve is still rising, though slowly, at the 20-day interval even without the immediate test. It is apparent, however, that the immediate test is not nearly so important a factor in determining the rise in the S-curve, as it is in slowing the downward drop of the V.

It is perhaps worthwhile to emphasize that apparently it is the *immediate* rehearsal which counts. English, Welborn, and Killian found that the general form of his retention curves was but little altered by the dropping out for some of their groups of test-reviews at 8-, 14-, and 30-days. Under comparable experimental conditions with the same test material, we find a considerable change when we drop out only one review—the immediate test.

Discussion. Serious consideration must thus be given the value of relatively immediate testing after study. It is impossible to compare the retention of the A Group, which was given the immediate test, and that of the B, C, D, and E Groups, which were tested at intervals from 5 to 20 days after study, without being impressed by the very great value of some sort of immediate review. Does it have no significance for the teaching situation in our schools and colleges that the immediate review which a test provides—probably better than any other device—is so important in retarding forgetting? Yet, in how many cases is practical heed paid to this fact? It is a fair deduction that lectures and recitations should be closed in time to allow students a brief review or quiz. Students should be more strongly urged to intersperse reviews with their study.

The theoretical implications may be more briefly dealt with. The divergence in retention previously reported does not disappear with the alteration of still another variable. Whatever the interpretation of the divergence, therefore, we believe it is not an artifact of the experimental procedure. We have added one more straw at least to the burden of proof which must be borne by those who defend a qualitatively single learning process.

THE VISUAL PERCEPTION OF FILLED AND UNFILLED SPACE BY THE AFTER-IMAGE METHOD

By ALFRED L. BALDWIN, Harvard University

Koffka has suggested that the stability of the visual field is reduced in the after-image.[1] This view is substantiated by the works of Noll[2] and Rothschild.[3] The results suggest a technique for testing some of the implications of the theory of perception of movement presented by Brown and Voth,[4] and extended to the perception of stable configurations by Orbison.[5] The theory in its present form will not predict the results of the studies of the apparent size of filled and unfilled space. Knox[6] and more recently Spiegel[7] find that the phenomenal distance between two points is least when the interval between them is empty. As the interval is gradually filled by adding new dots to the figure, the phenomenal size increases to a maximum value. Further addition of dots decreases the apparent length of the interval, and the continuous line, while phenomenally longer than the unfilled interval, is phenomenally shorter than the maximum value.

The use of the after-image in the study of this problem seems promising for two reasons. (1) The after-image of an objective situation is distorted in the same direction as the phenomenal appearance of the objective situation. (2) The distortion of the after-image can be objectively measured.

As a first step in the use of this method the writer has undertaken a brief experimental investigation of the after-images of filled and unfilled space. The validity of the method may be tested by its agreement with the results of Spiegel, and the validity of the concept of 'cohesive force' for prediction of the distortion of after-images may be determined.[8]

EXPERIMENTAL PROCEDURE

A light-stimulus of a horizontal extent was presented to *O*. After observation for a sufficient length of time, a screen was lowered in front of the stimulus so that the

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* Accepted for publication October 27, 1938.

[1] K. Koffka, *Principles of Gestalt Psychology*, 1935, 138, 141.

[2] A. Noll, Beiträge zur Psychologie der Gestalt: XIII. Versuche über Nachbilder, *Psychol. Forsch.*, 8, 1926, 3-28.

[3] H. Rothschild, Ueber den Einfluss der Gestalt auf das negative Nachbild ruhender visueller Figuren, [Graefes] *Arch. f. Ophthalm.*, 112, 1923, 1-28.

[4] J. F. Brown and A. C. Voth, The path of seen movement as a function of the vector-field, this JOURNAL, 49, 1937, 543-563.

[5] W. D. Orbison, Shape as a function of the vector-field, this JOURNAL, 52, 1939, 31-45; also 309.

[6] H. W. Knox, On the quantitative determination of an optical illusion, this JOURNAL, 6, 1894, 413-421.

[7] H. G. Spiegel, Ueber den Einfluss des Zwischenfeldes auf gesehene Abstände, *Psychol. Forsch.*, 21, 1937, 327-382.

[8] The experimental work for this study was done in the Psychological Laboratory of the University of Kansas. The writer wishes to thank Dr. J. F. Brown of the University of Kansas and Dr. E. G. Boring of Harvard University for their assistance in preparing this report.

projection-surface was practically the same distance from *O* as the stimulus. On the screen were two pointers which *O* was instructed to adjust until they were at the ends of the interval in the after-image. On the end of each pointer was a small point of light to make the pointers readily visible.

The measurement is made by superimposing a stimulus-field (two points of light used to measure the interval objectively) upon a field of after-image (two or more spots of light). The success of the method depends upon the fact that these two superimposed fields act, each as a whole and each relatively independently of the other. Thus, when the termini of the after-image, moving under field forces, expand or contract the interval, they do not carry the previously coincident perceived points of light with them. These perceived points, being anchored to their stimuli, can be moved to coincide with the changed after-image only by moving their stimuli. It is for this reason that the movement of the stimuli can be used to measure the change in the after-image that occurs under its field forces. This method has been described by Koffka and found to be satisfactory.[9]

In order to assist *O* in maintaining fixation, observations were made through artificial pupils.

Two *O*s (*A* and *B*) were used. They were undergraduates with no psychological training and were naïve with regard to the purpose of the experiments. They were given two weeks preliminary training in the observation of after-images of single points of light. *A* gave results that were more consistent than those of *B*. He also experienced less difficulty in making the observations.

RESULTS

There were five experiments, each of which consisted of 50 observations.

Experiment 1. In this experiment *O* adjusted one point to the after-image of a single point of light 8 cm. to the right of the fixation-point. In a second situation the stimulus was 8 cm. to the left of the fixation-point. These measurements were designed to determine as nearly as possible the perceptual positions of the lights independently of all field forces. The results in the first part of Table I show the two halves of the experiment combined to give the theoretical length of an objective 16 cm. interval. The two *O*s differed rather widely under this condition, but the standard deviation for each is quite small.

Experiment 2. Two lights were presented simultaneously at the same positions as in Experiment 1. According to the Brown-Voth theory the distance between the after-image of two lights should be less than it was in Experiment 1 because of the cohesive force between the two lights. The results in the second part of Table I confirm this theory. For both *O*s the distance between the points of light was decreased, although the decrease was much more marked for *A* than for *B*.

Experiment 3. Five evenly spaced points of light covering the same distance were presented. The theory predicts that the distance will decrease still further unless there should be an increase in 'restraining force' which would cause the interval to approach its objective length. Spiegel found that the interval appears longer under these conditions than under the conditions of Experiment 2. The results in the third part of Table I conform to Spiegel's results. In the case of the more consistent *O*

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[9] K. Koffka, Ueber die Messung der Grösse von Nachbildern, *Psychol. Forsch.*, 3, 1923, 33-41.

(*A*) this increase was enormous. An objective distance of 16 cm. was observed to be 37.5 cm! So large a difference needs checking, but a repetition of the situation for 50 more observations left the results essentially unchanged. The theory is unable to predict this increase.

Experiment 4. A line 16 cm. long was presented. Spiegel's results would indicate that the line should seem to be shorter than the five-dot situation described in Experiment 3. Again the theory would be unable to account for apparent lengths greater than those obtained in Experiment 1. The results of *A* shown in the fourth part of Table I confirm the results of Spiegel's experiment. The results of *B* in this case do not correspond. In addition, the large difference between the two series of trials shows the results of *B* to be unreliable.

TABLE I

THE DISTANCES OF THE POINTERS FROM THE ENDS OF THE OBJECTIVE INTERVAL AND THEIR DISTANCES FROM EACH OTHER

A positive value indicates that the pointer is placed further from the center than the end of the objective interval; a minus value, that it is placed closer. The results under "Combined" were obtained by adding 16 cm. (the objective length) to the sum of the values of the two pointers. Every entry is the average of 50 adjustments.

Experiment No.	*O*	Left pointer		Right pointer		Combined	
		cm.	σ	cm.	σ	cm.	σ
1	*A*	2.4	.9	3.9	.8	22.3	1.2
	B	2.0	.8	−3.7	.7	14.3	1.0
2	*A*	−4	.6	1.0	.4	13	.8
	B	4.7	1.2	−7.4	1.0	13.3	1.6
3	*A* 1st 50	16.7	.8	9.7	.6	42.4	1.0
	A 2nd 50	9.2	.9	8.6	1.0	33.8	1.3
	A av.	13.4	3.9	9.1	.9	38.5	4.0
	B 1st 50	1.4	1.0	−1.6	1.0	15.8	1.5
	B 2nd 50	5.8	1.1	−4.9	.8	16.9	1.3
	B av.	3.6	2.3	−3.2	1.8	16.4	2.9
4	*A* 1st 50	5.0	.9	5.0	.9	26.0	1.3
	A 2nd 50	5.0	.9	11.0	.8	32.0	1.2
	A av.	5.0	.9	8.0	3.1	29.0	3.2
	B 1st 50	11.4	1.1	8.2	.9	35.5	1.4
	B 2nd 50	−1.0	.8	1.0	.4	16.1	.9
	B av.	5.2	6.6	4.6	4.1	25.8	7.9
5a. Repetition of 3	*A*	8.2	1.1	16.2	1.2	40.4	1.6
	B	−3.6	1.0	−3.8	.8	8.6	1.3
5b. Repetition of 4	*A*	9.0	.7	16.0	.7	41.0	.9
	B	−3.5	1.0	.1	.5	12.6	1.0

Experiment 5. This experiment is a repetition of Experiments 3 and 4 without the use of artificial pupils. *A* shows no marked change under the conditions of Experiment 3, but the repetition of Experiment 4 shows a significant increase. *B* shows a marked decrease in the perceived distances. These results are inexplicable at the present time.

DISCUSSION

The effect of the data is to confirm the results of Spiegel and to embarrass the theory of Brown and Voth as it is presented in their report. We should note, however, that the work of Brown and Voth and of Orbison is for conditions which resemble the situation of Experiment 2. In nearly every case these investigations were concerned with the interaction of two objects. Even the experiments reported by Brown and Voth in which there were four objects in the visual field never exhibit more than two objects in a line and are thus reduced to the resultant of two situations in which there are only two objects acting at a time. In the same way Orbison's data are concerned with the interaction of one line up on another, and not with the apparent size of the whole field. It would seem likely, then, that the forces described for the prediction of such phenomena do not predict for other types of situations. What must eventually be done is describe these dynamic systems in more general terms so that the same forces will be 'cohesive' in certain situations and 'expansive' in others.

A notable result of the present experiment is to show that the changes in the after-images under different conditions may be enormous. The distortion that ordinarily occurs in the perception of filled and unfilled space is of the order of 5 to 10% of the length of the line, whereas with after-images one may obtain increases of 100 or even 200%.

SUMMARY

(1) Measurements were made of the after-images of filled and unfilled space.

(2) The data correspond to Spiegel's results in exhibiting first a shrinking and then an expansion as the interval is filled. They do not exhibit the final shrinking as the interval is made continuous.

(3) The data confirm the theory of Brown and Voth in predicting a shrinking, when a situation showing no cohesive forces is changed to one in which cohesive forces are present, *i.e.* from Experiments 1 to 2.

(4) The distortions recorded by this method may be much greater than those obtained in the usual study of illusions of filled and unfilled space.

THE SEX DIFFERENCE ON THE COLOR-NAMING TEST

By PHILIP H. DUBOIS, University of New Mexico

It has frequently been observed that women and girls reply in general more quickly than men and boys when tested with the Woodworth-Wells color-naming test. Ligon presented results based upon the records of 638 *Ss*, ranging from 6 to 18 yr. in age and from the first to the ninth grade in educational placement.[1] He also used the word-reading test in which the names of five colors were read from typewritten sheets. From the fact that the differences were greater and statistically more reliable with the color-naming test than with the word-reading test, he argues that the sex difference on the color naming test is based on a "color naming special factor," rather than on verbal facility.

Ligon was interested only incidentally in the sex difference, as his experiment, like most of those reported in which the color-naming test is used, was chiefly concerned with the effect of practice.

The present experiment represents an investigation of Ligon's hypothesis. The *Ss* were 149 men and 133 women, all junior college students, mostly of English and Scandinavian extraction. Records of several other *Ss* who did not show normal color vision on the Ishihara test were discarded.

Three tests were used: the Woodworth-Wells color-naming test with oral responses, the word-reading test, and a non-verbal form of the Woodworth-Wells test. For the latter a board was prepared on which was pasted a rectangle, 3 × 5 in. in size, of each of the following five colors: red, green, yellow, blue, and black. *S* indicated his response by tapping with a stylus on the rectangle corresponding to the stimulus-color. Every test, involving 100 responses, was given twice at a single sitting, but in the two trials the order of responses was different. The sequence of tests was: color-naming A; word-reading A; non-verbal A; non-verbal B, word-reading B, and color-naming B. Scores were recorded in terms of the number of seconds necessary to complete each test. When an error was made *S* was required to correct it. The number of errors was negligible.

TABLE I

CORRELATIONS OF THE TWO TRIALS OF EVERY TEST

Test	Men (N=149)		Women (N=133)	
	Obt.	Sp. Br.	Obt.	Sp. Br.
Color-naming	.56	.72	.83	.91
Word-reading	.80	.89	.86	.93
Non-verbal	.88	.93	.88	.93

Reliabilities of the tests were computed by correlating scores on the first and second trials of each test. Since intercorrelations were computed using the sums of the scores on the two trials, reliabilities are also presented as estimated by the Spearman-Brown formula. Reliabilities corrected to two decimals are given in Table I.

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* Accepted for publication December 10, 1938.

[1] E. M. Ligon, A genetic study of color naming and word reading, this JOURNAL, 44, 1932, 103-122.

The higher reliability of the non-verbal test may have resulted from the fact that the two trials followed one after the other, while the color-naming test appeared at the beginning and end of the series.

Table II gives means, standard deviations, differences between means of men and women and critical ratios (D/S.D.$_{diff.}$) for the two trials and for summed scores on each test.

It will be noted that differences on all tests favor the women in that they have lower scores. On the first trial the difference on the color-naming test is to be considered significant according to the conventional standard. On the word-reading test the differences cannot be considered completely reliable. The greatest differences and the differences most reliable statistically are those on the non-verbal form of the Woodworth-Wells color-naming test. This test involves a double response to color; first on the stimulus-sheet, and second in making the choice among the rectangles of color. A plausible interpretation is that the non-verbal test involves Ligon's "color-naming special factor" to a double degree. Perhaps this factor should be re-christened a "color recognizing special factor." The results give clear-cut support to the hypothesis that the sex difference on the color-naming test is not to be attributed to differences in verbal facility but are due to the operation of a special factor.

TABLE II

THE PERFORMANCES OF THE MEN AND WOMEN IN THE DIFFERENT TRIALS WITH EVERY TEST

Test	Men (N=149)		Women (N=133)		D	D/S.D.$_{diff.}$
	Mean	S.D.	Mean	S.D.		
Color-naming A	66.60	11.19	62.25	10.80	4.35	3.31
Color-naming B	63.51	9.72	62.04	11.27	1.47	1.17
Total	130.12	18.46	122.18	20.99	7.94	3.36
Word-reading A	43.48	7.09	41.49	7.55	1.99	2.27
Word-reading B	39.96	7.18	38.40	6.17	1.57	1.97
Total	83.44	13.53	79.86	13.08	3.59	2.26
Non-verbal A	89.90	11.95	80.87	12.49	9.03	6.19
Non-verbal B	87.41	12.75	79.71	13.04	7.70	5.00
Total	177.32	23.94	160.14	24.86	17.18	5.90

The evidence presented in Table II is confirmed from a study of the medians. Medians of total scores are given in Table III.

TABLE III

MEDIANS OF THE TOTAL SCORES OF THE VARIOUS TESTS

Test	Men	Women
Color-naming	127.65	119.35
Word-reading	81.86	78.53
Non-verbal	176.64	158.50

On the color-naming test, 36.7% of the men attained the median score of the women, compared with 40.6% on the word-reading test, and only 19.9% on the non-verbal test.

Intercorrelations. Intercorrelations of the three tests, and partial correlations with the variability due to the third test held constant are presented in Table IV.

There is some indication that the abilities measured by the tests are somewhat differently organized in men and women. The greatest discrepancy in zero order correlations is between word-reading and the non-verbal test. The difference in the two correlations is 0.22. The critical ratio of the difference ($D/S.D._{diff.}$) is 2.2, showing that the chances are approximately 99 in 100 that there is a difference greater than zero.

TABLE IV

INTERCORRELATIONS AMONG THE THREE TESTS

Tests	Men		Women	
	Zero order r	Partial r	Zero order r	Partial r
Color-naming and word-reading	.51 ± .04	.36	.61 ± .04	.57
Color-naming and non-verbal	.53 ± .04	.34	.53 ± .04	.47
Word-reading and non-verbal	.49 ± .04	.31	.27 ± .05	−.07

With the men, partial correlation reduces the correlations somewhat, but all tests reveal some degree of relation. With the women, however, the correlation between word-reading and the non-verbal Woodworth-Wells test becomes slightly negative when the variability due to the color-naming test is held constant.

Brown and Thompson state that when for three variables $r^2_{ab} + r^2_{ac} + r^2_{bc} + 2r_{ab}r_{ac}r_{bc}$ is significantly greater than unity there must be a common factor running through the tests.[2] For the men, this value is 1.05, and for women, 0.90. There is some evidence, therefore, that the three tests show a common factor—possibly a speed factor—for men but not for women.

Conclusions. The results of this study lead to the following conclusions.

(1) Ligon's hypothesis of a "color-naming special factor" rather than a verbal factor accounting for the sex difference on the Woodworth-Wells color-naming test is confirmed.

(2) The sex difference is increased if non-verbal responses involving tapping on the appropriate color are substituted for the usual oral response. On this non-verbal test the difference between the sexes is striking, with only 19.9% of the men attaining the women's median.

(3) There is some evidence of difference in the organization of the abilities measured by the three tests in the two groups of *S*s.

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[2] W. Brown, and G. H. Thompson, *Essentials of Mental Measurement,* 1925, 142.

THE INTER-SOCIETY COLOR COUNCIL SYMPOSIUM ON COLOR TOLERANCE

INTRODUCTION

In the first Articles of Organization and Procedure of the Inter-Society Color Council there occurs the following statement: "The aims and purposes of the Council are to stimulate and coördinate the work being done by various societies and associations leading to the standardization, descriptions and specification of color, and to promote the practical application of these results to the color problems arising in science, art and industry." In furtherance of these aims, successive administrations of the council have sponsored a wide variety of activities. Problems of a concrete nature have been assigned to individuals and to committees, and attempts have been made to crystallize questions that were in a more nebulous state. In addition, it has been evident from the beginning that a mutual exchange of ideas performs a valuable function where questions are not sufficiently formulated. As a result, in the business meetings, committee reports have become veritable symposia. Miss Nickerson has shown, in her historical review of the activities of the Council, how more extensive programs were added to its annual meetings. There was at first, I believe, no clearly outlined policy. The executive Committee made use of available program material in an exploration of the desirability of expanding Council activities in that direction. The answer was unmistakable and last year Mr. Little demonstrated what could be done with a technical program built about a special topic. We have followed much the same plan this year, without the implication, however that we are establishing a permanent policy. It is to be expected that the organization will remain responsive to new and changing demands from workers in the field of color, and that its activities will evolve accordingly.

The present group of papers centers about a topic with which every person who deals with colors must eventually be concerned. That does not mean that the problem will appear in the same light from all angles. Indeed, the certainty that it would not made it appear especially suitable for sponsorship by the Council. The particular papers were invited with the expressed understanding that their function is not to present new technical data to workers in their special field, but to acquaint those in other fields with established facts and techniques. It was recognized that color tolerance is in large measure a psychological matter, and therefore the major call for contributions was made upon psychologists. Nevertheless, other aspects of the problem were not lost to sight. We may have been too ambitious, but it is our hope that by considering it from various angles, color tolerance may be seen in a new light from every point of view, and in this way old difficulties be resolved and new problems be formulated that will carry all of us a step forward.

Hobart College FORREST LEE DIMMICK, Chairman

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* Accepted for publication February 1, 1939. These papers were presented at the eighth annual meeting of the Inter-Society Color Council in coöperation with the American Psychological Association, New York, February 23, 1939.

THE PSYCHOPHYSICS OF COLOR TOLERANCE

By Edwin G. Boring, Harvard University

Any particular color tolerance can and must be measured in terms of the physical units of the color-stimulus, since the stimulus-scale is an ultimate system of reference. One hopes, nevertheless, for some general statement of the limits of any particular kind of tolerance, and it would seem that such a generalization must be more likely to be expressible in the units of some psychological scale which is definitely but perhaps not linearly related to the physical scale of the stimulus.

Tolerance is a matter of perception and must therefore be related to discrimination. There must be as many kinds of tolerance as there are reasons for fixing the character of colors, and two colors that are not noticeably different would necessarily lie within the range of all tolerances, if they are really not discriminable. That sentence is, however, much too simple to express the whole truth. While many tolerances are supraliminal, certainly there are others which are subliminal. The difference limen is the average jnd (just noticeable difference), that is to say, it is the difference which is as often perceived as not. A difference that is perceived only forty per cent of the time is subliminal, yet it is perceived almost, though not quite, as often as not. Complete intolerance, a requirement that two colors should never be perceived as different, would specify a difference that is far below the limen.

There are three psychological scales, any one of which might be used for measuring tolerance. (1) The first is the DL (difference limen). It can be chosen as the unit, and a tolerance specified as a permissible number of DL. (2) A more stable measure is σ (standard deviation) of the psychometric function. (3) The third measure is the sense-distance. Since these three measures are, unfortunately, almost the private property of the psychologists, I must undertake to explain them now, to show you their natures and disadvantages. My thesis is that, for the measurement of tolerances, the DL has the most disadvantages, and the sense-distance the least. Unfortunately it is the DL that has been used most often by psychologists, whereas the sense-distance has hardly been used at all.

(1) The difference limen. The physicist, when he encounters a problem of psychological measurement, generally works with the jnd. He changes one of two stimuli until the two impressions are just noticeably different, or else he decreases the difference until the two are just not noticeably different. Then he takes an average and calls it the jnd. I fear that sometimes he does not take more than four, or perhaps ten, readings, thus failing in proper respect for the great variability of the organism. Nevertheless in principle his procedure is correct: the average jnd is the DL.

Since this statement may not be immediately obvious to everyone, we had better validate it. Fig. 1 is a schematic diagram for showing why the average jnd is the point where a perception of difference is just as likely to occur as not.[1] The seven-step distribution curve is supposed to show how the organism varies in its sensitivity to some specified kind of difference. When the organism is momentarily favorably disposed toward perceiving the difference, then a small difference will elicit a jnd. The heights of the steps of this curve are 1, 6, 15, 20, 15, 6 and 1—a total of 64 cases. That is to say, once in 64 times the organism will be so well disposed toward the perception of a difference that a small difference, represented by the stimulus-difference 8 will be perceived. If the stimulus-difference is as great as 9, then it will be perceived 7 times out of 64—the once when the disposition of the organism is what is called '6' in the diagram, and also the 6 times when the disposition is '5.' The tall ogive is the integral curve, the sum of the values on the distribution curve. It shows that at a middle value of the stimulus-difference—actually a value of 10.5—the difference will be sensed just as often as not, or the frequency of the perception of the difference will be 50%. This value is the DL and is the same as the average jnd. The ogive curve is called a *psycho-*

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[1] E. G. Boring, A chart of the psychometric function, this JOURNAL, 28, 1917, 465-470; J. P. Guilford, *Psychometric Methods,* 1936, 172.

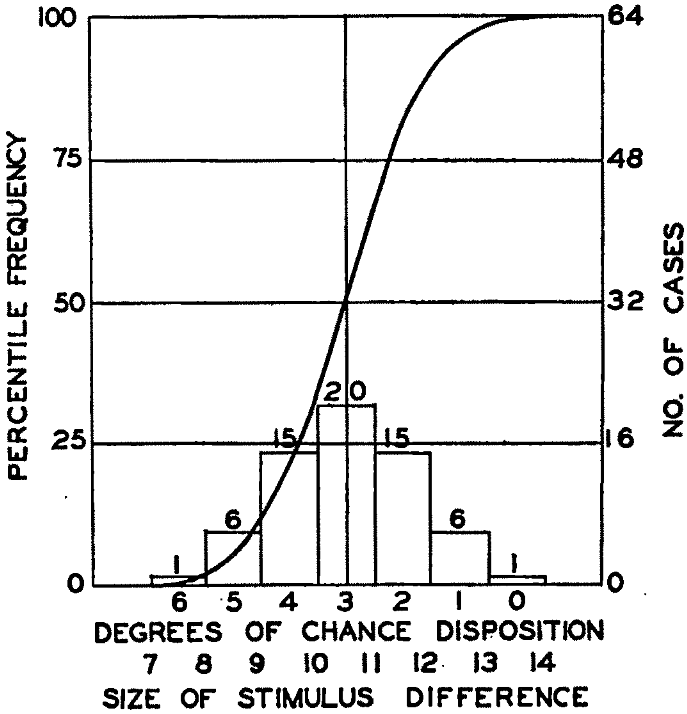

FIG. 1. RELATION OF THE PSYCHOMETRIC FUNCTION TO THE VARIABLE DISPOSITION OF THE ORGANISM

The degrees of chance disposition of the organism are shown in the seven-step distribution curve. The heights of these steps are 1, 6, 15, 20, 15, 6, 1—64 cases in all. The tall ogive is the psychometric function; it shows the sum from left to right of the levels in the other curve. The abscissa-scale shows the stimulus-differences that are candidates for being perceived. See text as to why the DL, the average jnd, lies at the midpoint 10.5. After E. G. Boring, this JOURNAL, 28, 1917, 466.

metric function, and the first advantage of its use is that it directs attention to the variabiliy that underlies all discrimination. When the physicist determines an average jnd he is likely to think of variability as an error to be got rid of by averaging, whereas it is, as we shall see presently, just exactly this spread of variability that measures discrimination.

Fig. 2 shows a typical set of psychometric functions for the perception of lifted weights.[2] The psychometric function for the judgement *greater* is a frequency function which shows how often the comparison weights were judged *greater* than the standard of 100 gm. There is a comparable function for the judgment *less.* The two curves do not, however, add up to 100% because the judgment *equal* was also allowed. The frequency of the use of the *equal* category is shown in the third curve. The lower and upper limens, L_l and L_u, are placed by definition at the 50% points. The upper limen is where the judgment *greater* is just as likely to occur as not; when it does not occur, the judgment may be either *less* or *equal.* Because of the use of the *equal* category, the two limens are not coincident, and their size is really dependent upon the frequency with which equality appears in the data. We shall return to this point in a moment.[3]

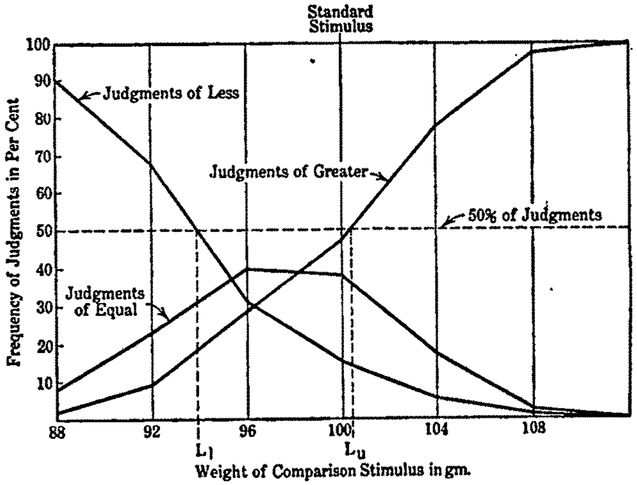

FIG. 2. PSYCHOMETRIC FUNCTIONS FOR LIFTED WEIGHTS
(A representative set. After D. W. Chapman in E. G. Boring, H. S. Langfeld, H. P. Weld et al., *Psychology: A Factual Textbook,* 1935, 55.)

Now the real trouble with the DL as a measure of tolerance lies in the fact that its size depends upon the number of *equal* judgments, and that the use of that category can not always be controlled. In Fig. 3 you see a hypothetical case where the range of equality is very large. The middle curve is for the *equal* judgments, and you see how its rise has forced the liminal points, L_l and L_u, apart. On the other hand, if the *equal* judgments fall off as they do in the low curve in Fig. 4, the limens get very small. If the judgment of equality is eliminated entirely, as in Fig. 5, then the limens also disappear entirely. The two remaining psychometric functions, one for *greater* and one for *less*, now add up to 100%: either entirely determines the other.[4]

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[2] D. W. Chapman in E. G. Boring, H. S. Langfeld, and H. P. Weld, *Psychology: A Factual Textbook,* 1935, 55.

[3] The fact that, due to the 'time-error' of successive comparison, these particular curves are assymetrical about the value of the standard weight, 100 gm., need not concern us here, since most visual comparison is simultaneous.

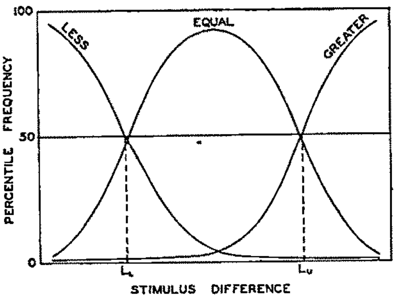

FIG. 3. PSYCHOMETRIC FUNCTIONS WITH LARGE EQUAL CATEGORY AND LARGE LIMENS

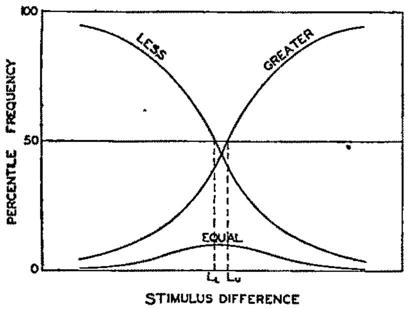

FIG. 4. PSYCHOMETRIC FUNCTION WITH SMALL EQUAL CATEGORY AND SMALL LIMENS

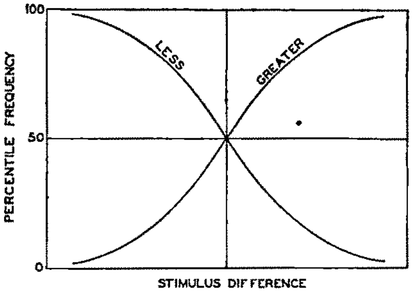

FIG. 5. PSYCHOMETRIC FUNCTIONS WITH NO EQUAL CATEGORY AND VANISHED LIMENS

It has been found that the number of *equal* judgments can be altered by changing the attitude of the observer toward this middle category of judgment. In some cases the observer may use the category quite liberally, but then it becomes possible to diminish its use or even to abolish it altogether. If you say to an observer: "Of course, you know that no two stimuli can ever be exactly the same; there is always some difference; try your best to see the difference,"—if you say that, then the observer will give less *equals* and more *greaters* and *lesses* and the psychometric functions may look like Fig. 4. If you say: "Since the stimuli are never really equal, you must guess the difference; the fact is that people more often guess right than wrong under such circumstances,"—if you say that, then you are likely to get the situation in Fig. 5, where the *equal* category has disappeared and the limens are zero.

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[4] Cf. in general Guilford, *op. cit.*, 194, 196.

Now I have not made up this psychophysical trouble out of my imagination. It is a well-established fact that the attitude toward the use of the *equal* category varies somewhat adventitiously and is not easily controlled. Fernberger showed how instructions will diminish or abolish the category. Its rejection has been advocated ever since 1884 and the standard handbooks now point out the trouble.[5]

(2) The standard deviation. Even when the limens disappear as in Fig. 5, there is left a measure of sensitivity. It is represented by the spread of these psychometric functions. We can use as a unit the semi-interquartile range (half the distance from 25% to 75%), the probable error (which is the same thing in a symmetrical case), the standard deviation (σ), or the modulus for a normal distribution (h).[6] These measures are all related, and we shall speak of σ as the unit. The great advantage of the use of σ is that it is free of the uncontrolled *equal* category. We tell the observer that there is a difference whether he is sure of it or not, that he must observe it or guess it, and then we measure the spread of his responses by σ. In it we have a more constant and reliable unit for sensory discrimination than we had in the DL.

It has been argued that σ and the DL vary together, and that there is therefore no reason why σ should not be substituted for the DL. Such a relationship as a generality is, however, manifestly impossible, since change of attitude toward the equal category may not affect σ and must affect the DL. Holway has recently shown that for certain special conditions the DL for visual brightness varies with its σ when the level of intensity is changed, but this is a special case.[7] Although Holway did not explicitly control the attitude of his observers toward equality, he arranged his procedures so that it would be impossible for *equal* to disappear entirely. A different procedure might give very different values for the DL. At any rate the substitution of σ is desirable because it avoids the constant suspicion under which the DL labors.

It has been suggested that the use of σ may not be practicable in the case of simultaneous visual comparison of two apposed fields, for the reason that it might prove impossible to eliminate the *equal* category of judgment. Ordinarily the judgment *equal* in such a case involves the disappearance of the contour that separates the fields. Would the disappearance of the contour compel the judgment *equal?* We need experimentation to settle such a point, but it seems improbable that we have here to do with a special case. What is called a 'homogeneous' visual field never appears entirely homogeneous to the most careful inspection. It varies from moment to moment and from point to point, as the eyes move and adaptation takes place; and total field which has lost a dividing contour would still present a problem of decision between its two halves. There is little reason to expect that the *equal* judgment could prove compulsory here.

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[5] For the history and references, see S. W. Fernberger, The use of equality judgments in psychophysical procedures, *Psychol. Rev.*, 37, 1930, 107-112; also E. Culler, Studies in psychometric theory, *Psychol. Monog.*, 35, 1926, 97-122; Guilford, *op. cit.*, 192 f., 201-205; R. S. Woodworth, *Experimental Psychology*, 1938, 421-425. I confess here to having once been on the other side of this argument: Boring, The control of attitude in psychophysical experiments, *Psychol. Rev.*, 27, 1920, 440-452.

[6] Culler, Guilford, and Woodworth, *locc. citt.*

[7] A. H. Holway, On the precision of photometric observations, *J. Opt. Soc. Amer.*, 27, 1937, 120-123.

(3) The sense-distance. It is quite possible for an observer directly to compare two supraliminal sense-distances, and it would seem that such a measure must provide a correct unit for color tolerance. The DL and σ are measures of frequency of perceived difference. In questions of tolerance one is more likely to want to know how great the perceived difference actually is than how often the difference will be perceived. Wide limits of tolerance when measured in terms of the DL or σ are merely multiples of some small difference perceived with a given frequency, whereas measured in sense-distance a statement of tolerance has direct significance.

A sense-distance is determined by its two limiting points; hence it follows that a pair of sense-distances would be fixed by four points. Suppose that A, B, C and D represent four colors. Then an observer can be asked to say whether the color-distance from A to B is greater or less than the distance from C to D. The fact that this distance is empty does not necessarily make the judgment difficult. If A, B, C and D are four points marked in visual space, it is not hard to say whether the distance AB is greater or less than the distance CD, even though the lines AB and CD are not drawn in to fill the visual vacuum.

Actually there are available at least four methods for the measurement of sense-distances.

(a) The *method of equation* is the one I have just described.[8] Two pairs of limiting values are given and at least one limit is varied. Is AB greater or less than CD?

(b) The *method of bisection* is more usual.[9] In it three successive values are given, A, B and C. One of them, usually the middle one, B, is varied until the distance AB equals BC. This method is, of course, the method of equation with the two distances coterminous in the middle.

(c) In the *method of halving* there are only two stimuli. A variable stimulus is adjusted so that its sensation B appears to be half as great as a constant sensation A. Logically there is implicit in this procedure a third point, zero: B is made to bisect the distance between zero and A. This method is new but has been used successfully for the measurement of the loudness of tones[10] and also, surprising as that may seem, for the measurement of their pitch.[11] Other researches applying it to visual brightness and to times are said to be in progress.

(d) There is also possible a *method of fractionation,* which is like the method

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[8] The method of equation is similar to the classical method of equivalents; cf. Guilford, *op. cit.,* 134 f.

[9] The method of bisection is essentially the classical method of equal sense-distances; see Guilford, *op. cit.,* 141-165; D. B. Judd, Saturation scale for yellow colors, *J. Opt. Soc. Amer.,* 23, 1933, 35-45, was in principle using this method.

[10] See S. S. Stevens and H. Davis, *Hearing,* 1938, 112-114, 117-120, and references there cited.

[11] See Stevens and Davis, *op. cit.,* 76-81, and references there cited.

of halving but uses fractions other than the half.[12] B may be adjusted so as to be one-third as great as A, or the stimuli may remain unchanged and the ratio of the distances between them estimated.

The first two methods are historically the oldest and they apply to different cases. The method of equation must be used in equating sense-distances that can not be coterminous, as, for example, the equation of a difference in lightness of two greens to a difference in brightness of two yellows. The method of bisection, being simpler, is desirable whenever two distances can end in a common point. The method of halving is simpler to administer since it is a bisection with one stimulus omitted, and the proof of its value will lie in its successful use. It is starting out well. The method of fractionation has been used even less, and I should not expect it to give as constant results as the other methods. The ultimate judgment in scientific measurement is the judgment of *equal* or *not-different*. In the methods of bisection and halving, the observer is really making an equation between the two halves. In the method of fractionation, if the ratio required be one-third, the observer has to judge that the given third is half of the remaining two-thirds, an involved judgment. One has only to remember what it is like to estimate two-sevenths of a specified distance to see how complicated with imaginal points fractionation may become.

It becomes plain that there are available two general measures of sensation: the DL and the comparable but more accurate σ of the psychometric function, on the one hand, and the sense-distance on the other. Are the two fundamentally different or the same? In other words, do all DL—we speak here about DL since σ has not yet been widely used in this context—do all DL represent the same sense-distance? That is the present form of the eighty-year old problem: are all jnd equal? And the answer to it is: No, not necessarily. The evidence is new and it comes from the field of hearing. The DL for pitch seem to represent approximately equal sense-distances,[13] but the DL for loudness do not.[14] When the loudness of a tone, as determined by the method of halving, is plotted against the number of DL that the tone lies above its absolute threshold, the function is not linear. The sense-distance that corresponds to the DL increases with the loudness. In general it increases most for low tones and less for high tones. The corresponding data for visual lightness have not yet been determined, although Judd makes it appear that DL for the saturation of yellow represent about equal sense-distances.[15]

There is a good deal of resistance to accepting this conclusion about the DL because it seems to be contrary to common sense. It is said that jnd must all be equal because they are all just noticeable. As a matter of fact, in that sense, they are equal: they are determined by identical operations and must be equal. For the same reason all DL must be equal in the sense that they represent the same frequencies of judgments. Fifty per cent must equal fifty per cent. The important point, however, is that, although all DL may be thus operationally equivalent, they are not necessarily equal *as sense-distances,* for the operations used in their measurement as sense-distances are different from the operations of their measurement as frequencies. The fact is that the frequency with which a difference is perceived is no guarantee of the exact size of the difference that is perceived. Similarly there is no particular reason to suppose that all jnd are equal. The jnd of the instant occurs under a release mechanism; something breaks up the levelling tendency and the two sensations jump apart, so that suddenly, with continuous change of the stimulus, a difference is perceived. The jump, however, may be large or small. All the evidence now points to the fact that that distance which is jnd is not necessarily always the same size when it is measured as a sense-distance.

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[12] The method has been used by P. H. Geiger and F. A. Firestone, The estimation of fractional loudness, J. *Acous. Soc. Amer.*, 5, 1933, 25-30, and also in this symposium by S. M. Newhall, The ratio method in the review of the Munsell colors, *infra*.

[13] Stevens and Davis, *op. cit.*, 84-89, 95-97.

[14] Stevens and Davis, *op. cit.*, 147-152.

[15] Judd, *loc. cit.*

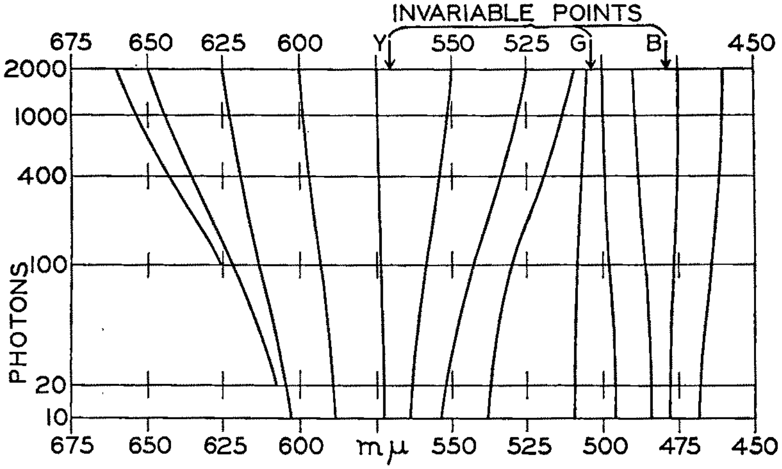

FIG. 6. CONTOURS FOR CONSTANT HUE ON THE SPECTRAL DIAGRAM: THE BEZOLD-BRÜCKE PHENOMENON

(From D. M. Purdy, this JOURNAL, 49, 1937, 314.)

A diagram for sense-distances. Scientists interested in color ought to think more about a diagram for sense-distances. Even if no one ever had the time to determine the 10,000 values that might make it possible to construct such a diagram for monochromatic lights, still contemplation of the problem would impress investigators with the need for more data of the sort that would be especially useful in the measurement of tolerances. May I describe, for this purpose, the diagram that I should like to see constructed?

We should have first, I suggest, what might be called a spectrogram for the *lightness* of monochromatic lights. (The term *spectrogram* is analogous to *audiogram* in researches on hearing.) It would be a graph plotted against wave-length and energy for constant lightness, a family of curves with lightness varying as the parameter. Such graphs are the plots of the basic functions of which the visibility curves are the reciprocals, and we have had such data ever since König. I want, however, to have the different curves of the family separated by equal sense-distances of lightness. Thus this spectrogram would consist of a batch of U-shaped curves, which measure lightness as well as wave-length and energy. In other words, the total graph would be really a representation of a surface plotted against three dimensions—wave-length, energy and lightness—and giving the interrelation of these three variables for pure light. It would be a trough with its longitudinal axis perpendicular to the plane of the axes for wave-length and energy.

Next I want the corresponding family of curves—that is to say, the corresponding surface—for wave-length, energy and *hue*. This is the surface that gives the complete summary of the Bezold-Brücke phenomenon, and Fig. 6, giving Purdy's data for this phenomenon, shows what the surface might be like.[16] It happens that the ordinate here is scaled in photometric instead of radiometric units, but that does not matter. Each curve shows how, when energy is changed, wave-length must also be changed if hue is to remain constant. The diagram gives these functions for 13 hues, but it makes no assumptions at all about the sense-distances between these hues. If the separations of the hues were known, then hue could be plotted perpendicular to this plane and we should have a surface that is slightly ogival, bending one way in the middle and the other way at the two ends.

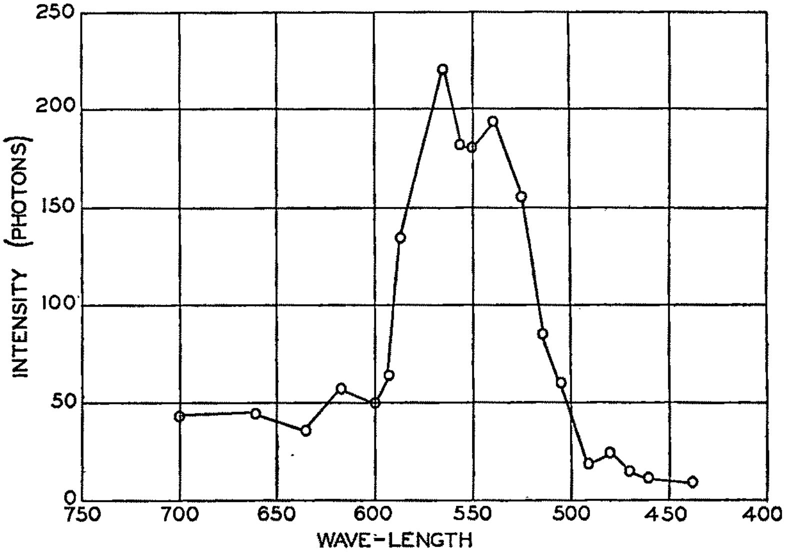

FIG. 7. PLAN VIEW OF CREST OF MAXIMAL SATURATIONS ON THE SPECTRAL DIAGRAM

(From D. M. Purdy, *Brit. J. Psychol.,* 21, 1931, 303.)

Next I want a family of curves, and thus a surface, for the relation of wave-length, energy and *saturation*. Maximal saturation occurs at middle energies: very light and very dark colors are poorly saturated. Fig. 7 shows Purdy's determination of the energies which, for different wave-lengths, give maximal saturation.[17] One can think of this curve as the plan view of the crest of a ridge of saturation, in a diagram in which saturation is plotted vertically on a base for which wave-length and energy are the axes. This ridge, however, varies in height. Its profile would be given by the measurement of the different maximal saturations. It might look like Fig. 8, for here is the curve of Jones and Lowry measuring spectral saturation by the counting of DL's.[18] I have had to draw the curve down at each end, because it seems plain that the spectrum loses in saturation as well as lightness as it passes into invisibility. There are no well-saturated very dark colors.

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[16] D. M. Purdy, The Bezold-Brücke phenomenon and contours for constant hue, this JOURNAL, 49, 1937, 313-315.

[17] Purdy, On the saturations and chromatic thresholds of spectral colours, *Brit. J. Psychol.,* 21, 1931, 283-313, esp. 302-304.

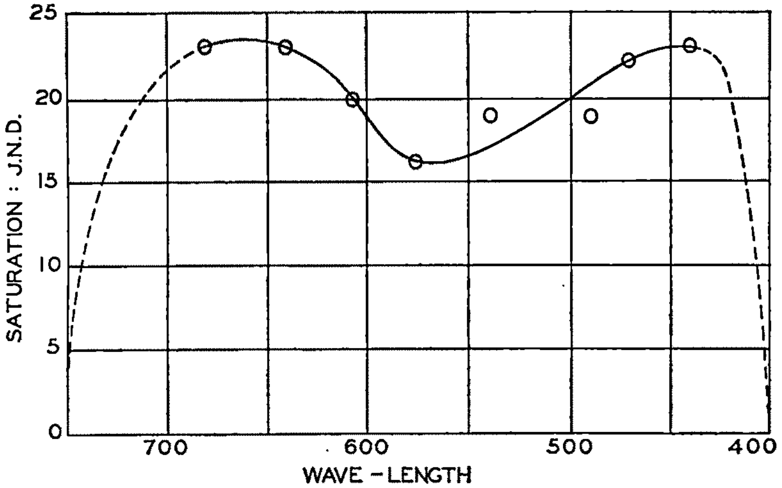

FIG. 8. ELEVATION OF CREST OF MAXIMAL SATURATIONS ON THE SPECTRAL DIAGRAM

(The data are from L. A. Jones and E. M. Lowry, *J. Opt. Soc. Amer.*, 13, 1926, 25-34, but the dotted extensions are the present author's idea.)

Now the lines of constant saturation would be the contour lines for this ridge. Fig. 9 shows one such contour.[19] The dotted line is the crest of the ridge—Purdy's data. The data of Jones and Lowry have been used to show how far down the ridge a contour of constant saturation-level might lie. Actually we want a set of these contours spaced at equal distances of saturation. Since the ridge has two peaks, the highest contours would be two separated rings. It is not, however, possible to draw them because we do not have the necessary measurements.

These three sets of facts together represent a surface in five dimensions. It is the complete spectrogram for monochromatic light. Its dimensions are wave-length, energy, hue, lightness and saturation. Since the figure is a surface, any two of the coordinate values determine the other three, once the shape and position of the surface are fixed.

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[18] L. A. Jones and E. M. Lowry, Retinal sensibility to saturation differences, *J. Opt. Soc. Amer.*, 13, 1926, 25-34.

[19] E. G. Boring, Isochromatic contours, this JOURNAL, 49, 1937, 130-134.

It is in some such diagram that color tolerances might best be plotted. They would appear as little closed cells surrounding a point. They would not necessarily be five-dimensional hyperspheres, because the diagram would not assert any equivalence between a difference in hue and a difference in lightness. One might, however, hope—and this is my sole excuse for mentioning this elaborate figure—to get constant diameters for the cells parallel to the axes for hue, lightness and saturation, since they are perceptual variables; that is to say, one might hope that, for a given purpose, tolerance for hue would be linearly related to sense-distance for hue, and similarly in respect of the tolerances for lightness and saturation. The measures of tolerance would not then show a linear relation to wave-length and energy, since these dimensions of the stimulus are not linearly related to sense-distance. In other words, perceptual scales would be found best for measuring perceptual differences.

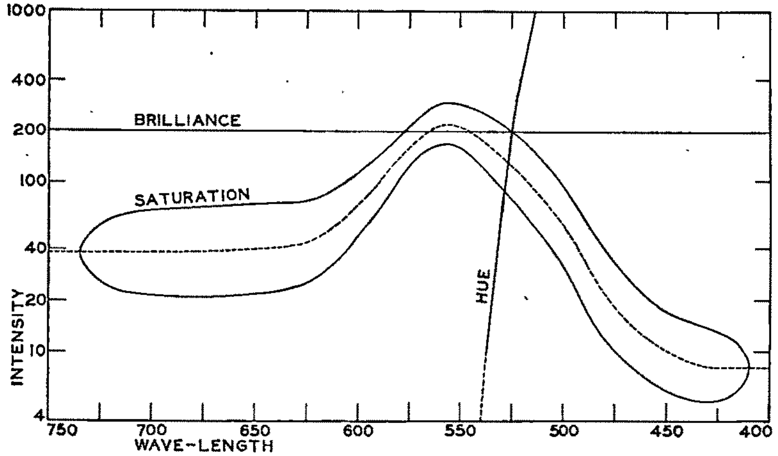

FIG. 9. ISOCHROMATIC CONTOURS FOR HUE, LIGHTNESS AND SATURATION ON THE SPECTRAL DIAGRAM

(From E. G. Boring, this JOURNAL, 49, 1937, 132.)

THE RATIO METHOD IN THE REVIEW OF THE MUNSELL COLORS

By SIDNEY M. NEWHALL,* Johns Hopkins University

There are several reasons why the review of the spacing of the Munsell colors is of interest in connection with the special subject of color tolerances. The ratio method,[1] which has been relied upon from the beginning in this review, not only seems suitable for color tolerance determinations, but is actually being used for that purpose by Judd[2] and by Balinkin.[3] The present study provides the test of further use under complex conditions for this new method of color estimation.

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* Chairman of the Subcommittee of the Colorimetry Committee on the review of the spacing of the Munsell Colors.

[1] D. B. Judd and D. Nickerson, Review of the spacing of the Munsell colors, *Mimeo. Statement,* (April) 1936, 1-6.

The perfection of the Munsell spacing would provide a suitable system for the psychological expression of color tolerances. There is a need for such a system, and belief in its eventual availability has already lead to the development by Nickerson of a tolerance formula for use with it.[4] Incidentally, whether or not technical interest lies in tolerance problems, an adequate direct color comparator system would have the advantages of great facility and wide application.

This paper describes the ratio method and its application to the spacing problem and presents some preliminary results.

The ratio method. The ratio method is psychological and implies psychological measurement, but the general question of the possibility of color measurement need not detain us now. Color sensations may be unitary or indivisible, but they have magnitude; sensations can not be added or multiplied, but numerical values representing the magnitudes of their attributes can. The ratio method is employed in the important operation of estimating or assigning the values to represent the magnitudes.

This method apparently originates with L. F. Richardson, and it was first applied by him to color.[5] As generalized, it consists in the estimation by direct impression of the ratio of supraliminal sense magnitudes. The one magnitude is given or taken as standard, and the ratio of the other to it is then estimated directly.

There are really two forms of this method, both of which are applicable to color problems. The first form may be referred to simply as the ratio or R form and represented by the simple operational equation

$$(C_2/C_1)_A = R_A$$

in which C_1 represents the chosen standard or comparison color; C_2, the color to be evaluated; R, the estimate of the ratio; and the subscript A, the attribute with respect to which the estimate is made. Thus, the given color C_2 is judged equal to RC_1 with respect to A. Suppose, for instance, that C_1 and C_2 represent two patches of color and the observer's task is to estimate their relative lightness. He compares the two colors directly with each other and reports a quantitative judgment concerning the fraction or multiple which the lightness of C_2 is of C_1. In case of surface colors, A may become either H, S, or L, according to whether hue, saturation, or lightness is the attribute being estimated.

The second, or difference ratio method, is designated by R′ and represented by the analogous equation

$$(|C_4 - C_3|/|C_2 - C_1|)_A = R'_A$$

in which $|C_2 - C_1|$ is the perceived standard difference or interval, $|C_4 - C_3|$ is the difference to be evaluated, and R′ is the estimate of the ratio of intervals with respect to A, the particular aspect under estimate.

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[2] D. B. Judd, Specification of color tolerances at the National Bureau of Standards, *infra*, 418-428.

[3] I. A. Balinkin, Industrial color tolerances, *infra*, 428-448.

[4] D. Nickerson, The specification of color tolerances, *Textile Research*, 6, 1936, 505-514.

[5] L. F. Richardson, Quantitative mental estimates of light and colour, *Brit. J. Psychol.*, 20, 1929, 27-37.

The observer records his estimate either by linear extents or by numbers which seem to him to be in the ratio of the sense magnitudes in question. Perhaps the easiest method is to adjust the relative positions of the samples themselves until a space ratio represents the sense ratio. This method was demonstrated by throwing on the projection screen three color patches in a row, as C_1, C_2, C_4 in Fig. 1. These colors were in order with respect to a particular attribute in which they differed obviously. The intermediate color, C_2, was moved back and forth between the other two until a position was attained for which the space ratio, C_4 to C_2 : C_2 to C_1, seemed to equal the sense ratio, $C_4 - C_2 : C_2 - C_1$.

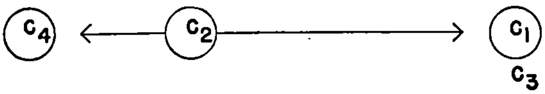

FIG. 1. ADJUSTING THE SPACE DISTANCES TO REPRESENT THE SENSE DISTANCES

A more generally practicable recording method is graphically to indicate rather than actually to make, the representative space adjustment. This is illustrated by Richardson whose subjects quantified a certain pink by making a check mark on a straight line representing a white-to-scarlet continuum.[6] It is also illustrated by the form of vector notation which is being used in the review of the spacing of the Munsell colors. Fig. 2 suggests the application of graphic recording to the R form. *O* represents the attributive magnitude of C_1 by a line of any convenient length. He then draws a second line, representing C_2, of such length relative to the first line that the ratio formed by the two lines is the same as the ratio of the two colors. Fig. 3 shows the same kind of recording in case of the R′ form of the ratio method. Here one line is drawn to represent the sense difference or sense distance between C_2 and C_1, while the other line is so drawn that its length relative to the first will be the same as $C_4 - C_3$ relative to $C_2 - C_1$.

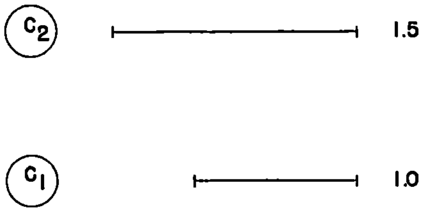

FIG. 2. MODE OF RECORDING ESTIMATES MADE BY THE SIMPLE RATIO METHOD, R

A third method, one which depends more on *O*'s unaided judgment, is to record a number indicating what the appropriate adjustment would be. Without bothering to draw any lines, *O* simply decides what multiple or fraction the one chromatic magnitude is of the other and assigns the number. If the methods are interchangeable, this number would be the same as the corresponding ratio of lines. In Fig. 2, for instance, the line representing C_2 is 1.5 times the line of unit length representing C_1; consequently a numeric estimate of about 1.5 is to be expected also.[7]

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[6] L. F. Richardson, Quantitative mental estimates of light and colour, *Brit. J. Psychol.*, 20, 1929, 31-32.

All three methods assume that, given the colors, a ratio is to be found. This is the obvious premise in our general problem of color evaluation. A fourth recording method would be required to cover the case where *O* starts with a given numerical or spatial ratio and has the task of painting, adjusting or otherwise producing a sample which will conform to it. This reverse problem is posed in the traditional sense distance procedures of the psychological laboratory.

The ratio method, being a sense distance method, is related to the four specific sense distance procedures defined by Boring.[8] One of these procedures, known as fractionation, is essentially the simple R form of the ratio method. The only difference seems to be that fractionation has been restricted by definition to the case where *O* adjusts a stimulus to produce a prescribed ratio.[9] A special case of fractionation is the halving procedure. The other two procedures, equation and bisection, resemble the R′ ratio form in that they deal in differences, but apart from the special case of equality, they do not involve the ratio principle at all.

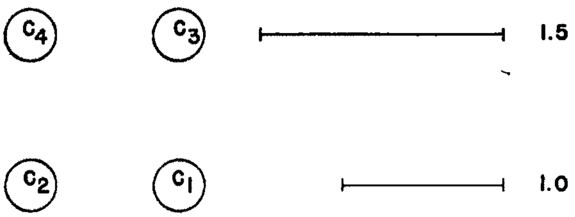

FIG. 3. MODE OF RECORDING ESTIMATES MADE BY THE DIFFERENCE RATIO METHOD, R′

The fact that estimates by both the R and R′ forms of the ratio method are recorded in the same manner should not conceal the fundamental difference in the forms themselves. The direct comparison of one sensory magnitude with another is a different operation from the comparison of intervals between sensations. Experiment and introspection evidence a different set which may, therefore, lead to different results. Even in the limiting case, equivalence cannot be assumed or even expected. The perception of a certain magnitude of saturation, for instance, is a different process from the perception of the interval between that magnitude and no saturation. Even if C_3 and C_1 in Fig. 3 both represent zero saturation and $[(C_4 - C_3)/(C_2 - C_1)]_S$ is equal arithmetically to $(C_4/C_2)_S$ it fails to be equal to $(C_4/C_2)_S$ introspectively. Lack of equality in reported results would raise a fundamental issue in color measurement.

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[7] The same would hold for Fig. 3 because the recording methods are identical. Incidentally, Fig. 1 like Fig. 3, illustrates the R′ form and the two are simply related arithmetically. The situation in Fig. 1 differs in two respects from that in Fig. 3, *i.e.* (1) the ratio of estimate is $C_4 - C_2/C_2 - C_1$ instead of $C_4 - C_3/C_2 - C_1$, and (2) the attributive equality of C_1 and C_3 is assumed. Under these conditions it is easy to show that $(C_4 - C_2/C_2 - C_1) + 1 = C_4 - C_3/C_2 - C_1$. So, if an estimate has been recorded by adjusting the position of an intermediate color, the unit constant might be added to the result. Thus, in this instance, $C_4 - C_2/C_2 - C_1 = 0.5/1.0 = 0.5$, and $0.5 + 1 = 1.5$. This result is seen to agree arithmetically with the more usual R′ form, but whether psychological differences would permit agreement in actual practice is not known.

[8] E. G. Boring, The psychophysics of color tolerance, *supra*, 384-394.

[9] E. B. Newman, J. Volkmann, and S. S. Stevens, On the method of bisection and its relation to a loudness scale, this JOURNAL, 49, 1937, 134.

Other interesting characteristics of the ratio method which have emerged or gained emphasis through use in experiment are, for convenience, presented here rather than in a subsequent section on results.

(1) Despite greater complexity, the R′ or difference form has a technical advantage over the R form because the former yields two points on the attributive scale of estimate and therefore defines it. Zero is defined by zero difference, that is, $|C_4 - C_3| = |C_2 - C_1|$, and a finite amount is represented by the unit amount or chosen standard, $|C_2 - C_1|$. In the case of the R method, the unit amount is also provided by the chosen standard, C_1, but the zero is unknown. Thus the R form is characterized by the same strange feature as the fractionation and halving procedures, *i.e.* the attributive estimate must be independent of a perceptually defined scale. That the estimate can be made is, nevertheless, a well known fact.

(2) Surprisingly enough, the ratio method appears to apply to the so-called qualitative variable, hue, as well as to saturation and lightness; at least this is true for the relatively small hue differences encountered in the present review. The reason may be that the hue scale is virtually quantitative for short ranges lying between 'unitary' colors. The evaluation of R_H, however, involves a limitation in addition to that mentioned in *(1)*. Though C_2 can be estimated as departing more or less from C_1, it seems impossible even to imagine a zero for the hue scale. Consequently *O* can not even imagine C_2 as being more or less 'hueful' than C_1. Apparently, the most he could do would be to estimate the ratio of the departures of C_2 and C_1 from the nearest 'unitary' color or from some other reference color. But that would really be an application of the R'_H form to which the above limitation does not apply.

(3) The work of Judd and of Balinkin indicates that the ratio method can be used in the evaluation of over-all color difference without explicit attributive abstraction. The importance of this discovery for visual psychophysics and the determination of psychological color tolerances is not generally recognized.

(4) Little is yet known regarding precision of estimate by the ratio method. The limited data available suggest that the precision is definitely lower than that of the standard psychophysical methods. In a study of the attribute of loudness, the uncertainty of the individual estimate is 0.6 of the mean.[10] In the study on the placement of pink—mentioned above, the standard deviation of the individual estimate is 0.3 of the mean.[11] Balinkin's research yields valuable information on this matter. Considerable interest naturally attaches to the order of precision under the complicated conditions of the Munsell review, the figures of which will be available eventually.

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[10] L. F. Richardson and J. S. Ross, Loudness and telephone current, *J. Gen. Psychol.*, 3, 1930, 296, 304.

[11] L. F. Richardson, Quantitative mental estimates of light and colour, *Brit. J. Psychol.*, 20, 1929, 37.

The spacing problem. An important requirement in the construction of an ideal surface color solid is that equal sense distances shall correspond to equal linear extents. How closely this condition can be approximated in Euclidean space is still uncertain. A second requirement often imposed is that the dimensions of space shall represent the attributes of color, *i.e.* hue, saturation, and lightness, or in Munsell terms, hue, chroma, and value. The spacing problem consists in adjusting the locations in space of the regular Munsell samples until they are in essential agreement with the two requirements just mentioned. This amounts to the correction of imperfecions in a real chromatic skeleton of 400 units so that it will more properly fit into an ideal chromatic body.

The psychologically equi-stepped solid, which the Munsell system approximates in part, includes a vertical achromatic axis upon which is stacked a finite series of horizontal, iso-lightness planes ranging from black below to white above. Surrounding the axis is a series of concentric, iso-saturation cylinders ranging from minimum within to maximum without. Radiating from the axis is a closed series of vertical iso-hue planes arranged in the spectral order and including the purples. The 7 Munsell constant-value charts and the 20 constant-hue charts[12] correspond, respectively, to equi-stepped iso-lightness and iso-hue planes of the ideal figure. The saturation cylinders as well as the planes are broken and limited by the bounding surface of the solid.

Fig. 4 exhibits a Munsell solid as photographed from five different angles separated by 72°, to show the shape relative to the loci of the five principal Munsell hues, R, Y, G, B, P. The achromatic axis is indicated, and constant-value and constant-hue planes can be visualized as horizontal or vertical cuts through the solid. Great differences in horizontal distances from the axis to the surface indicate great differences in the chroma maxima for the various hues and lightnesses.

The Munsell solid approximates the psychological ideal in regard to its three dimensions taken separately but not collectively. This is because the scaling units of hue, chroma, and value are far from equivalent perceptually. Fig. 4 shows the solid to be half as high as it is broad, but when the scaling units have been roughly equalized and the proportions of the solid more closely approximate the ideal, the height and breadth become about equal.

This picture may help to make the spacing problem seem concrete and to suggest that spacing adjustments in three dimensions are indicated. But it is well to remember that the particular shape of the Munsell solid is really a secondary matter, depending not only on the choice of units but also on the availability of sufficiently permanent pigments for practical purposes.

Though this solid is much smaller than the theoretical pigment maximum,[13] the contained number of just discriminable color elements is undoubtedly in the millions. In comparison, the 400 Munsell colors may seem a very small number, and the sensory distances among them must be very large under favorable conditions of observation. But adjacent Munsell samples do not seem very widely separated under ordinary conditions of observation and so, perhaps, do not form too bare a skeleton. This statement gains support from the allowance by Judd and Kelly, for drug description, of about one color name per 20,000 colors.[14] In any event, if these colors can be correctly located, further refinement might follow or be proved not feasible.

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[12] Munsell Color Co., *Munsell Book of Color,* 1929.

[13] D. L. MacAdam, Maximum visual efficiency of colored materials, *J. Opt. Soc. Amer.,* 25, 1935, 361-367.

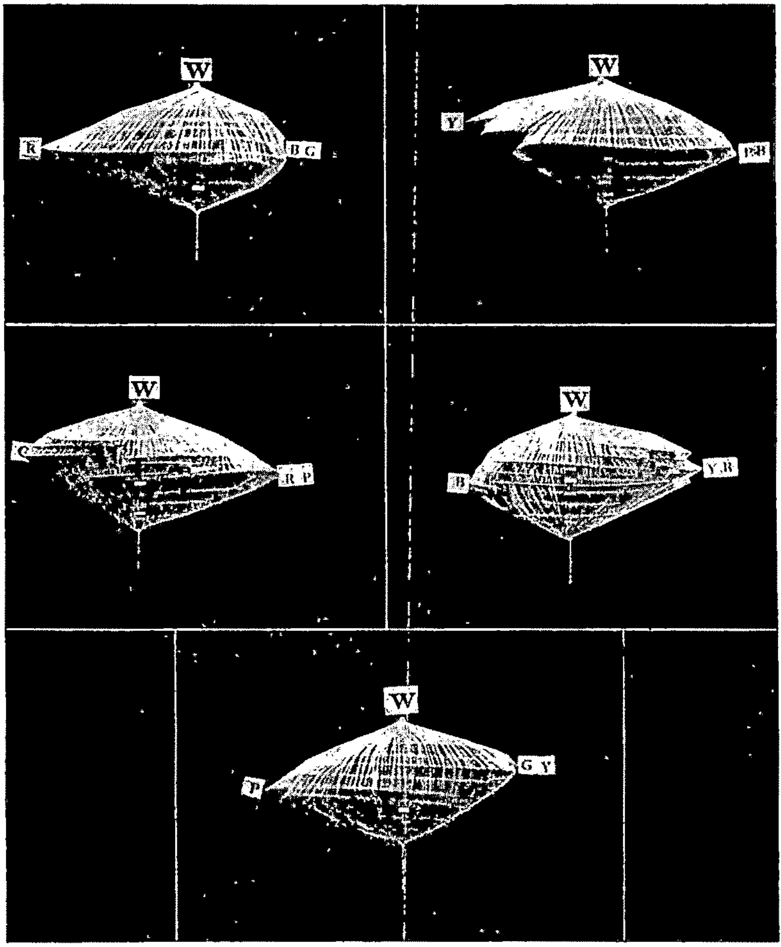

FIG. 4. THE ROUNDED MUNSELL SOLID

The question of the degree of accuracy requisite to the successful completion of the spacing problem is itself a tolerance problem. Several considerations may be mentioned: (a) Since the whole point of the respacing effort is to reduce discriminable irregularities, it would seem ideal to take as the measure of tolerance the differential threshold under favorable conditions of viewing. Probably this ideal may be approached but not reached with the present procedure. (b) There is the fact that the Munsell papers themselves exhibit a limited amount of perceptible variation in samples assigned the same notation. Thus the variability of the papers might be taken as the tolerance limit. (c) On the other hand, variability of the papers imposes no necessary limitation on accuracy. If that variability is found to be too high, as evaluated perceptually, it can be reduced by adjusting the manufacturing tolerance. The master series of samples deposited with the National Bureau of Standards by the Munsell Color Company can be taken as standard and held constant indefinitely by appropriate analysis and colorimetric specification.

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[14] D. B. Judd and K. L. Kelly, Scientific color naming of drugs, *J. Amer. Pharm. Ass.*, 27, 1938, 208-211.

The general attitude taken in the present review is that any substantial improvement in spacing which may prove possible in a few years' time will be well worthwhile, whether or not the ideal of the DL tolerance limit can be closely approximated. Indeed, the DL under favorable conditions is so small that considerable departures from it would be tolerable in many circumstances.

The ratio method applied to the spacing problem. The suitability of the ratio method for the spacing review may be briefly explained. First of all, the method is sufficiently flexible to permit the use of a relatively general or free instruction to the observer. In this new application of a new method the desirability of providing *O* with considerable latitude may be fairly evident; such a provision might lead to more representative results or to the discovery of better ways of observing than would a more rigid instruction. Another reason for having this free instruction is the great variation in the experience of the participating *O*s; the instruction needs to be relatively general to be comprehensible to all of the *O*s. Even so, the complete instructions cover nearly five typewritten pages.

The fact that the ratio method is rapid is an advantage in making the large number of color comparisons required. It is the time factor which makes the thorough-going application of presumably more precise methods seem impracticable. The experimental time factor imposed by the standard threshold methods would seem to prohibit their use.[15] Similarly, a computational time factor tends to exclude multidimensional psychophysics of the sort suggested by M. W. Richardson.[16]

The use of the ratio method seems peculiarly significant because, as Judd has pointed out, it amounts to the direct application of the definitions of the attributes of color. Unless the definitions are changed or other methods are proved comparable, only the ratio method is strictly applicable.

Observers are making the estimates of hue, value, and chroma on the Munsell constant-value and constant-hue charts. Both forms are being applied: the color ratio, or R form, consists here in the direct comparison of several samples with respect to the given attribute; and the difference ratio, or R′ form, calls for the comparison of the attributive differences between samples. In the one case *O* asks himself, "How much does each of these colors differ in this attribute in regard to the particular degree of it which I select as my comparison standard?" In the other case, "How much does the attributive interval between this pair of colors differ in regard to the degree of attributive difference which I have taken as standard?" In both cases the estimates of departures from standard are recorded in vector notation in a record form similar in shape to the color chart. The effort is to represent various degrees of departure by drawing vectors of proportionate lengths.

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[15] J. P. Guilford, *Psychometric Methods,* 1936, Pt. I.

[16] M. W. Richardson, Multidimensional psychophysics, *Psychol. Bull.,* 35, 1938, 659-660.

Hue is estimated from the constant-value charts. The series of samples in a given 'constant' hue radius is isolated by masking the remainder of the chart and examined for variations in hue. If such are observed, the effort is made to estimate a representative or average hue and indicate departures therefrom by tangential vector notation. The same procedure is to be followed in turn for each 'constant' hue radius on the chart. Next, the spacings of the hue radii considered as wholes are examined, and in any cases where hue differences seem out of proportion to the space differences, a tangential vector is affixed to the end of the radius to indicate rotation.

Chroma, like hue, is estimated from the constant-value charts. The samples in a given 'constant' chroma ring are masked off, and *O* looks them over in an effort to select a sample of representative chroma to serve as standard. Any sample which differs in chroma from the standard is then evaluated in terms of it, and a suitable radial vector is drawn to indicate the displacement. Subsequently, the masks are removed, and the 'constant' hue radii are examined for irregularities in the size of the chroma steps between adjacent colors. After selection of a representative step or chroma difference, each of the other steps can be evaluated in terms of it.

Chroma is estimated again, as a check, from the constant-hue charts. A constant chroma column is masked off, and the R form is applied. After each column has been observed, the masks are removed, and the equality of the chroma steps is examined by the R′ form.

Incidentally, most subjects seem to find the applications of the R′ form to chroma the most difficult part of the entire procedure. This is because of the considerable number of chroma steps to be taken into consideration.

Value is also estimated from the constant-hue charts. 'Constant' value rows are masked off and evaluated by the R form, after which the masks are removed, and the size of the value steps between successive 'constant' value rows are adjusted by the R′ procedure. In the case of value, a special difficulty in applying the vector recording technique led to the substitution of numerical estimates of the spacing in terms of the value step unit.

Since there are 7 constant-value charts and 20 constant-hue charts, and since each of these 27 charts is prepared with three backgrounds—white, gray, and black—there are to be examined a total of 81 charts. The task is long and so exacting that it can not be relegated to periods of fatigue. Those few who complete it usually take more than a year to do so.

Complications in the application of the ratio method. Psychophysical methods differ from physical in that none of the former seems satisfactory to the naïve *O*. The reason is that the critical judgments necessarily fall within a range of uncertainty and seem more or less like guessing. Incidentally, this condition is meritorious, for the consequent variability is more indicative of sensitivity than lack of precision. Some of the *O*s in this review find the judgmental situation so "difficult," "impossible," or "terrible" that there is special reason to consider the problems reported by them.

(1) There is the problem of complex anisochromatic comparisons forcing, as it does, the abstraction of the attribute to be estimated. Attributive abstraction is

peculiarly difficult in cases of strong association; for instance, when saturation is the judged attribute while hue is constant and value is variable, there is the strong tendency to estimate lower-value colors as of higher saturation. That is a relation often found in everyday life. Another familiar problem is that of equating for lightness colors of differing hue. A similar difficulty which is also emphasized in case of unsophisticated subjects is that of equating chromatic and achromatic colors in respect to lightness. The untrained *O* usually sees chromatic samples as lighter relative to achromatic samples, than does the trained *O*. Inspection of data from the constant hue charts reveals the interesting fact that the vectorial lightness adjustments for the chromatic samples are never downward. Wherever an adjustment is made, it indicates that the chromatic sample seemed too bright relative to the achromatic of the same Munsell value.

*(2) O*s making saturation estimates by the R′ method report greater facility in difference comparison when both differences are well about achromaticity than when one terminates there. The trouble seems to be that a first step extending from zero to some chroma appears as a qualitative step and so tends to give the impression of an infinite quantitative step. The second, step, or difference, from some chroma to more chroma is, on the other hand, a definitely limited quantitative step. Though a very low chroma level is actually attained in the first step, the step itself is relatively great. This 'zero difficulty' of the R′ method bothers only some of the *O*s.

Possibly related to it is a certain change of attitude correlated with a change of chroma level. When observing chroma differences near neutral, some observers report that they 'just look for any difference' whereas at the higher chroma levels they think more definitely in terms of chroma. Such a change of attitude might be expected to occur naturally on the basis of least effort. Looking for a mere difference would be easier when the detection of chroma is difficult, whereas looking for a chroma difference would be easier when chroma is present in obvious degree.

(3) The long recognized difficulty of spatial or temporal separation of samples is evidently present in this study where widely separated parts of a chart are compared. Of incidental interest are preliminary data indicating surprisingly good color matches when time differences between standard and match are over two minutes!

In the face of the difficulties, what do the *O*s do? All that can be done here is to note a few common tendencies apparently present. (a) The series of colors, or color differences, to be judged is first surveyed as a whole, and the *O* essays a decision with respect to rank order for the attribute in question. Often no progressive graduation is found; and instead of arriving at an order, *O* is able to identify a few extreme values and a number which seem about equal. (b) After some such preliminary survey, *O* may proceed to the assignment of vectors. A common practice is to draw in vectors representing the more extreme displacements first, and then, partly on the basis of those, to fill in the shorter vectors indicating the lesser irregularities. (c) There is the tendency to 'carry' a standard subjectively even though some particular perceptual standard has been chosen. *O* may occasionally refer to the standard sample for verification, but actually make various comparisons without it.

This 'absolute' procedure and the ranking procedure mentioned above were both hit upon without instruction, probably because they facilitate *O*'s difficult task. Certainly it is easier to decide which of two sensory magnitudes is greater than to estimate how much greater; and it is sometimes easier when making numerous comparisons to carry a subjective standard than to refer back each time to a perceptual one.

Evidently, it would be a mistake to conclude that the ratio method has been applied to the spacing problem in anything like pure form. Neither, in advance of repetitive data, can one draw any definite conclusions with respect to the self-consistency of the given *O*. On the other hand, the instructions call for so thorough an examination of the charts that marked irregularities can scarcely be overlooked.

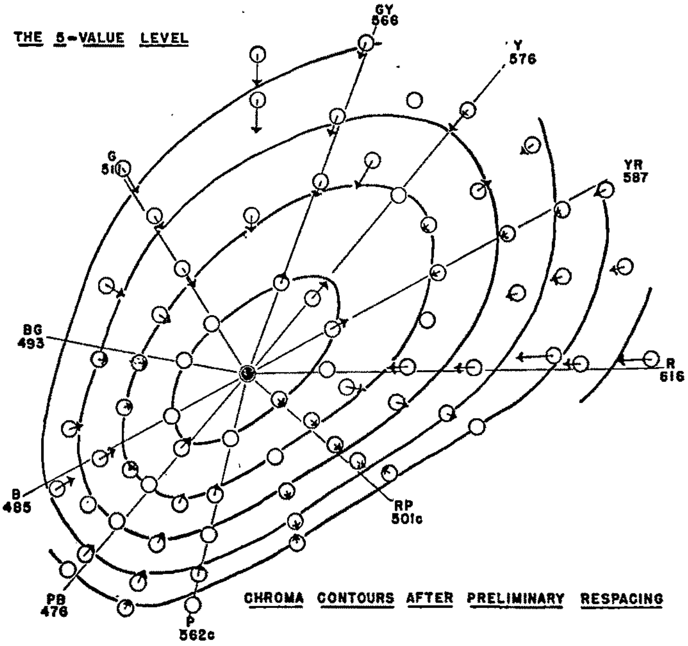

FIG. 5. ADJUSTED IRREGULARITIES AND CHROMA CONTOURS AT THE 5-VALUE LEVEL

Preliminary results. Most of the results now available have to do with the nature and technique of the ratio method; and these results have already been described *en passant.* For the purposes of this paper, the status of the respacing itself may be dismissed in a few words.

Preliminary results on the respacing of saturation were published by Nickerson last summer.[17] The Munsell colors of each constant-value chart were plotted in ICI trilinear coördinates, utilizing the Hardy-Glenn-Killian data. Then a summary was made of the chroma estimates for each individual Munsell color. Corresponding to the summary estimate, a radial arrow was attached to each point in the diagrams which was not confirmed by the consensus. The direction of this vector arrow would be inward or outward according to the consensus of estimates, and its length would be long or short according to whether the sample was judged to be considerably or only slightly out of line. After all the radial arrows had been marked in, representative chroma contours were established by drawing smooth curves through the scatters of the arrow-heads. Smoother and more sweeping curves could be passed through the arrow-heads than through the original unadjusted points from which the arrows were drawn. This result seemed to indicate that the visual estimates had reduced some of the irregularities in chroma, and, in general, that the ratio method was proving useful.

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[17] D. Nickerson, Use of ICI tri-stimulus values in disk colorimetry, *U. S. Dept. Agriculture,* 1938, 1-17.

This result is illustrated by Fig. 5, which shows the plot of the Hardy-Glenn-Killian determinations for the 5-value Munsell chart, together with Nickerson's smoothed curves and arrows for the gray ground. It suggests that many minor adjustments will probably prove desirable. The figure also shows that the ICI coordinate system is far from an equal chromaticity system and so not an ideal system of reference. Still, the departures from equal chromaticity do not seem sufficiently sudden to give rise to any serious misinterpretations, and it is probably best to utilize this standard system.

Preliminary comparison of results secured from use of the black, white, and gray charts indicates that background reflectance plays a significant rôle. This was to be expected from the results of Helson[18] and Judd.[19] The stimulus-specifications for the ideal solid vary markedly with the background, especially at the extreme values. Thus the solution of the spacing problem is not simple.

SUMMARY

(1) The ratio method consists in the estimation by direct impression of the ratio of supraliminal sense magnitudes. The one magnitude or interval is taken as standard, and the ratio of the other to it is then estimated.

(2) The spacing problem is to detect and correct errors of allocation in surface color space of the 400 regular Munsell colors. The readjustments are being made for the samples as mounted on white, on gray, and on black (cardboard) grounds.

(3) The application of the ratio method to the spacing problem proves to be a complicated, meticulous, and lengthy process of choosing vectorial equivalents for sensory magnitudes. The principal attributes are evaluated separately under a flexible instruction.

(4) *O*s are beset by the real and technical difficulties of the first chroma step, attributive abstraction, and separation of standard and sample. They do not use the ratio method in pure form, but resort to the methods of ranking and single stimuli in the effort to achieve results; and results are achieved.

(5) Preliminary results on the respacing of saturation tend to confirm the usefulness of the method and to suggest that many minor adjustments may prove desirable. Present data indicate that adjustments also will be necessary if the colors are to be corrected for gross variations in background reflectance.

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[18] H. Helson, Fundamental problems in color vision. I. The principle governing changes in hue, saturation, and lightness of non-selective samples in chromatic illumination, *J. Exper. Psychol.*, 23, 1938, 439-476.

[19] D. B. Judd, Hue, saturation and lightness of surface colors with chromatic illumination, *Bull. Amer. Phys. Soc.*, 14, 1939, 23.

COLOR TOLERANCES AS AFFECTED BY CHANGES IN COMPOSITION AND INTENSITY OF ILLUMINATION AND REFLECTANCE OF BACKGROUND

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The hue, lightness, and saturation of any object depend upon a complex of conditions, chief of which are spectral reflectance of sample and background, spectral energy distribution of illuminant, and state of the visual mechanism as determined by its properties as a receptor and mode of functioning. Since color tolerance concerns the extent to which colors match, or if they do not match, in what respects they differ, any factors which affect the color of standard and variables have importance for the problem of tolerance. How illuminant, background, contrast, adaptation, and so-called constancy operate in literally 'coloring' objects seems to be nearing solution after recent work with strongly chromatic illuminants and backgrounds of high, medium, and low reflectance.[1] The principles governing color changes which objects undergo in strongly chromatic illuminations throw light also on phenomena encountered in ordinary viewing situations. Hence consideration of the facts discovered under 'abnormal' conditions of viewing is of value in bringing into sharp relief the principles operating in all visual functioning.

TABLE I

Reports of 16 Os on Hue of Munsell and Achromatic Samples under Palo Daylight Illumination

	S	R	yR	RY	rY	Y	gY	YG	yG	G	bG	BG	gB	B	rB	RB	bR	A
R	3/7	12	3														1	
R	5/10	10	5														1	
R	7/6	13	2														1	
YR	4/5		1	4	9	2												
Y	5/7					10	6											
Y	7/8					15	1											
G	3/4								2	11	3							
G	5/7								3	13								
G	7/7								4	11	1							
B	3/5												6	10				
B	5/6												7	9				
B	7/4											1	1	14				
P	3/6														6	5	5	
P	5/6															10	6	
PB	7/4													16				
A	10/0																	16
A	5/0																	16
A	0/0																	16

Before discussing factors affecting color tolerances more or less common to all

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[1] D. B. Judd, Surface color, *J. Opt. Soc. Amer.*, 25, 1935, 44; H. Helson, Tridimensional analysis and the non-film modes of color appearance, *ibid.*, 27, 1937, 59; Fundamental problems in color vision, I. The principle governing changes in hue, saturation, and lightness of non-selective samples in chromatic illumination, *J. Exper. Psychol.*, 23, 1938, 439-476.

*O*s, let us consider briefly individual differences in color vision,[2] due to a variety of causes, which preclude absolute agreement on color naming and color matching no matter how favorable the viewing conditions may be. Data on surface colors show that some *O*s do not tolerate a sample as a representative of a hue which others give it. The results in Table I, wherein reports of 16 *O*s are given for 15 Munsell samples and three achromatic samples of less aristocratic origin under Palo daylight illumination, show that none of the samples supposed to exemplify one of the four unitary hues, Red, Green, Yellow, Blue, was given a single hue name by all *O*s. In every case some *O*s report a second, minor component in the unitary hues. Thus the three Munsell reds, R 3/7, R 5/10, R 7/6, are reported as having a *yellowish* or a *bluish* minor component by some *O*s. Similarly more than one hue-name characterizes the reports by some *O*s on the other 'unitary' hues.

While a large majority of the *O*s report the same hue, dissenters may be put into two groups, one reporting as a secondary component a hue on one side of the principal one (*yellow* in G ¾), the other reporting as secondary component a hue on the other side of the principal one (*blue* in G ¾). On the other hand, some binary hues, for example YR ⅘, are seen as unitary by some *O*s. Agreement is much better, as Table I shows, with respect to binary hues than unitary hues. It is as if the unitary hues either do not have sufficiently *strong* secondary components to be visible to all *O*s, or sufficiently *weak* components to remain invisible so far as all *O*s are concerned. Practically, we may put it thus: a merchant selling blue shirts would find from data such as these what percentage of his customers would not tolerate his goods but would object that they are off-color, say greenish or reddish blue instead of a good blue. Furthermore he would be more apt to find agreement among his customers on binary hues than on so-called pure or unitary hues.

Apart from individual differences such as we have just discussed two of the chief factors influencing color tolerances are the composition of the illuminant in which the samples are viewed and the reflectance of the background on which they are placed. A clear-cut decision as to the rôle of each of these factors could only be made after many samples were observed in a variety of strongly chromatic illuminations on backgrounds of different reflectances because illuminant effects depend largely upon background influences.

Inspection of Table II brings out clearly the combined effect of illuminant and background on color of objects. Taking the 18 samples as a whole we find that on black background the hue of that illuminant predominates while on white background the complementary hue is strongly in evidence as Table II clearly shows. With gray backgrounds both hues appear about equally often. For each background reflectance with any given set of samples there is established a certain level of adaptation identified by a reflectance above which appear illuminant colors and below which appear the complementary colors. Hence background furnishes a clue not only to hue but to saturation as well because samples near adaptation reflectance are either achromatic or low in saturation, highest saturations being found in either the lightest or darkest samples.[8]

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[2] One of the chief sources of differences in reports on hue names, aside from practice and training, lies in the macular pigment which varies from person to person. Cf. W. O'D. Pierce, Individual differences in normal colour vision, Reports of the committee upon the physiology of vision, *Med. Res. Council, His Majesty's Stationery Office, London,* 1933, No. 181, 34. "The conclusion has thus been reached," says Pierce, "that the main source of variation among normal trichromats is due to the macular pigment varying greatly in density and to a less extent in dominant hue." We do not, however, imply this is the only factor governing the reports of our observers.

TABLE II

Hue (H), Lightness (L), Saturation (S) and Apparent Reflectance (A), of 15 Munsell Samples and Three Achromatic Samples (S) in Four Illuminants on Three Backgrounds

Red Illumination

			White		Gray		Black	
	S	A	H	L/S	H	L/S	H	L/S
A	10/0	.800	R	9.0/4.0	RY	8.0/7.5	yR	8.0/9.0
R	7/6	.781	yR	9.0/3.0	RY	8.0/7.0	yR	7.5/9.0
R	5/10	.706	yR	8.0/3.5	yR	8:0/7.0	yR	7.5/9.0
Y	7/8	.577	RY	8.0/3.0	yR	7.5/6.0	yR	6.5/9.0
pB	7/4	.428	yR	7.0/2.5	RY	7.0/4.5	R	6.0/8.0
B	7/4	.381	rY	6.0/1.0	rY	6.0/5.0	R	5.5/8.0
P	5/6	.359	yR	6.0/2.0	RY	6.0/5.0	R	5.5/8.0
G	7/7	.329	R	5.0/1.0	rY	6.0/4.0	yR	5.0/7.0
YR	4/5	.297	yR	4.5/1.0	RY	5.0/4.0	yR	4.5/7.0
R	3/7	.276	R	4.5/1.0	RY	5.0/4.0	R	4.5/7.0
Y	5/7	.272	A	4.5/0.0	rY	5.0/4.0	R	4.5/7.0
A	5/0	.240	R	4.0/0.4	yR	5.0/1.5	R	4.5/3.5
B	5/6	.161	gB	3.0/1.0	YG	3.0/0.5	R	3.0/5.0
P	3/6	.153	gB	3.5/1.0	Y	3.0/0.7	bR	3.0/5.0
G	5/7	.087	GB	1.0/4.0	gB	2.0/2.0	yR	2.0/2.0
G	3/4	.072	GB	1.0/7.0	GB	2.0/2.0	yR	1.5/2.0
B	3/5	.064	bG	2.0/7.0	BG	1.5/2.0	yR	1.5/2.0
A	0/0	.030	gB	2.5/9.0	G	2.5/9.0	A	0.0/0.0

Green Illumination

			White		Gray		Black	
	S	A	H	L/S	H	L/S	H	L/S
A	10/0	.800	G	8.5/3.0	gY	9.0/9.0	gY	8.5/7.0
G	7/7	.596	yG	8.0/3.0	YG	8.0/5.0	yG	7.5/6.0
B	7/4	.581	yG	8.0/3.0	yG	8.0/5.0	yG	7.5/6.0
pB	7/4	.485	G	7.0/3.0	yG	6.5/6.0	yG	7.0/6.0
Y	7/8	.455	rY	6.5/4.5	Y	6.5/5.0	gY	7.0/4.5
G	5/7	.377	yG	5.0/2.5	yG	6.0/7.0	yG	6.0/7.0
R	7/6	.357	Y	5.5/2.5	Y	6.0/3.0	YG	6.0/4.0
B	5/6	.324	bG	5.0/3.0	bG	5.0/7.5	G	5.0/7.0
A	5/0	.240	A	5.0/0.0	gY	5.0/2.0	yG	5.0/2.0
P	5/6	.235	rB	5.0/2.0	YG	5.0/0.5	yG	5.0/5.0
Y	5/7	.221	RY	5.0/4.0	rY	5.0/4.0	yG	5.0/3.0
B	3/5	.133	rB	3.0/4.0	B	3.0/1.0	G	4.0/7.0
R	5/10	.110	bR	3.0/5.0	bR	4.0/4.0	gY	4.0/2.0
YR	4/5	.110	bR	3.0/4.5	yR	4.0/4.0	Y	4.0/2.0
G	3/4	.107	rB	3.5/4.5	rB	4.0/2.0	G	4.0/2.0
P	3/6	.097	rB	3.0/4.5	rB	3.0/3.0	G	3.0/2.0
R	3/7	.049	RB	1.5/7.0	bR	2.5/7.5	RY	2.0/1.5
A	0/0	.030	RB	2.0/5.0	RB	2.0/9.0	A	0.0/0.0

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[8] For a more thorough discussion of adaptation reflection and its relation to saturation and lightness, see H. Helson, *ibid.*

TABLE II—*Continued*

			Yellow Illumination					
			White		Gray		Black	
	S	A	H	L/S	H	L/S	H	L/S
A	10/0	.800	Y	9.0/4.5	Y	8.5/9.0	Y	9.0/8.0
R	7/6	.618	RY	8.0/5.0	rY	7.5/7.0	rY	8.0/7.5
Y	7/8	.588	gY	8.0/4.0	Y	8.0/7.0	Y	8.0/7.0
B	7/4	.445	yG	7.0/5.0	YG	8.0/5.5	YG	7.0/5.5
G	7/7	.428	yG	7.0/7.0	yG	7.5/7.0	yG	7.0/5.0
pB	7/4	.405	gY	7.0/3.0	gY	5.0/4.5	gY	7.0/5.0
R	5/10	.404	yR	7.0/8.0	yR	7.0/9.0	RY	7.0/8.5
Y	5/7	.288	rY	5.0/1.0	gY	5.0/3.5	yG	6.0/5.0
P	5/6	.273	bR	5.0/3.5	RY	5.0/3.5	rY	6.0/4.0
YR	4/5	.246	bR	4.5/3.5	rY	5.0/3.5	rY	5.5/4.0
A	5/0	.240	gY	4.5/0.5	Y	5.0/1.5	Y	6.0/2.0
B	5/6	.185	bG	3.0/0.5	yG	3.0/3.0	yG	5.0/4.0
G	5/7	.171	bG	4.0/8.0	G	5.0/9.0	G	5.0/8.5
R	3/7	.151	bR	4.5/9.0	bR	5.0/8.5	yR	5.0/7.0
P	3/6	.119	bR	3.0/5.0	yR	3.0/4.0	RY	4.5/4.0
G	3/4	.078	rB	2.0/2.5	rB	2.0/2.0	G	3.0/2.0
B	3/5	.072	rB	2.0/2.5	rB	2.0/2.0	yG	3.0/3.0
A	0/0	.030	rB	1.0/2.0	RB	1.0/3.0	A	0.0/0.0

			Blue Illumination					
			White		Gray		Black	
	S	A	H	L/S	H	L/S	H	L/S
A	10/0	.800	B	9.0/3.5	B	9.0/6.0	B	8.5/7.5
B	7/4	.642	B	8.0/6.0	B	8.5/6.0	B	8.0/6.0
pB	7/4	.600	B	8.0/5.0	B	8.5/7.0	B	8.0/6.0
G	7/7	.493	gB	7.0/4.5	B	7.0/6.0	B	7.5/5.0
B	5/6	.472	gB	7.0/3.5	B	7.5/7.0	B	7.5/8.0
R	7/6	.346	rB	6.0/6.0	RB	6.0/5.0	rB	6.5/6.0
P	5/6	.310	rB	6.0/6.0	rB	6.5/7.0	rB	6.0/8.0
G	5/7	.267	G	6.0/7.0	G	6.0/7.0	BG	5.5/6.5
A	5/0	.240	A	5.0/0.0	A	5.5/0.0	B	5.0/5.0
B	3/5	.217	B	5.0/0.5	BG	5.0/1.0	B	5.0/6.5
Y	7/8	.159	rY	3.0/3.5	rY	4.0/3.0	RB	4.0/1.5
P	3/6	.153	RB	5.0/6.5	rB	4.0/7.5	RB	4.0/8.0
R	5/10	.110	bR	3.0/1.5	bR	3.5/3.0	RB	3.0/6.0
Y	5/7	.097	yR	3.0/1.5	rY	3.0/2.0	bR	3.0/2.0
G	3/4	.093	Y	3.0/0.5	yG	3.0/1.5	B	3.0/1.5
YR	4/5	.069	RY	2.0/1.0	yR	3.0/3.0	bR	2.0/2.5
R	3/7	.048	yR	2.0/1.5	R	2.0/2.0	bR	2.0/3.0
A	0/0	.030	yR	1.0/0.5	RY	1.0/1.0	A	0.0/0.0

Under ordinary conditions of viewing the illuminant is not strongly chromatic and hence these effects are not so patent but the principle just formulated operates in all illuminants. Illumination having only a tinge of chroma colors objects in the ways stated even though to a slight extent.[4] Mixture of chromatic (filtered) light with 2848°K in varying degrees made possible illuminants of varying chromaticities and the results in Table III show that the hue of samples is a resultant of the 'white' light hue and the dominant chromatic component of the illuminant. Non-selective samples betray the chromatic component in the illuminant to an even greater degree than selective samples. The larger number of reports of illuminant hue on black background as compared with white shows the tendency of dark grounds to induce illuminant hue on objects even in non-homogeneous illuminations. By its preponderant influence on adaptation level the background can determine whether objects shall be tinged with illuminant hue, the contrasting hue, or shall lose in saturation to the point of achromaticity.

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[4] D. Nickerson (Artificial daylighting for color grading of agricultural products, *J. Opt. Soc. Amer.*, 29, 1939, 1-9) has reported that illuminant 'C' gave a 'too-yellow' effect in grading cotton which was remedied by increasing the color temperature of the illuminant from 6500°K to 7400°K.

TABLE III

RETURN OF HUES TO DAYLIGHT APPEARANCE WITH ADMIXTURE OF 0.3 F.C. AND 4.5 F.C. 2848°K WITH 3.7 F.C. RED

3.7 f.c. R plus 0.3 f.c. 2848°K

S		A	Wh		Gr		Bl	
A	10/0	.800	R	9.0/0.5	R	8.0/3.0	R	7.0/7.5
R	7/6	.759	RY	8.0/4.5	RY	7.5/7.5	yR	7.0/8.5
R	5/10	.671	yR	7.0/6.0	yR	7.0/7.0	yR	7.0/9.0
Y	7/8	.573	Y	7.0/4.0	rY	6.5/6.0	yR	7.0/8.0
pB	7/4	.429	rB	6.0/1.0	RB	7.5/4.0	bR	6.5/6.0
B	7/4	.391	gB	7.0/1.5	B	7.5/1.0	bR	7.0/5.0
P	5/6	.351	RB	6.0/2.5	bR	5.0/5.0	yR	7.0/7.0
G	7/7	.343	bG	6.0/4.0	bG	6.0/5.0	bR	6.5/5.0
YR	4/5	.288	rY	5.0/2.0	rY	5.0/5.0	R	6.0/7.0
Y	5/7	.271	G	5.0/0.5	gY	5.0/3.0	yR	5.0/6.0
R	3/7	.263	bR	5.0/3.0	yR	5.0/6.5	R	5.5/7.0
A	5/0	.240	gB	4.0/0.5	R	4.5/1.0	bR	5.0/6.0
B	5/6	.168	gB	3.5/3.0	B	5.0/3.0	RB	5.0/4.0
P	3/6	.150	rB	2.5/2.0	bR	3.5/4.0	bR	4.0/6.0
G	5/7	.100	bG	3.5/7.0	G	4.0/8.0	bG	5.0/5.5
G	3/4	.074	bG	2.0/3.0	bG	2.0/1.0	bR	4.0/2.0
B	3/5	.067	gB	2.0/4.0	gB	2.0/4.0	rB	3.0/3.0
A	0/0	.030	BG	0.5/3.0	GB	1.0/2.0	A	0.0/0.0

3.7 f.c. R plus 4.5 f.c. 2848°K

S		A	Wh		Gr		Bl	
A	10/0	.800	RY	9.5/0.5	R	9.5/0.5	R	9.0/1.0
R	7/6	.622	yR	8.0/6.0	R	7.5/4.5	yR	7.5/5.0
Y	7/8	.548	gY	7.0/6.0	Y	7.0/6.0	Y	6.5/7.0
R	5/10	.458	yR	7.0/9.0	yR	7.5/9.0	yR	6.0/9.0
B	7/4	.455	gB	7.0/6.5	gB	7.5/4.0	B	8.0/2.5
pB	7/4	.436	rB	7.5/5.0	rB	7.5/3.0	rB	8.0/2.0
G	7/7	.429	G	7.5/7.0	G	6.5/5.5	GB	7.5/4.0
P	5/6	.301	RB	6.0/6.0	bR	6.0/6.0	bR	6.0/5.0
Y	5/7	.264	YG	5.0/5.0	gY	5.0/5.0	Y	5.0/6.0
A	5/0	.240	A	4.5/0.0	A	5.5/0.0	bR	6.0/4.0
YR	4/5	.234	rY	5.0/6.5	rY	5.0/7.0	RY	5.0/6.5
B	5/6	.211	B	5.0/7.5	gB	5.5/7.0	B	5.5/6.5
G	5/7	.183	G	6.0/9.0	G	7.0/9.0	G	6.0/9.0
R	3/7	.179	R	4.5/7.5	yR	5.0/8.0	R	5.0/7.5
P	3/6	.129	RB	4.0/5.0	RB	5.0/6.0	bR	5.0/7.0
G	3/4	.082	G	1.0/2.0	G	3.5/4.0	G	3.5/4.0
B	3/5	.052	B	3.0/5.0	B	4.0/6.5	B	4.0/5.5
A	0/0	.030	A	0.0/0.0	A	0.0/0.0	A	0.0/0.0

Classically it was thought that non-selective backgrounds exerted only a brightness contrast effect on samples but the reports in Tables II and III prove that backgrounds differing only in lightness can change not only the lightness of samples but their hue and saturation as well. This holds both for homogeneous and, to a lesser extent, for non-homogeneous illuminants. Hue and saturation depend as much upon background as lightness and can no longer be regarded as intrinsic properties associated solely with spectral reflectance of the object. Let us consider each of the three fundamental variables in relation to background when illumination is held constant.

The lightness contrast effects of different backgrounds do not appear striking unless we compare identical samples on different backgrounds *simultaneously.* Then the higher lightness of samples on darker grounds is unmistakable. When several samples are viewed on dark and light grounds *successively,* the *O*s report changes in hue and saturation rather than in lightness due to shift in lightness adaptation.

TABLE IV

Average Lightness and Saturation of 18 Samples in 4 Illuminants on Backgrounds with 4 Intensities

(Each value is the average of 90 observations)

Lightness

Illum.	White bkgd.				Gray bkgd.				Black bkgd.			
	I	II	III	IV	I	II	III	IV	I	II	III	IV
R	4.33	4.86	4.19	4.18	4.99	5.03	4.30	3.92	4.77	4.47	4.61	4.22
Y	5.08	5.08	4.86	4.87	5.36	5.14	4.93	4.96	5.50	5.67	5.38	5.14
G	4.75	4.92	4.40	4.55	5.25	5.14	4.65	5.01	4.94	5.03	4.66	4.62
B	4.47	4.94	4.26	4.10	4.94	5.16	4.47	4.22	5.11	4.92	4.36	4.19
Av.	4.66	4.95	4.43	4.43	5.14	5.12	4.59	4.54	5.08	5.02	4.75	4.54
Grand Av.			4.62				4.85				4.85	

Saturation

Illum.	White bkgd.				Gray bkgd.				Black bkgd.			
	I	II	III	IV	I	II	III	IV	I	II	III	IV
R	3.33	2.86	2.32	2.86	3.50	4.20	3.99	3.42	4.77	5.97	5.05	4.55
Y	4.64	4.14	3.82	4.22	4.69	5.14	4.22	4.28	4.75	5.00	4.44	4.19
G	3.14	3.61	3.83	3.55	4.08	4.58	3.44	4.32	4.58	4.11	3.65	3.35
B	2.36	3.25	2.47	2.18	3.69	4.17	3.69	2.93	4.42	4.94	4.05	3.11
Av.	3.37	3.47	.11	3.20	3.99	4.52	3.84	3.74	4.63	5.01	4.30	3.80
Grand Av.			3.29±1.01				4.02±0.50				4.43±0.70	

Lightness is so closely correlated with reflectance of sample, which is constant, and with adaptive level of eye, which affects all samples equally, that the *O*s tend to see samples relatively constant in lightness when background reflectance is raised or lowered within very wide limits. The lightness values given in Tables II and IV bear out this generalization.

The case is different with saturation when background is changed even though all samples are subjected to the same change. Saturation effects are selective and depend upon more complicated relations. First, the samples, on the average, gain in saturation on darker grounds in strongly chromatic illumination, as shown in Table IV. The difference in average saturation between white and gray grounds is 0.73 steps, that between the gray and black grounds is 0.41 steps, and between the black and white grounds it is 1.14 steps. While only the difference between the black and white grounds is completely statistically reliable, the differences are all in one direction and in favor of the darker grounds.

In non-homogeneous illumination some samples are more saturated on white ground, others on black ground, a fact which seems to contradict our findings with homogeneous illuminants. However, we can explain this fact through the interdependence of lightness and saturation. Colors which are better saturated at lower lightness values will be more saturated on white ground while colors which are more saturated at higher lightness values will be more saturated on black ground. Observation of identical yellows and blues on white and black grounds simultaneously bears out the correctness of our explanation. Samples which would pass muster on one ground may be unacceptable as matches on another ground.

Changes in intensity of illumination probably have less direct effect on color than any of the factors which we have discussed. The averages for lightness and saturation in Table IV for four illuminants at four intensities having a range of 1 to 96 show very small differences which are consistently in favor of the two highest intensities. Greater saturations and lightnesses are found in the two highest intensities as compared with the two lowest intensities, but considering the large range of intensities the lightness and saturation changes seem small.

In summary we can say that hue, lightness, and saturation of objects depend as much upon background reflectance and composition of illuminant as upon spectral reflectance of the object itself. For in viewing any sample we see it against a particular background and in a given illumination with, perhaps, other samples present in the field of vision. The fact that the eye can move freely over the surface of the sample plane means that we are dealing with a dynamic event whenever colors are being named or matched. Adequate specification must predict the color of objects under the dynamic conditions of viewing even though this is much more difficult than to assume that dynamic factors can be left out of account. Progress along these lines has already been made and the work is still going on.[5]

REPRESENTATION OF COLOR TOLERANCE ON THE CHROMATICITY DIAGRAM

By David L. MacAdam, Eastman Kodak Research Laboratories

The maximum acceptable variations of sample colors from a standard color for some definite use are called color tolerances.[1] The purpose of this paper is to show that the coördinate system adopted by the International Commission on Illumination in 1931 is entirely adequate for the representation of color tolerances,[2] and that any revision of the standard coördinate system based on the desire to represent or predict color tolerances more conveniently than on the present standard system would be illusory and is unnecessary.

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[5] Formulae for the color of objects viewed in any illuminant on backgrounds of different reflectances have been devised by Judd and found satisfactory in general although a complete check with our extensive data has yet to be made. See D. B. Judd. Hue, saturation, and lightness of surface colors with chromatic illumination, *J. Opt. Soc. Amer.*, 29, 1939.

[1] Communication No. 715 from the Kodak Research Laboratories.

[2] Proceedings of the 8th Session, Commission Internationale de l'Eclairage (International Commission on Illumination, ICI) Cambridge, 1931, 19; T. Smith and J. Guild, The CIE colorimetric standards and their use, *Trans. Opt. Soc.*, 33, 1931,

Color tolerances must be established by agreement between the manufacturers and the users of the colored materials. Usually this agreement is secured by selecting from a group of available samples those which are acceptable for the purpose under consideration. A wise manufacturer will submit for such selection samples having various degrees of all probable variations from the standard color. The consumer, realizing that the cost of production will depend on the percentage of rejections demanded by the limits he selects, will accept all .colors whose departures from the standard are not seriously objectionable. The limiting acceptable departures from the standard color will therefore depend on two important factors: (1) the ease with which the manufacturer can control the several possible kinds of variations from the standard color; and (2) the relative objectionabilities of these different kinds of variations in the opinion of the user of the colors. The first of these factors does not depend in any way on the degree of noticeability of the variations from the standard color, and the second factor depends only partially on noticeability. For some purposes, noticeable variations from the standard dominant wave-length will be much more objectionable than equally noticeable variations from the standard purity, and for other purposes the relative objectionabilities of these same variations from the same standard color will be quite reversed. The relative noticeabilities of dominant wave-length variations and purity variations are not determined by the purpose for which the color is used, and depend only on the dominant wave-length and purity of the standard color, and on the conditions of observation, including the shape and the size of the colored areas being compared, the distribution and color of the light in the surrounding field of vision, and the state of brightness and color adaptation of *O*'s eye at the time of the comparison.

Typical examples of color tolerance specifications. The A.A.R. Signal Section Specification 69-35 for railway signal glasses is expressed in terms of the ICI 1931 standard coördinate system, as shown in Fig. 1. Only the lower limit of the luminous transmittance for the 2360°K. Planckian radiator is specified for each glass, since the glasses are to be used in a manner which does not require any upper limit of transmittance, provided that the chromaticity of each glass is within the limits shown in the figure. An additional specification is required to differentiate the blue and purple signals. In practice the principal difference between these two signal colors is not one of chromaticity, but is due to the fact that the purple glass gives a dichromatic signal, *i.e.* red surrounded by blue, resulting from the chromatic aberration of the eye, whereas the blue glass, transmitting but little red light, gives only the blue signal color. This difference in the relative amounts of red light transmitted by the blue and purple glasses forms an essential part of the specification for these two signal colors. For this particular purpose, red light is defined to include the wave-length from 650 to 770 mμ, and the proportion of the red to total luminous transmission is expressed by the ratio T_r/T_w. There is no evident relation between the relative magnitudes of the tolerances in different directions on the chromaticity diagram, and the relative magnitudes of the just noticeable dif-

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73-130; D. B. Judd, The 1931 ICI standard observer and coördinate system for colorimetry, *J. Opt. Soc. Amer.*, 23, 1933, 359-374; Handbook of Colorimetry, *Technology Press, Mass. Inst. Tech.*, 1936.

ferences of chromaticity in the corresponding directions.[3] These tolerances on the chromaticity and the lower limits of transmittance for railway signal glasses were determined by agreement, in which the primary considerations were availability of the glasses described, and their usefulness for their specific purpose. The exact locations of the tolerance boundaries were chosen so that they could be expressed simply in analytical form, in terms of the ICI coördinate system, and the fact that

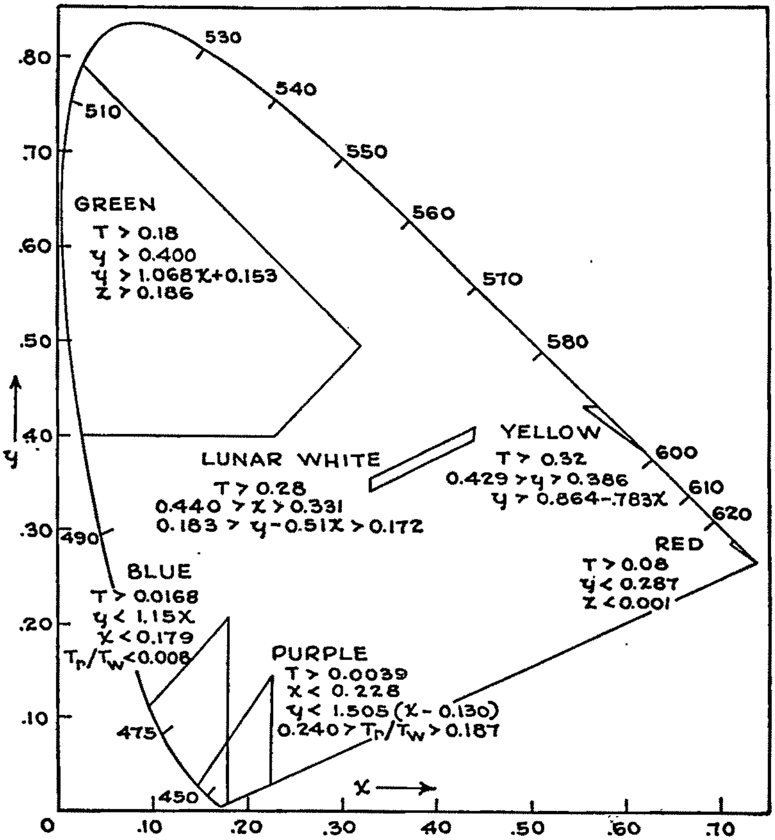

FIG. 1. CHROMATICITY DIAGRAM BASED ON 1931 ICI STANDARD COÖRDINATE SYSTEM FOR COLORIMETRY

these boundaries consist of straight lines demonstrates that convenience of specification was considered of greater importance than noticeability of color differences. Such a series of tolerance specifications could be made in any trichromatic coördinate system based on valid color mixture data. A coördinate system based also on just noticeable color difference data would have been of little assistance in the establishment of suitable tolerances for railway signal glasses.[4] Noticeability of color difference between signal glasses of the same kind probably would be overemphasized in importance, perhaps at the expense of convenience, by such use of a uniform chromaticity scale coördinate system.

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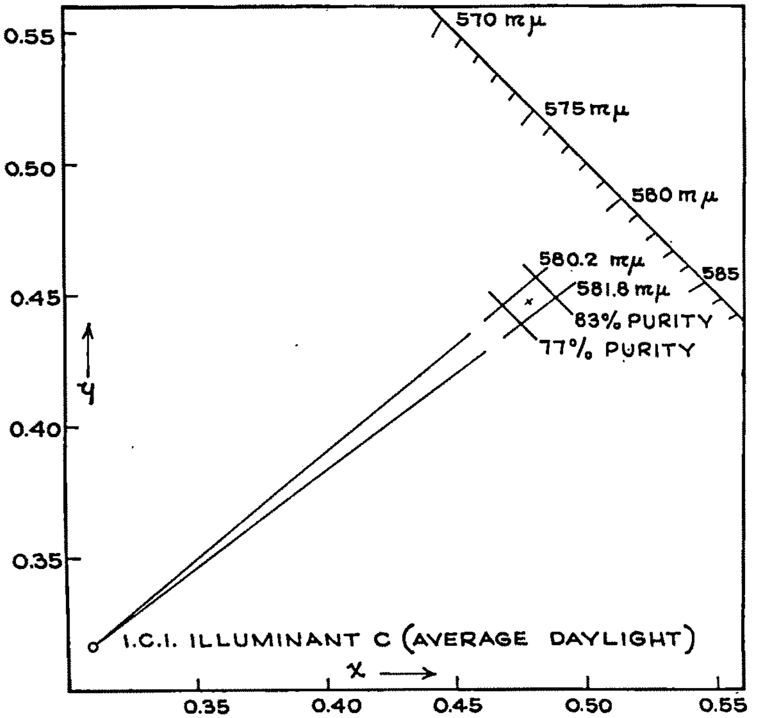
[3] D. B. Judd, A Maxwell triangle yielding uniform chromaticity scales, *J. Opt. Soc. Amer.*, 25, 1935, 24-35; Estimation of chromaticity differences on the standard ICI colorimetric coördinate system, *ibid.*, 26, 1936, 421-426.



	STANDARD	TOLERANCES
DOMINANT WAVELENGTH	581 mμ	580.2 - 581.8 mμ
EXCITATION PURITY	80%	77% - 83%
DAYLIGHT REFLECTANCE	54%	52% - 56%

FIG. 2. TOLERANCE LIMITS ADOPTED FOR THE INSPECTION OF A YELLOW REFLECTING MATERIAL

Fig. 2 shows the tolerance limits adopted for the inspection of a yellow reflecting material. Both upper and lower limits of daylight reflectance are included in this tolerance specification, because daylight reflectance is an important characteristic of the colors of reflecting surfaces. In this specification, the tolerance boundaries coincide with loci of constant dominant wave-length and excitation purity compared to the ICI illuminant C. This type of limit specification was adopted because the magnitude of the just acceptable variation of dominant wave-length appeared in repeated tests to be almost independent of all acceptable variations of purity and daylight reflectance; because the just acceptable variations of purity appeared to be almost independent of all acceptable variations of dominant wave-length and reflectance; because the just acceptable variations of daylight reflectance appeared to be independent of all acceptable variations of chromaticity; and because the expression of tolerances in terms of upper and lower limits of dominant wave-length, excitation purity, and daylight reflectance corresponded quite closely to the manner in which the inspectors customarily grade the samples, and classify the rejections. That noticeability of color variations is of only secondary importance in these tolerances is confirmed by the agreement of the inspectors that they can see the maximum acceptable variations of dominant wave-length more easily than they can see the maximum acceptable variations of purity. This corresponds closely to the fact that dominant wave-length, which depends quite critically on the concentration of the pigment applied to the surface, is more difficult to control in large-scale production of this material than are purity and reflectance, which depend, respectively, primarily on the composition of the pigment and on the quality of the surface to which the pigment is applied. In this, as in most commercial problems of color tolerance, the shape of the tolerance boundary on the chromaticity diagram is determined primarily by the relative costs of production control of the several probable variations of color. Colored materials of the kind specified in Fig. 2 and having high purity and low reflectance look soiled. Such samples having low purity and high reflectance look faded. Consequently, purity and reflectance variations from the standard are more objectionable than equally noticeable variations in dominant wave-length. In this case, therefore, production control difficulties combine with objectionability to make the just acceptable variations of purity and reflectance considerably less noticeable than the just acceptable variations of dominant wave-length.

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[4] T. Smith, The colour triangle and colour discrimination, *Report of Joint Discussion on Vision, University Press, Cambridge,* 1932, 212-226; F. C. Breckenridge and W. R. Schaub, Rectangular uniform-chromaticity-scale coördinates, *J. Opt. Soc. Amer.,* 27, 1937, 226; R. H. Sinden, A further search for the ideal color system, *ibid.,* 27, 1937, 124-131; 28, 1938, 339-347.

In general, it must be anticipated that, for some purposes, the size and shape of the tolerance boundary on the chromaticity diagram may depend on the luminous reflectance (or transmittance). The tolerances of photographic and photomechanical color reproduction appear to require some such dependence, which is not primarily determined by the variation of noticeability of color variations among samples having different brightnesses when the eye of the critic is maintained at a fixed state of adaptation.

Relation of noticeability to color tolerance. The examples just given are indications that noticeability of color variations is not the sole factor determining color tolerances, nor is it among the most important factors in determining the shape of the tolerance boundary on the chromaticity diagram. Only in cases where a 'perfect' color match is required, regardless of all other considerations, will noticeability be the most important consideration determining color tolerances. A perfect color match can only mean that the sample is indistinguishable from the standard under the conditions of examination of the samples. It is commonly assumed that the tolerances in such cases are proportional to the just noticeable color differences from the standard color. This assumption is reasonable provided that the just noticeable differences have been determined with the same conditions of observation as those prevailing at the time of comparison of the standard and sample materials. These conditions include the size and brightness of the colored areas being compared, and the distribution and color of the light in the surrounding field of view both before and during the comparison. No one set of data on just noticeable color differences will therefore be satisfactory for the derivation of color tolerances in the widely varying conditions of observation with which miscellaneous materials are examined and used. Consequently, a trichromatic coördinate system designed to represent just noticeable differences in some simple fashion would be valid for only one class of observations, and would be useful for the derivation of the tolerances for 'perfect' color matches only if the samples are to be examined under the corresponding conditions. For example, a coördinate system based on just noticeable differences for samples subtending a small angle at *O*'s eye and observed with an unilluminated surrounding field might conceivably be valid for the establishment of color tolerances for signal lights for use at night, but such a system would be quite misleading if used for the establishment of color tolerances for textiles used in surroundings well illuminated with various phases of daylight. The standardization of a chromaticity diagram which conveniently represents just noticeable differences with any one set of conditions of observation would encourage the use of that data for all problems of color tolerance regardless of the applicability of the data to the conditions of observation imposed by each specific problem.

Although no one uniform chromaticity scale diagram is of sufficiently general applicability to justify its standardization in place of the 1931 ICI standard coordinate system for general use in colorimetry, each uniform chromaticity scale diagram can be used advantageously for many problems in which the corresponding conditions of observation are employed. Each uniform chromaticity scale diagram is therefore desirable as an auxiliary to the 1931 ICI standard coördinate system. The derivation of such uniform chromaticity diagrams for all important sets of conditions of observation is a task worthy of serious investigation, and encouragement from standardizing committees. The transformation of data from the standard coördinate system to each of the uniform chromaticity scale diagrams should be put in the simplest possible form for computation.[5] If such transformation methods are made available, all data expressed in terms of the standard coördinate system can be easily transformed to the most suitable uniform chromaticity scale diagram. The ambiguity and inconvenience which might result from the wide-spread use of several coördinate system in colorimetry can be avoided if all published data are expressed in terms of the 1931 ICI coördinate system as well as in terms of whichever uniform chromaticity scale system is appropriate to the conditions of the special problem.

Summary. Actual practice in the establishment of color tolerances indicates that visual examination of a representative group of samples, and agreement between the manufacturers and representative users of the colored materials is more satisfactory to all parties concerned than any theoretical deduction of tolerances from abstract experiments. Tolerances established by such agreement can be represented just as clearly on the ICI chromaticity diagram as on any other chromaticity diagram. The ICI chromaticity diagram is recommended for such use because it has been standardized internationally and used extensively for many years, and because its use will not encourage any false simplifications of the color tolerance problem.

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[5] D. L. MacAdam, Projective transformations of ICI color specifications, *ibid.,* 27, 1937, 294-299.

SPECIFICATION OF COLOR TOLERANCES AT THE NATIONAL BUREAU OF STANDARDS

By Deane B. Judd, Washington, D. C.

The various branches of the Federal Government often request the services of the National Bureau of Standards in the preparation and certification of color standards used in the inspection of manufactured materials or articles. Similarly, various national organizations have sought and obtained the coöperation of the Bureau in such work. In Table I are listed some of the branches of the government and some national associations recently receiving such assistance, together with a description of the several materials dealt with and the kinds of working standards used.

TABLE I

Organizations Recently Receiving Assistance from the National Bureau of Standards, the Materials Dealt with, and the Kind of Working Standards Used

No.	Organization	Material	Working standards
1	Federal Specifications Executive Committee	paint	plaques of vitreous enamel on metal
2	Bureau of Navigation and Marine Inspection	glassware for marine signal lights	glass squares
3	Bureau of Air Commerce	glassware for aviation signal lights	glass squares
4	Department of the Interior	chinaware	chinaware
5	Post Office Department	decalcomania gold letters and numerals for Post Office trucks	decalcomania gold letters on cardboard plaques
6	Association of American Railroads	glassware for railway signal lights	glass squares
7	American Oil Chemists' Society	cottonseed oil	lovibond glasses
8	National Association of Sanitary Ware Manufacturers	sanitary ware	vitreous plaques
9	National Association of Hosiery Manufacturers	raw silk	plaques of vitreous enamel on metal
10	National Retail Dry Goods Association	kitchen and bathroom accessories	plaques of vitreous enamel on metal

There are usually three parts (A, B, and C) in a color specification for the purchase of materials. Part A is the specification in fundamental terms of the color or range of colors acceptable. This part of the specification includes a statement of the angular and spectral distribution of the illuminant, the angle of view, the spectral characteristics of the observer, and the coördinate system within which the color or range of color is specified. Part B is the specification of the working standards together with the tolerances which they must meet. Part C consists of the directions for the use of the working standards. These three parts are not always given explicitly, but they are implied in every color inspection accomplished by direct visual comparison with working standards. This analysis was first carried out by Gibson and Walker in their work on standardization of railway signal glasses.[1]

The purpose of a color specification is to assure that the colors of purchased materials shall fall within a prescribed range. Sometimes the selection of such a range is governed almost wholly by possible degree of manufacturing control, but a more usual consideration is the perceptibility of the color difference between the standard and the delivered unit. The methods of stating and administering color tolerances vary with the material to be purchased and its intended purpose. The present paper will discuss these methods and will show their dependence upon psychophysical data and techniques.

(1) *The 1931 ICI system.* The 1931 ICI standard observer and coördinate system is particularly applicable to the part of the color specification in which consideration of perceptibility of the color difference is not important, that is, to Part A. It has been used in Part A of Specifications 1, 2, 3, 6, 9, and 10 listed in Table I. The color range is indicated by specifying the permissible area on the mixture diagram (usually the (x,y)-diagram) combined with stated tolerances restricting luminous transmission of samples viewed by transmitted light, or restricting luminous apparent reflectance of samples viewed by reflected light.[2]

In Fig. 1 is shown an example (Specification 6, Table I) of the use of the (x,y)-diagram to indicate permissible chromaticity ranges. This diagram illustrates Part A of A.A.R. Signal Section Specification 69-38, Railroad Signal Glasses (exclusive of Kerosene Hand Lantern Globes). The permissible areas for red, yellow, green, lunar white, blue, and purple signal colors are indicated. The equations of the straight lines forming the boundaries of the areas are also given. Thus a green pressware glass would conform to Part A of this specification if, when combined with its intended illuminant, its trichromatic coefficients (x, y, z) be such that: (1) y be greater than 0.400; (2) z be greater than 0.186; (3) y exceed $1.068x$ by more than 0.153; and (4), its luminous transmission exceed a prescribed minimum value.

It will be noted that perceptibility of one green signal as different from another is of little concern in fixing these boundaries. It is only necessary to enforce the liberal tolerance that the signal be not too yellowish to be confusable with the yellow signal nor too pale to be confusable with the lunar white, and so on. These limits were set by the signal engineers by experiments with the whole group of signals under service conditions.

Part A of Specification 7 (Table I) is given in terms of the O.S.A. observer and coördinate system,[3] which was superseded by the ICI system in 1931 wherever feasible in the work of the National Bureau of Standards. Furthermore, Part A of Specification 5, given in the uniform-chromaticity-scale (UCS) system,[4] could have

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[1] K. S. Gibson and G. K. Walker, Standardization of railway signal glasses, *Signal Section Proceedings, A.R.A.* 30, 1933 (No. 5), 384.

[2] For definition of apparent reflectance consult H. J. McNicholas, Absolute methods in reflectometry, *Bur. Stand. J. Res.*, 1, 1928, 29-73.

[3] L. T. Troland, Report of committee on colorimetry for 1920-21, *J. Opt. Soc. Amer.* 6, 1922, 527-596.

[4] D. B. Judd, A Maxwell triangle yielding uniform chromaticity scales, *J. Opt. Soc. Amer.*, 25, 1935, 24-35; also *Bur. Stand. J. Res.*, 14, 1935, (no. 756), 41-52.

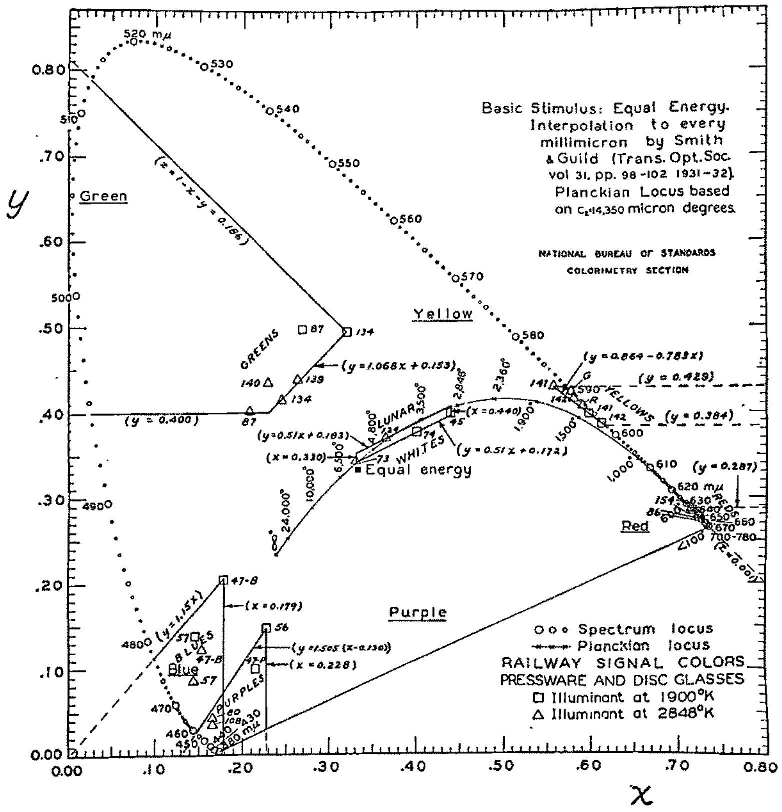

FIG. 1. MIXTURE DIAGRAM ACCORDING TO THE 1931 ICI STANDARD COÖRDINATE SYSTEM

This diagram illustrates Part A of the Association of American Railroads Signal Section, Specification 69-38, for railroad signal glasses exclusive of kerosene hand lantern globes. It is taken from their published specification and is an example of the use of the ICI system for specification of color tolerances.

been given in the ICI system, in agreement with the statement by Dr. MacAdam of its general applicability,[5] though somewhat less conveniently since Part B is particularly suited to the UCS system.

The ICI system is also used in Part B of Specifications 2, 3, 6, and 9 (Table I), but is in no case used in Part C.[6] There is no reason why each item of delivered goods should not be compared with the working standard by means of the ICI observer instead of by direct visual inspection except that the required spectrophotometry plus computation is usually much the more time-consuming. Furthermore, existing spectrophotometers are not well adapted to the measurement of many manufactured articles; for this reason Part A of Specifications 4, 8, and 9 is not expressed in any fundamental system. These specifications thus have to depend, for the present, upon the integrity of material master standards of color.

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[5] D. L. MacAdam, Representation of color tolerances on the chromaticity diagram, this JOURNAL, *supra*, 412-418.

[6] The actual application of working standards to the color inspection of manufactured articles or materials described in Part C of a color specification is carried out at the Bureau so infrequently that the methods scarcely come within the scope of this paper. For the sake of completeness, however, it may be mentioned that

(2) *Standard and tolerance sample.* Whenever the purpose of a color specification is to make sure that delivered goods depart from a standard color by as little as possible, Part A will be the specification of a single standard color, and Part B will include a color tolerance for the working standards. If, in addition a master standard in material form is available, the color tolerance for the working standards frequently takes the form of a tolerance sample. To apply such a tolerance the observer has to judge whether the color difference between the purported working standard and the master standard is greater or less than that between the tolerance sample and the master standard. In Dr. Boring's terminology, this is a sense-distance judgment corresponding in the critical case to the method of bisection (AB = BC.[7] In Dr. Newhall's terminology it is the "ratio method" in which the critical value of the ratio is unity.[8]

The directness of the method and its aptness for the central purpose of the color specification commend it; its only disadvantage is the lack of precision and reproducibility. It is common for even a trained observer to change his judgment by a factor of 2; and one trained observer of the Bureau staff will differ from another frequently by factors of 3 or 4.

This method was used until recently in Part B of Specification 8 (Table I), and it was also used in Part B of Specification 10, except for critical samples, because of its relative simplicity and rapidity.

(3) *Unit of surface-color difference.* In an effort to develop a method of setting color tolerances which would combine the aptness of the sense-difference judgment

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when Part A specifies a single color, Part C usually states the observing conditions (field size, illuminant, and so on) for the comparison of the working standard with the manufactured goods, and says that any appreciable difference warrants rejection (*e.g.* Specifications 8 and 10). If Part A specifies a color range that is essentially a one-dimensional continuum, Part B will specify working standards representing the limits of this range, and Part C will state that the color of the manufactured goods shall fall between the limits exemplified by the two working standards. The working standards of Specification 7 and some of Specification 3 are used in this way. If, however, the range of color specified by Part A is not a simple one-dimensional continuum, Part B will often specify more than two limit standards, and Part C will usually require the inspector to make a more complicated judgment such as "not greener than the green-limit standard" and "not lighter than the light-limit standard" and so on. The difficulty of these judgments arises from the necessity of the inspector having to respond to the algebraic sign of a difference in one attribute of color in the presence of fairly large differences in other attributes; that is, he has, for example, to judge the direction of a hue difference in the presence of a fairly large saturation or lightness difference, or both. These more complicated responses are required by Part C of Specifications 2, 3, 5, and 6.

[7] E. G. Boring, The psychophysics of color tolerance, this JOURNAL, *supra,* 384-394.

[8] S. M. Newhall, The ratio method in the review of the Munsell colors, this JOURNAL, *supra,* 394-405.

with the precision and reproducibility afforded by the ICI coördinate system, a program of research has been prosecuted at the National Bureau of Standards for several years. The research is not completed, but the preliminary techniques are sufficiently successful that the unit of mensuration defined by them has been called the NBS unit of surface-color difference. The techniques are furthermore applicable to many samples which cannot be measured by existing spectrophotometers; this applicability results from the design of reflectometers and chromaticity-difference colorimeters, both visual and photoelectric, suited to cylindrical as well as flat samples, and to a considerable range of sample size.[9]

(a) The uniform-chromaticity-scale triangle. The first step in this research program leading to the general derivation of scales of uniformly perceptible chromaticity intervals was taken by Priest and Brickwedde in 1926 when they determined sensibility to chromaticity differences among stimuli closely resembling sunlight.[10] Before a complete account of these determinations was published in 1938,[11] they had already been corroborated by three independent investigations.[12] The method used in all of these determinations was direct setting of the just noticeable difference (jnd).

The second step was a critical review of the literature of chromaticity sensibility which resulted in the discovery of an empiric relation with the O.S.A. colorimetric coördinate system.[13] The literature studied consists of about 80 papers, of which more than 20 supplied useful quantitative data, some based upon direct setting of the jnd, others on the precision of setting for a chromaticity match.

Other paths of the mixture diagram were then explored by Judd[14] and Tyndall,[15] and in 1935 these new data were combined with the old as guides in the derivation of a projective transformation of the ICI system whose mixture diagram plotted in triangular coördinates was intended to indicate by distance the perceptibility of differences between color stimuli. This is the uniform-chromaticity scale (UCS) system; it is the basis of the present NBS unit of surface-color difference. Fig. 2 shows the mixture diagram of the UCS system. Parts A and B of Specification 5, Table I, which is purely a chromaticity specification, make use of the UCS system.

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[9] R. S. Hunter, A null method photoelectric reflectometer, *J. Opt. Soc. Amer.*, 26, 1936, 225-230, also *Bull. Amer. Ceramic Soc.*, 15, 1936, 79-80; Problems in the development of a multipurpose reflectometer, *J. Opt. Soc. Amer.*, 27, 1937, 225-235, also *Bull. Amer. Ceramic Soc.*, 16, 1937, 90-94; D. B. Jull, A subtractive colorimeter for the measurement of small chromaticity differences between surfaces of moderate spectral selectivity of reflectance, *J. Opt. Soc. Amer.*, 26, 1936, 225-230; Specification of uniform color tolerances for textiles, *Textile Res.*, 9, 1939, 253-263, 292-308.

[10] I. G. Priest and F. G. Brickwedde, The minimum perceptible colorimetric purity as a function of dominant wave length with sunlight as neutral standard, *J. Opt. Soc. Amer.*, 13, 1926, 306-307.

[11] Priest and Brickwedde, Minimum perceptible colorimetric purity as a function of dominant wave length, *J. Opt. Soc. Amer.*, 28, 1938, 133-139, also *Bur. Stand. J. Res.*, 20, 1938, (no. 1099), 673-682.

[12] D. M. Purdy, On the saturations and chromatic thresholds of the spectral colours, *Brit. J. Psychol.*, 21, 1931, 283-313; L. C. Martin, F. L. Warburton, and W. J. Morgan, Determination of the sensitiveness of the eye to differences in the saturation of colors, *Spec. Rep. Ser. Med. Res. Coun. Lond.*, 1933, (no. 188), 1-42; W. D. Wright and F. H. G. Pitt, The saturation-discrimination of two trichromats, *Proc. Phys. Soc. London*, 49, 1937, 329-331.

[13] Judd, Chromaticity sensibility to stimulus-differences, *J. Opt. Soc. Amer.*, 22, 1932, 72-108.

[14] D. B. Judd, Sensibility to color-temperature change as a function of temperature, *ibid.*, 23, 1933, 3-14; Saturation scale for yellow colors, *ibid.*, 23, 1933, 35-45.

[15] E. P. T. Tyndall, Chromaticity sensibility to wave-length difference as a function of purity, *ibid.*, 23, 1933, 15-24.

(b) Derivation of the formula for color difference. The basic idea by means of which this formula for size of differences among surface colors was derived is that Euclidean geometry can be applied; that is, it is assumed that it is possible to represent surface colors by points so arranged as to make up a solid figure, known as the surface-color solid, within which Euclidean distances between points are proportional to the size or perceptibility, ΔE, of the color difference between the colors represented by the points.

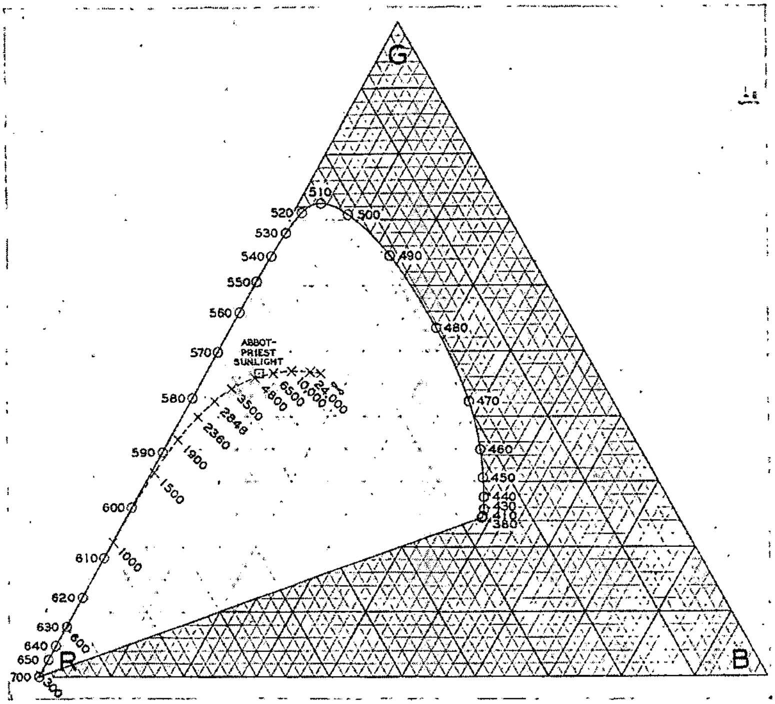

FIG. 2. THE UNIFORM-CHROMATICITY-SCALE TRIANGLE
Wave-length along the spectrum locus is indicated in mμ; color temperature along the Planckian locus is given in degrees Kelvin.

The first assumption on which the formula depends is that surface colors of the same lightness (Munsell term is value) can be represented on horizontal planes in the surface-color solid, and that variations in lightness at constant chromaticity are to be represented along lines perpendicular to the constant-lightness planes.[16] If we have, therefore, two colors, E_1 and E_2, having lightnesses, L_1 and L_2, and chromaticities, C_1 and C_2, this assumption yields by ordinary geometry the familiar distance formula: $(E_1-E_2)^2=(L_1-L_2)^2+(C_1-C_2)^2$ or, in the difference notation which we shall use hereafter:

$$\Delta E^2 = \Delta L^2 + \Delta C^2 \ldots\ldots\ldots\ldots [1]$$

The second assumption is that the true relation connecting lightness of the surface color with the luminous apparent reflectance, A, of the surface departs only by a negligible amount from the square-root relation; that is, we assume that:

$$L = k_1 A^{1/2} \ldots\ldots\ldots\ldots [2]$$

or, in difference form

$$\Delta L = k_1 \Delta\ (A^{1/2}) \ldots\ldots\ldots\ldots [2a]$$

Lightness is a function not only of the luminous apparent reflectance of the sample but also a function of that of the background against which it is viewed.[17] The formula proposed by Godlove is probably the most accurate available;[18] but we have chosen the simple relation [2] as a convenient approximation for both light and medium backgrounds because it is desired for the present to have the formula for color difference apply not to any particular background but to average viewing conditions. Equation [2] is a special case of the Godlove formula for luminous apparent reflectance of the background equal to unity; it also falls nearly within the spread of observers for a middle gray background.[19]

The third assumption deals with the use of the uniform-chromaticity-scale triangle. Although the experimental data from which this particular transformation of the standard 1931 ICI colorimetric coördinate system was derived are chiefly aperture colors of small angular extent (confined to the macula lutea) it is assumed that this triangle applies sufficiently closely to differences between surface colors of the same lightness viewed so as to subtend a large visual angle (not confined to the macula). This assumption would permit us to write for chromaticity difference, ΔC, between surface colors of any one lightness:

$$\Delta C = k_2\, \Delta S \ldots\ldots\ldots\ldots [3]$$

where:

$\Delta S^2 = (r_1 - r_2)^2 + (g_1 - g_2)^2 + (b_1 - b_2)^2$, the trilinear coördinates of the first color being (r_1, g_1, b_1) and those of the second (r_2, g_2, b_2).

The fourth assumption has to do with the experimental fact that, under ordinary conditions of inspecting samples for color, a given chromaticity difference corresponds to a larger trilinear-coördinate difference for dark samples than for light. It is assumed that equation [3] extended to colors of all lightnesses should be written:

$$\Delta C = k_2 A^{1/4} \Delta S \ldots\ldots\ldots\ldots [4]$$

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[16] Chromaticity of a chromatic color is determined by the hue and saturation attributes of the color; see L. A. Jones, Colorimetry: Preliminary draft of a report on nomenclature and definitions, *J. Opt. Soc. Amer.*, 27, 1937, 207-213.

[17] E. Q. Adams and P. W. Cobb, The effect on foveal vision of bright (and dark) surroundings, *J. Exper. Psychol.*, 5, 1922, 39-45; E. Q. Adams, A comparison of the Fechner and Munsell scales of luminous sensation value, *J. Opt. Soc. Amer.*, 6, 1922, 932-939.

[18] I. H. Godlove, Neutral value scales: II. A comparison of results and equations describing value scales, *ibid.*, 23, 1933, 419-425.

[19] A. E. O. Munsell, L. L. Sloan, and I. H. Godlove, Neutral value scales: I. Munsell neutral value scale, *ibid.*, 23, 1933, 394-411.

This formula would state that a deep 'red' is not a perceptibly different color from a deep 'green,' provided the surfaces exemplifying them have sufficiently low luminous apparent reflectances. The reason why the eye could not detect the color difference between two such samples (both of which would appear black) is that the relatively much stronger stimulation of neighboring retinal areas prevents it by irradiation and contrast effects. Equation [4] like equation [2] holds, therefore, most closely for inspection of samples under the ordinary condition that the surrounding field is lighter than dark samples, but if precautions were taken to have no part of the retina receive greater illumination than the part on which the image of the two samples falls, then equation [4] would not hold; on the contrary, equation [3] would probably apply to colors of all lightnesses without restriction. The reason for choosing the fourth root of the luminous apparent reflectance, A, in equation [4] rather than some other function of A is first because of its simplicity, and second because the samples of the Munsell system approximately follow this relation.

By substitution of [2a] and [4] in [1], for $k_1 = 120$ and $k_2 = 600$, we obtain the formula for size of color difference which has been tested in a preliminary way for conformity to the results of direct visual estimates.

$$\Delta E = 600\ [A^{1/2}\Delta S^2 + 0.04\Delta(A^{1/2})^2]^{1/2} \qquad [5]$$

The ratio of the constants k_1 and k_2 has to be adjusted so as to evaluate lightness differences relative to chromaticity differences in the same way as the average inspector weights them. The ratio 120:600 is a tentative choice based on a few preliminary experiments for samples (such as textiles and papers) which can be compared with a dividing area either non-existent or very narrow. It is known that the presence of a conspicuous dividing area interferes with the perception of chromaticity differences,[20] but preliminary trials indicate that the perception of lightness differences is even more reduced. For a narrow dividing area contrasting with the samples by about the lightness difference betwen black and mid-gray (such as is common in the comparison of two plaques of vitreous enamel on metal, Specifications 1, 8, and 10), better agreement with direct visual estimate is found by taking $k_2 = 90$ or 100 instead of 120; separation by a conspicuous broad design of the areas to be compared requires k_2 as small as 30 or 40 for good agreement.

The sum of the constants, k_1 and k_2, determines the size of the unit of color difference; it has been adjusted with the intention of choosing a unit color difference of such size that smaller differences can be neglected in the average commercial transaction involving a color match but larger differences cannot. This adjustment refers to observation of fairly large samples (say 10 cm. squares at a distance of 25 cm.) and is also tentative; it gives 120 units of color difference from black to white, and about 300 units between the strongest possible red and the strongest possible green for samples of daylight apparent reflectances of 0.10.[21] This makes the surface-color solid a rather squat figure, the maximum horizontal dimension being nearly 3 times that of the maximum vertical dimension.

Formula [5] must be restricted to matt surface colors. With glossy samples, under the usual inspecting conditions, most specularly reflected light is directed away from the eye of the inspector, but with this customary precaution there still remains enough specularly reflected light to interfere significantly with the perception of color differences between dark samples. There is less interference for samples of medium lightness and little or none for near-white samples. Improved agreement for glossy samples has been obtained by expanding the constant, 600, into the following function of the specular gloss, G_s, and luminous apparent reflectance, A, of the samples: $(600\ AB_o)/(AB_o + 0.05\ G_sB)$ where B_o is the brightness which a uniform hemisphere over the sample would need have to give the sample its actual illumination, and B is the brightness of the area imaged in the sample.[22] It will be noted that if the area imaged in the sample be completely dark $(B = 0)$ or if the sample be matt $(G_s = 0)$, this function reduces to the simple constant, 600. For inspection in an ordinary room near a window by light from the sky, values of $B = B_o/2$ are common; so for typical inspection of glossy vitreous plaques $(G_s = 1)$ the function becomes: $600A/(A + 0.025)$. This function departs but slightly from 600 for near-white samples $(A > 0.70)$ but drops to 300 for dark samples for which $A = 0.025$. This accords approximately with our preliminary results.

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[20] Judd, Precision of color-temperature measurements under various observing conditions: A new color comparator for incandescent lamps, *Bur. Stand. J. Res.*, 5, 1930, 1161-1177.

[21] D. L. MacAdam, Maximum visual efficiency of colored materials, *J. Opt. Soc. Amer.*, 25, 1935, 361-367.

(c) Use and test of the NBS unit. The NBS unit of surface-color difference has been used in Part B of Specifications 4, 8, and 10. Several hundred differences between these vitreous enamel samples have been evaluated in this way and have been checked by comparison with direct visual estimates of the sense differences (the ratio method). This study is continuing. Some consistent discrepancies by about a factor of 2 were discovered in this way which suggested that the third assumption, the one dealing with applicability of the UCS system to surface colors of large angular extent, was not wholly justified. The amount of the discrepancy is about equal to the uncertainty of judgments obtained by a single observer by the ratio method, and was considered encouragingly small.

However, a coöperative study of the formula with the Du Pont Technical Laboratory by means of samples dyed for that purpose, corroborated these discrepancies, and showed for certain deep reds and blues that the formula was in error through failure of the third assumption by about a factor of 4. These experiments are described in a recent article including details of construction and calibration of the colorimeter used,[23] and also a comparison of this formula with the Nickerson Index of Fading now being studied by a subcommittee of the American Society for Testing Materials.[24]

An analysis of the nature of the discrepancies shown indicates that a considerably improved agreement could be obtained by moving the daylight point (near 6,500°K, see Fig. 2) away from the spectrum at 570 mμ to a point about twice as distant as the present point, and at the same time shortening the distance of the daylight point from the long-wave extreme of the spectrum. Both of these changes could be made by an homogeneous, linear transformation; so formula [5] would not be changed in form, but the coördinate system within which the term, ΔS, is evaluated would be different. This suggested new coördinate system would not apply so well to chromaticity-sensibility data on aperture colors of small angular extent (confined to the macula lutea) as the present uniform-chromaticity-scale system; but it is probably significant that both the increased distance between the daylight point and the spectrum at 570 mμ and the decreased distance to the long-wave extreme of the spectrum are consistent with the use of a large retinal area not confined to the macula lutea. Note that a 10 cm. sample viewed at the distance of most distinct vision (about 25 cm.) would subtend an angle of 22°; this is more than 5 times the diameter of the average macular pigment.

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[22] Judd, Uniform tolerances for surface-color specification, *ibid.*, 28, 1938, 52-56.

[23] Judd, Specification of uniform color tolerances for textiles, *Textile Res.*, 9, 1939, 253-263, 292-308.

[24] Dorothy Nickerson, The specification of color tolerances, *Textile Res.*, 6, 1936, 505-514; How can results of fading tests be expressed, *A.S.T.M. Stand. Tex. Mat.*, 1936, 238-241.

Whether the suggested new coördinate system will result in the expected improved agreement for deep reds and blues and, at the same time, avoid introducing discrepancies for other colors, remains to be determined by trial. If it should fail, the more complicated and more flexible formulation by Sinden,[25] which also is consistent with an Euclidean arrangement, could be tried next; then completely unrestricted (non-Euclidean) forms.

Summary and conclusions. The various parts of a color specification to be administered by working standards have required different methods for color-tolerance specification. Choice of method is also affected by the article whose color is specified and by the instruments used. Three chief methods are discussed: (1) the standard ICI coördinate system; (2) material standard and tolerance sample; and (3) the NBS unit of surface-color difference.

The ICI system applies aptly to color specifications not requiring account of the perceptibility of differences; it yields a precise and reproducible specification of color tolerance in fundamental terms. The use of a material standard and tolerance sample requires a sense-distance judgment, and within the limitations of precision of that judgment is perfectly adapted to insure tolerances of uniform perceptibility. The NBS unit of surface-color difference is intended to combine the precision and reproducibility of a fundamental system with the aptness of the sense-distance judgment by means of a tolerance sample. The formula for color difference defining this unit makes use of sensibility data obtained over a period of years by psychophysical methods, and in effect interpolates between the various series of stimuli for which color-difference sensibility is known. The NBS unit of surface-color difference suffers from two disadvantages: first, the psychophysical data on which it is based are so small a proportion of the total required for reliable evaluation of the complete surface-color solid that attempts to use the unit still reveal ways in which the interpolations yielded by the color-difference formula may be significantly improved; and, secondly, the unit varies with observing conditions so that the color-difference formula defining it is necessarily rather complicated. The NBS unit is already successful enough to justify its use in some standardization work, but our research continues to be directed toward its improvement. It should be emphasized therefore, that changes in definition of the unit are to be expected, but it is believed that a practical unit of surface-color difference will result from our study, and that the definition of the unit will be no more complicated than the tentative form discussed here.

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[25] R. H. Sinden, A further search for the ideal color system: I. A new mechanicographical method, *J. Opt. Soc. Amer.*, 27, 1937, 124-131; II. A reconsideration of the Helmholtz line element, *ibid.*, 28, 1938, 339-345.

INDUSTRIAL COLOR TOLERANCES

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The scope of the subject of industrial color tolerances embraces such a wide field that we shall discuss only those phases, defined in the previous papers, which have direct application to the problems of evaluations of color differences, tests on consumers acceptance, and production control of color uniformity. Since the author's connections for the past three years have had to do with ceramic glazed wall tiles, problems of industrial color tolerances will be discussed in their relation to that particular industry. In so far as the principles discussed are of fundamental nature, they will be found to be also applicable to other industrial or commercial products.

The ceramic industry is a good proving ground for finding out the usefulness and applicability of recently developed methods and techniques for measuring color and color differences. There are several reasons why such is the case. (1) The variety of available colors covers a wide range of hues with large variations in brightness and saturation. (2) The colors are permanent. This is particularly important if colors are to be measured on different instruments, or the precision of the instrument itself is to be determined. (3) The surface texture of tiles is fairly uniform. (4) In the present state of manufacturing and color control there is a large color variability from tile to tile. (5) The size of the tiles being 4¼ x4¼ in. makes them very easy to handle and their color can be measured on standard instruments. (6) And finally, there seems to be a rather consistent demand for a close color matching between the tiles used in the same installation.

(1) Standard conditions. An important factor which gave impetus to the considerable amount of activity going on at present in the field of color measurements is the group of resolutions adopted by the International Commission on Illumination which met in Cambridge, England, in 1931.[1] For surface-color specification it was found necessary to establish certain standard conditions dealing with the light source, visual response of a standard observer, viewing conditions, and proper coordinate system. The adoption of standard conditions made it possible to specify uniquely any color in terms of trichromatic coefficients. At the present time these standards are adopted by scientific workers in colorimetry all over the world. Useful as such specifications are, they ought to be considered only as the first step toward solutions of many industrial problems where color is an important attribute of manufactured goods. The I.C.I. system of color specification deals primarily with color *identity.* Two colors are defined and appear to be identical under standard conditions when their trichromatic coefficients are numerically the same. Nothing

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[1] D. B. Judd, The I. C. I. standard observer and coördinate system for colorimetry, *J. Opt. Soc. Amer.,* 23, 1933, 359-374. T. Smith and J. Guild, The C. I. E. colorimetric standards and their use, *Trans. Opt. Soc., London,* 33, 1931, 73-134.

is said about the evaluation of color variability or color differences. In fact, the possibility of expressing color differences as numerical quantities is considered by many as impossible or meaningless. Yet, if the definition of color is based upon a definite set of physical operations,[2] there is no reason why the definition of color differences could not be based also upon some other set of physical operations.

The I.C.I. system of color specifications is frequently referred to as the *objective* method of color specifications, whereas the question of numerical evaluation of color difference is considered to deal with purely *subjective* aspects which do not lend themselves to mathematical computations. It must be remembered, however, that the specification of color identity does involve an element of subjectivity which comes from the incorporation of psychophysical data into the I.C.I. specifications. Since our present mode of color specification involves a human element determined by the response of the retina to different radiant energy as a function of wave-length, it may be possible to establish also a scale of color differentiation determined by the response of the human eye to stimulus-differences. A quotation from a paper by Hardy stresses the fact that this problem is by no means a simple one. "If it happens that they (two colors specified in I.C.I. system) would not look alike, it is ordinarily impossible to specify the degree of their difference in terms of the color difference that a normal observer could just perceive. The determination of just perceptible differences for a large gamut of colors under a wide variety of observing conditions is a task whose magnitude dwarfs the development of the science of colorimetry. Nevertheless, it is an investigation that can profitably be undertaken, now that the imperishable language of color has been devised."[3]

We are facing now a situation in which the standard conditions for specifying color-sameness enumerated by I.C.I. will have to be supplemented by other factors which affect the appearance of color differences. Among these we may enumerate the following: (1) size of the viewing field; (2) line of demarcation between the colors; (3) intensity of illumination; (4) color of the background; (5) and finally, condition of observer's light adaptation. It is only when we give complete specifications for these additional requirements that we can even begin to talk intelligently about the problem of color differences.

In considering color differences of tiles each one of the above factors was predetermined by the actual conditions of wall tile in use. The viewing field was taken as corresponding to the angle subtended by two tiles under comparison at a distance of approximately 18 in. The line of demarcation was limited to a condition resulting when the tiles were placed in contact with each other. Observation was by daylight illumination in the neighborhood of 100–200 foot-candles. The background was neutral gray having approximately the same luminous apparent reflectance as the samples compared. The observers were light adapted.

(2) Visual estimation of color differences. The possibility of establishing surface color tolerances upon a firm scientific basis leads to two requirements: first, physical measurement of color and its evaluation as a numerical quantity; and secondly, establishment of a correlation between color differences measured physically and visual perception of such color differences. The development of physical instruments

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[2] A. C. Hardy, Handbook of colorimetry, *Mass. Inst. Tech.*, 1936, 1-8.

[3] Hardy, The physical basis of color measurement, *J. Appl. Phys.*, 8, 1937, 233.

for measuring color and the adoption of the standard I.C.I. specifications gave the necessary essentials to satisfy the first requirement.

The second requirement is still in the process of development. In fact, some men of science even doubt whether such correlation can be established at all. The question is, whether color differences can be measured and, when expressed numerically, to correspond to the sense perception of such color differences. On purely philosophical or philological basis there may be a valid objection to the use of the word 'to measure' as applied to sensation, rather than to stimulus. If the true meaning of measuring is to depend upon determination of space-time coincidences, then of course there could be no meaning in speaking of measuring color differences, unless it were possible to find some process by means of which space coincidences could be established. Such an objection was raised by Guild,[4] in his criticism of the paper read by Richardson[5] during a symposium on vision sponsored by the Physical and Optical Societies in England.

From a practical standpoint such differences of opinion are only of secondary importance. The cardinal questions are: first, whether numerical magnitude of color differences can be estimated by direct visual observation; and secondly, whether there is an agreement between various observers concerning the magnitude of these color differences. The only way in which these questions can be answered is by a direct experimental test on a representative group of observers. It ought to be clear, therefore, that even before any attempt is made to discover a mathematical function applicable to the evaluation of color differences, we must be sure that both questions raised above can be answered in the affirmative.

Let us consider three surface colors designated by E_1, E_2, and E_3, which differ from each other in all three attributes: hue, lightness, and saturation. If we assign a unit value as the difference in color sensation between E_1 and E_2, that is $E_1 - E_2 =$ 1 unit of color difference, is it possible to give a numerical visual estimation of color differences between E_1 and E_3 or E_2 and E_3 in relation to the color difference between E_1 and E_2? In other words, can an observer, after viewing and comparing these three samples, state that $E_1 - E_3$ is equal to, say, 5 units and $E_2 - E_3$ is equal to, say, 3 units of color difference? And, further, if one observer would give such numerical values for his visual estimation, to what extent would they agree numerically with similar visual estimations of other observers?

In order to answer these questions the following experimental procedure was followed. Thirty-eight green tiles were selected at random and their spectrophotometric reflectance curves were made on a Hardy recording spectrophotometer. The color of these tiles varied from yellow green to blue green, from light to dark, and they were of various degrees of saturation. Trichromatic coefficients were computed from these curves for Illuminant C and each color was plotted on the I.C.I. mixture diagram. The apparent luminous reflectance of these colors was also designated by placing their values next to every point on the chromaticity plane. Every color was then designated by a consecutive number from 1 to 38, according to its increasing brightness. Five different colors were then selected to give a fairly wide range of variations in hue, brightness, and saturation, their numbers were

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[4] J. Guild, Color standardisation, *Nature,* 129, 1932, 453-455.

[5] L. F. Richardson, The measurability of sensations of hue, brightness or saturation, *Rep. Joint Discussion on Vision,* 1932, 112-116.

12, 21, 24, 26, and 37. Fig. 1 is a reproduction of their spectral reflectance curves measured in the Colorimetry Section of the United States Bureau of Standards. Table I gives the trichromatic coefficients, dominant wave-lengths, luminous apparent reflectance, and purity computed by 10 mμ method for I.C.I. illuminant C.

The color difference between tile 37 and 26 appeared to be smaller than any other pair in this group of five colors and it was, therefore, assigned a value of one unit of color difference. The author proceeded then to compare color differences between all other color-pairs with this standard pair. The question proposed was: If the color difference between 37 and 26 is one, what is the color difference between 37 and 12? It was the problem of estimating directly the ratio of color difference between 37 and 12 in relation to the color difference between 37 and 26. In the form of an equation in which E stands for color and the subscript indicates the reference number of any one of the colors compared we will have $(E_{37}—E_{12})/E_{37} — E_{26} = (E_{37} — E_{12}) = R_{37\text{-}12}$ since $E_{37} — E_{26} = 1$, where $R_{37\text{-}12}$ represents the numerical ratio thus estimated.

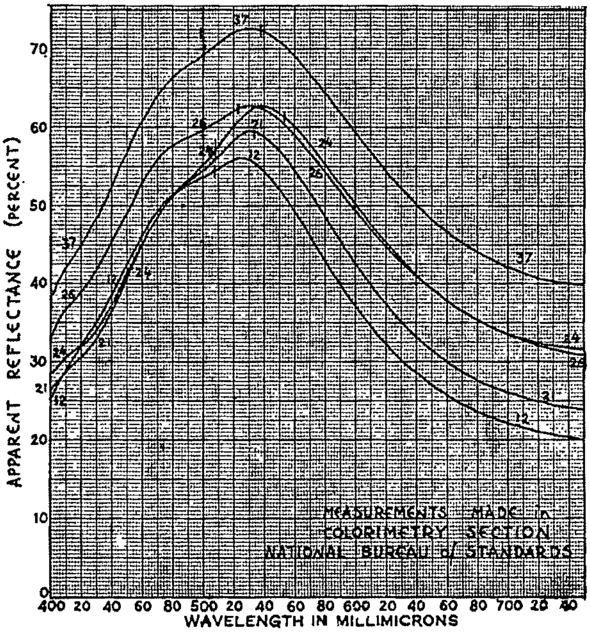

FIG. 1. SPECTROPHOTOMETRIC REFLECTION CURVES FOR FIVE GREEN COLORS USED IN ESTIMATION OF COLOR DIFFERENCES

Vertical cross-line on each curve indicates dominant wave-length for I.C.I. illuminant C.

Such a process of color difference estimation is the ratio method. With five different colors, n, the maximum possible number of pairs to be compared, N, is given by the formula: $N = n\ (n — 1)/2$, where for n equal to 5, $N = 10$. Since one

TABLE I

I.C.I. SPECIFICATIONS FOR 5 GREEN COLORS COMPUTED BY 10mμ METHOD FOR ILLUMINANT C

Color No.	Trichromatic coefficients x	y	z	Luminous apparent reflectance A%	Dominant wave-length (λ, mμ)	Saturation P%
12	.2870	.3443	.3688	47.39	506.8	7.6
21	.2999	.3546	.3454	51.17	533.4	8.1
24	.3122	.3615	.3263	56.14	553.6	12.9
26	.2995	.3422	.3583	56.34	523.6	5.1
37	.3044	.3431	.3525	66.24	538.0	6.0

pair is already assigned a unit of color difference, only nine additional pairs have to be estimated.

Color differences estimated by the author fell between one and six times as large as the standard unit color difference. Two other *O*s, D.B.J. and R.S.H., immediately participated in this test and it was rather surprising to find that in two instances the numerical estimates of all 3 *O*s were exactly the same, and that in 5 instances at least 2 *O*s had the same numerical ratio for color differences. The estimates of all the *O*s were made, of course, without the knowledge of the numeri-

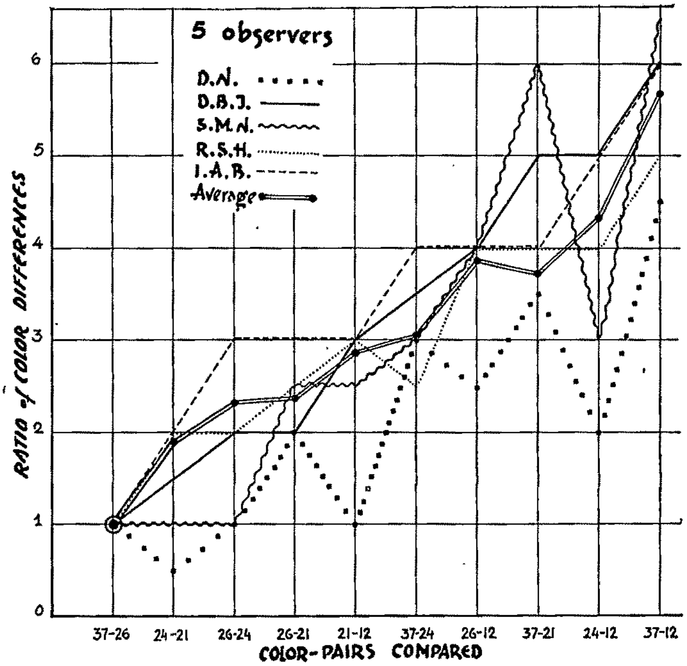

FIG. 2. NUMERICAL ESTIMATIONS OF COLOR DIFFERENCES BY RATIO METHOD ON FIVE GREEN COLORS

Color-pairs are arranged according to ranking of the three first observers D.B.T., R.S.H., and I.A.B.

cal values of any of the preceding *O*s. The rank of the increasing magnitude of color differences was chosen according to the estimates of these 3 *O*s which together with estimates of 2 other *O*s are shown in Fig. 2. Similar tests were made on 55

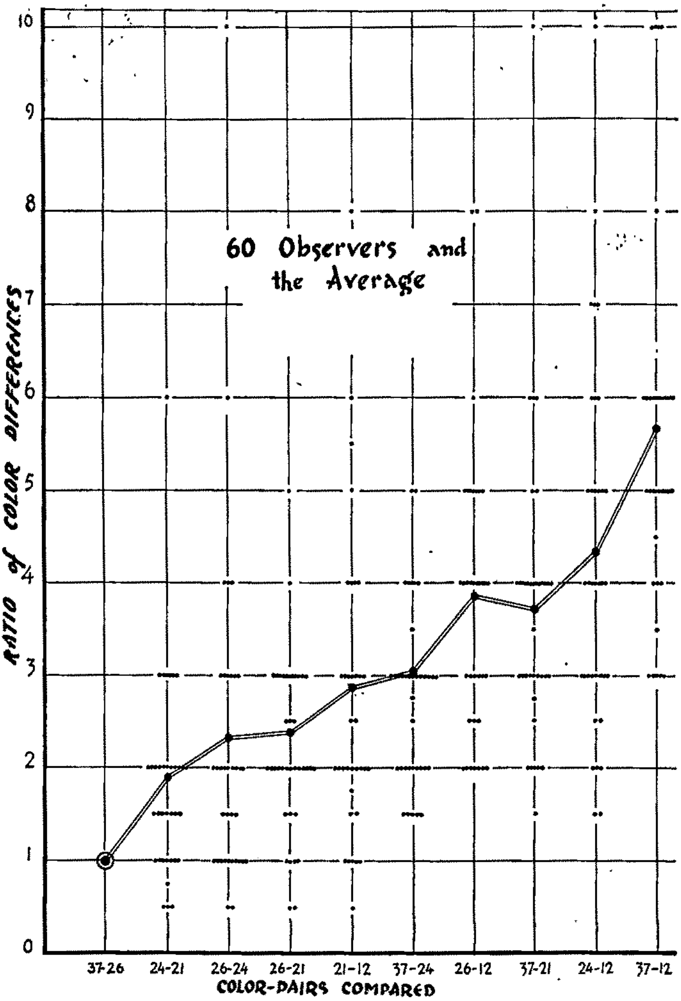

FIG. 3. NUMERICAL ESTIMATIONS OF COLOR DIFFERENCES BY RATIO METHOD ON FIVE GREEN COLORS (60 *O*s)

other *O*s and the results are presented in Fig. 3. Every point in this graph represents a single numerical estimate of color difference for a pair of colors shown along the abscissa.

In general there was no difficulty in obtaining such estimates. It was clearly explained to every *O* that color differences were to include the three attributes of color, *i.e.* differences in hue, lightness, and saturation, all combined in a single numerical estimation in relation to a standard color difference. Some of the *O*s did experience a reluctance in naming a specific number for such ratios, although they could point out without any difficulty pairs of colors which they thought to show larger or smaller color variations. Such color variations between different pairs were made from the standpoint of acceptance as to a better color match. In some instances the *O*s resorted to a method of mental counting of a number of equally appearing intervals corresponding to a chosen unit of color difference. The time taken by different individuals to complete the test ranged from 10 to 30 min. The record of estimates was made by every *O* on the following form:

> If the total color difference between tile 37 and 26 is taken to be equal to 1, what number would represent, according to your estimation, the total color difference between:

37-26	(1)	37-24	( )
37-12	( )	26-24	( )
26-12	( )	24-21	( )
37-21	( )	24-12	( )
26-21	( )	21-12	( )

After the card was filled a check was made to be sure that the estimates were consistent. This was done by cross comparison of numerical estimates for different color-pairs, to see whether their ranking was the same when compared pair by pair, rather than in reference to a standard unit color difference. For example, if 37-21 was given a ratio of 4 and 24-12 was given a ratio of 3, these two pairs were intercompared to be sure that *O* considered that the color differences were representative of his estimates between these two pairs taken by themselves. Although in some cases such cross-checking resulted in corrections of the original estimates, in every instance *O* was better satisfied with his estimates as a true representation of his opinion.

An examination of Fig. 3 shows that the distribution of color-difference estimates is sufficiently consistent to permit a computation of a fairly representative average. This is indicated by a double line drawn through solid circles. It must be said, however, that in certain instances there exists a considerable deviation among different *O*s in their numerical estimates of color differences. This is particularly true when color difference for one pair is due mainly to a variation in lightness, whereas in another pair it may be due mainly to a variation in hue or saturation. It was definitely shown that some of the *O*s had a tendency to give more consideration to differences in lightness, whereas others relied almost entirely upon the differences in hue. *O*s with previous training in color grading were very determined in holding to their original estimates, even when shown that their values were far from the averages. About half of the *O*s had some previous experience in visual color comparison, while the rest did not have any training whatsoever. On the whole, the results of this test are considered to be a sufficient proof that there exists a visual conception of color differences which can be expressed as a numerical magnitude.

(3) Evaluation of color differences from physical measurements. The problem, as it presents itself now, is to find a mathematical function which will express color differences from physical measurements of colors, and which will agree with

the average visual evaluation of such color differences, as shown in Fig. 3. This problem resolves itself into two parts: first, a numerical color specification based on physical measurements giving three psychophysical parameters for each color; and secondly, a mathematical equation by means of which the six parameters necessary for a specification of two colors could be reduced to a single number. To be of practical value, these measurements must be accurate, reproducible, simple and rapid. The use of Hardy photoelectric spectrophotometer in detecting and measuring small color differences was described by Nutting.[6]

The following methods were employed in measuring the psychophysical color specifications. (1) Spectrophotometric reflectance curves drawn by Hardy spectrophotometer from which dominant wave length, brightness and saturation were computed by the 10mμ method for I.C.I. illuminant C. (2) Disk colorimetry with Munsell disks evaluated in terms of I.C.I. specifications.[7] (3) Judd's wedge colorimeter.[8] (4) Hunter's multipurpose reflectometer.[9]

In evaluating color differences the following procedures were used. (a) Nickerson index of fading.[10] (b) New Nickerson's formula. (c) Judd's equation based on data from spectrophotometry,[11] subtractive colorimetry,[12] and Hunter's reflectometer.

(a) Nickerson's index of fading. A method of evaluating color differences was proposed by Nickerson, based upon Munsell[13] notation of colors in terms of hue, value (lightness), and chroma (saturation). It is assumed for the purposes of the proposal that the total variation in color between two samples may be expressed as a sum of the difference in hue, value, and chroma. Coefficients, based on certain experimental evidence, are assigned to each one of these attributes so as to place them on an equal basis as far as the magnitude of visual perception of color differences is concerned. This sum is designated as the "Index of Fading" and its numerical evaluation is made by the use of the following formula:[14]

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[6] R. D. Nutting, The detection of small color differences, *Textile Res.* 4, 1934, 323, 323-330; Color tolerance: Its measurement and specification for dyed textiles, *ibid.*, 6, 1935, 104-114.

[7] Dorothy Nickerson, Disk colorimetry; including a comparison of methods for computing tristimulus values for certain disks, *J. Opt. Soc. Amer.*, 25, 1935, 253-257; Use of I.C.I. Tristimulus values in disk colorimetry, *U. S. Dept. Agri.*, May, 1938.

[8] Judd, A subtractive colorimeter for the measurement of small chromaticity differences between surfaces of moderate spectral selectivity of reflectance, *J. Opt. Soc. Amer.*, 26, 1936, 225-230.

[9] R. S. Hunter, Problems in the development of a multi-purpose reflectometer, *J. Opt. Soc. Amer.*, 27, 1937, 225-235; F. Scofield and C. R. Cornthwaite, Measurement of the fading of tints, *Nat. Paint, Varnish, & Lacquer Ass.*, Circular No. 548, 1939.

[10] Nickerson, How can results of fading tests be expressed, *A. S. T. M. Standards on Textile Materials*, Sub-Committee A-7, Committee D-13, October, 1935; The specification of color tolerances, *Textile Res.*, 6, 1936, 505-514.

[11] Judd, Specification of uniform color tolerances for textiles, *Textile Res.*, 9, 1939, 253-263, 292-308. Uniform tolerances for surface-color specification, *J. Opt. Soc. Amer.*, 28, 1938, 52-55.

[12] Judd, Specification of color tolerances at the National Bureau of Standards, this JOURNAL, *supra*, 418-428.

[13] A. H. Munsell, *A Color Notation*, 1936, 1-40.

[14] The evaluation of the 'Index of Fading' by this formula gives a numerical value for I six times as large as the value computed by the first Nickerson's formula, given in the publication referred to. At present formula [1] is recommended by Miss Nickerson for computation of color differences.

$$I = (C/5)\ 2\Delta H + 6\Delta V + 3\Delta C \qquad [1]$$

where H stands for hue expressed in 100 steps along the hue circle, V is the value, and C is the chroma in Munsell notations.

In order to evaluate the Index of Fading it is first necessary to express each color in Munsell notations. Using the trichromatic coefficients and the charts prepared by Nickerson, based upon the Hardy-Glenn-Killian data,[15] it is a simple matter to convert I.C.I. specifications into the necessary notations of hue, value, and saturation. The Index of Fading was computed for each of the 10 color-pairs and the ratio of color difference was taken to that of a standard pair. These values are represented by circles in Fig. 4.

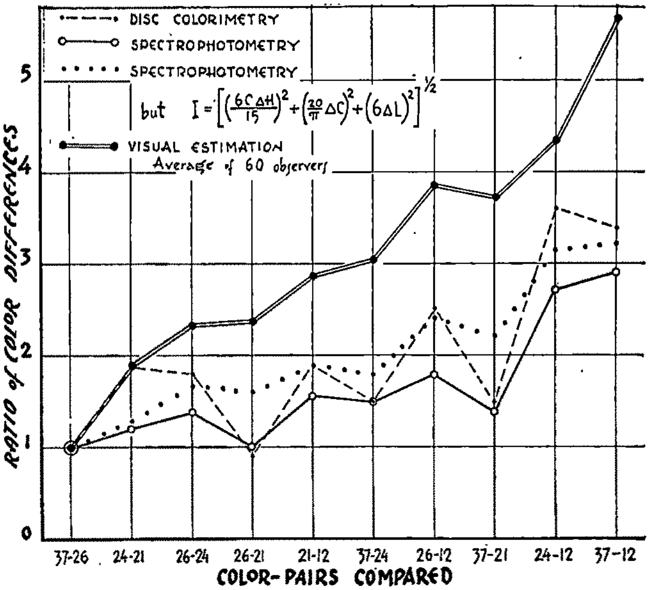

FIG. 4. COMPARISON OF COLOR-DIFFERENCE RATIOS EVALUATED BY NICKERSON'S FORMULA
(60 *Os*)

Munsell notations were also determined from disk colorimetry. The ratios of color-differences were evaluated again for formula [1] and plotted as shown by the broken line in Fig. 4. The discrepancy between the Munsell notations obtained from spectrophotometric curves and disk colorimetry is fairly large and there are probably several independent factors which may account for it.

These ratios of evaluated color differences may now be compared with those obtained by direct visual estimation, also shown in Fig. 4 by a double line. It can be seen that the color differences given by Nickerson's formula are too small by a factor of about two as compared with visual estimations.

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[15] It is expected that these data will be published in the forthcoming Report of the Colorimetry Committee of the Optical Society of America.

(b) Nickerson's new formula. The expression for Index of Fading as given by formula [1] does not represent a metric in a Euclidean space because the terms are not squared and the value of π, as a ratio between changes in hue to changes in chroma is not equal to 3.14. In order to modify Nickerson's formula to a Euclidian form it is necessary to change the relative weights assigned to differences in hue, value, or saturation. If we choose to preserve the same relative weights for the attributes of hue and value, but change chroma so as to conform to the requirements of the Euclidian geometry, we get the following expression for Nickerson's Index of Fading:

$$I = [(c/5 \cdot 2\Delta H)^2 + (6\Delta V)^2 + (20/\pi \cdot \Delta C)^2]^{1/2} \ldots\ldots\ldots\ldots [2]$$

New ratios computed by formula [2] from spectrophotometric data are plotted in Fig. 4 as black dots and show a considerable improvement over the values given by the original Nickerson's formula. It is conceivable that a still better agreement may be attained by a further modification of the relative weights assigned to differences in hue, value and chroma, a probability suggested by Nickerson in her papers.

(c) Judd's equation. Another method of computing color differences was proposed by Judd based upon the use of Maxwell's triangle yielding a uniform chromaticity scale.[16] The equation below gives a mathematical function for evaluating color difference ΔE in terms of trilinear coördinates *r, g,* and *b* derived from I.C.I. trichromatic coefficients, and apparent reflectance, A.

$$\Delta E = 600[A^{1/2}\ (\overline{\Delta r^2} + \overline{\Delta g^2} + \overline{\Delta b^2}) + .005\ \overline{\Delta A^2}/A]^{1/2} \ \ldots\ldots\ldots\ldots [3]$$

The magnitude of the multiplying factor of 600 was chosen so that $\Delta E = 1$ represents a surface-color difference of some significance in industrial or commercial practice. It is about five times larger than the least perceptible difference under good observation conditions. The constant 0.005 depends upon the line of demarcation between the two samples. If the width of the dividing area is imperceptible, this constant should be increased by a factor of 20 or it may be reduced by a factor of 10, if the two surface-colors under comparison are separated by an area of some other color. The purpose of this constant is to make the necessary adjustment in the visual perceptibility of differences in brightness or chromaticity which depend upon the area of separation between the two surfaces under comparison. It is well established that differences in brightness become enhanced in close proximity and are greatly reduced when separated, whereas differences in chromaticity are practically independent of the spatial arrangement.

With the aid of equation [3] and the I.C.I. specifications for 5 green colors, ΔE were evaluated and ratios of color differences computed. These are shown by circles in Fig. 5. Another set of measurements was made on a Subtractive Colorimeter and color differences evaluated from Judd's equation reduced to appropriate terms. These are shown by broken lines in Fig. 5.

Still another way in which Judd's equation can be made the basis for computing color differences is to use Hunter's multi-purpose reflectometer to secure the numerical magnitude of three parameters for specifying a desired color. These three parameters are the apparent reflectance through Blue, Amber, and Green filters. In terms of these three apparent reflectances, designated respectively by B, A, and G, the expression for color difference ΔE is given by the equation:

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[16] Judd, A Maxwell triangle yielding uniform chromaticity scales, *J. Opt. Soc. Amer.*, 25, 1935, 24-35.

$$\Delta E = 730[G^{1/2}(\overline{2\Delta\alpha}^2 + \overline{\Delta\beta}^2) + \overline{\Delta G}^2/300G]^{1/2} \ldots\ldots\ldots\ldots [4]$$

where $\alpha = (A - G)/(3B + 2A + 5G)$ and $\beta = (G - B)/(3B + 2A + 5G)$. The green filter in the reflectometer was selected so as to give in combination with the spectral sensitivity of the photoelectric cell a response closely approximating the luminosity function for a normal eye. It follows, therefore, that the numerical value of luminous apparent reflectance, A, used in equation [3] is identical with the apparent reflectance, G, as measured directly on Hunter's reflectometer. The constants in equation [4] were adjusted to give the same numerical magnitude for unit of color difference as in the original Judd's equation [3].

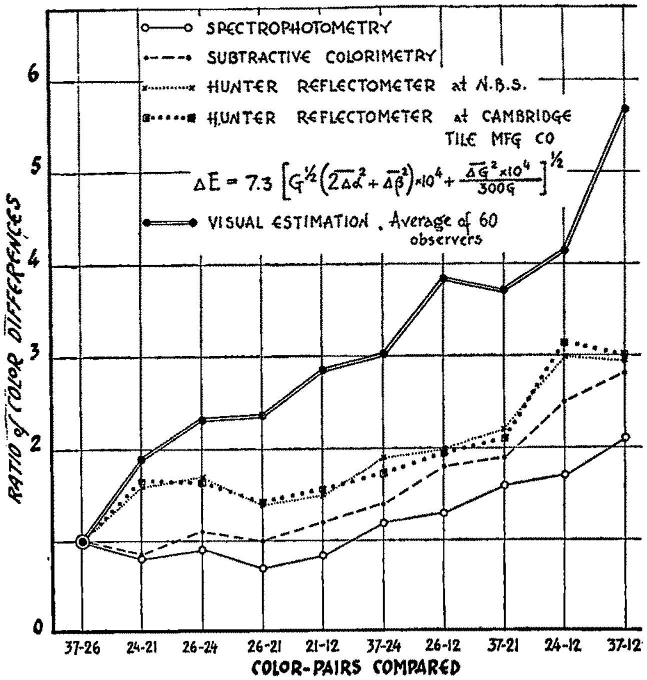

FIG. 5. COMPARISON OF COLOR-DIFFERENCE RATIOS EVALUATED BY JUDD'S EQUATION
(60 *Os*)

For the purpose of comparison, all the values of color differences were reduced to the arbitrarily chosen unit of color differences between titles 37 and 26, which came out to be equal to 5.81 units. A sample calculation of color differences between tiles 37 and 26 is shown in Table II. Following a classical tradition of naming a unit after the man who was primarily responsible for developing the basic principles upon which such measurements can be performed, it is suggested here to designate such unit of color difference by the name "judd."

The ratio of color differences computed from Judd's Equation on the basis of measurements made by the three physical instruments described above are plotted in Fig. 5. The examination of all the graphs will show that the methods of color-difference evaluation proposed by Nickerson and Judd are far from giving a perfect agreement with the average estimations made visually.

The hope for a better agreement lies in the proper adjustment of the formula so as to modify the relative weights assigned to differences in hue, lightness, and saturation. Such work is now in progress. It must be remembered that the individual estimates of color differences may vary by as large a factor as ten and it is very significant that at least the ranking of color differences in visual observations and mathematical computations based on physical measurements follows practically an identical order. From the point of view of agreement, Nickerson's new formula gives the best approximation to visual estimations. From the standpoint of performing physical measurements of color, Hunter's multipurpose reflectometer offers the most rapid, sensitive and sufficiently reproducible means of determining the three color parameters. Under these conditions it was felt that it would be a worthwhile attempt to adopt this instrument in conjunction with Judd's equation to the solution of several practical problems dealing with industrial color tolerances.

TABLE II

Sample Calculation of Color-Differences Between Tiles 37 and 26

Evaluation of color-differences: $\Delta E=7.3\llbracket G^{1/2}(\overline{2\Delta\alpha}^{2}+\overline{\Delta\beta)}^{2}\cdot 10^4+(\overline{\Delta G}^{2}\cdot 10^4)/300G\rrbracket^{1/2}$

	1	2	3	4	5	6	7	8	9	10	11	12	13
		Reflectance											
No.	Blue	Amber	Green	A−G $\times 10^2$	G−B $\times 10^2$	3B +2A+5G	α Col. 4/6	β Col. 5/6	G Average	ΔG $\times 10^2$	ΔL	ΔC	ΔE judds
26	.460	.446	.514	−6.8	5.4	4.842	−1.404	1.115	.561	9.4	5.36	2.24	5.81
37	.535	.537	.608	−7.1	7.3	5.719	−1.241	1.296					

(4) Types of color space. The fact that any color can be completely specified by three parameters suggests a possibility to represent any color as a point in space, whose three coördinates give directly the values of the three color attributes. Colors, therefore, may be considered to correspond to a manifold of points defined by a selected group of three coördinates. Interpreted geometrically, such manifold constitutes a color space.

In a color system established by Munsell, the central vertical axis corresponds to a series of achromatic colors beginning at the bottom with a perfect black and ending at the top with a perfect white. Along the planes perpendicular to the neutral axis are located all the colors of one value (lightness). Colors of different chroma (saturation) are represented in this chromaticity plane by their distance from the axis. Any point on the circle, drawn around the achromatic axis as a center, has the same saturation which increases as the radius of the circle is made larger. Hue is associated with the angular distance from an arbitrary point on the "hue circle." The colors in Munsell space are so arranged that equal linear distances along the vertical or radial axes, or equal angular displacements along the hue circle represent equal steps in sensation *within each color attribute.* This distribution of color variations may be associated with the property of the space itself. Whenever colors are set in order spatially in accordance with visual perception, as the only basis for classification, such an arrangement constitutes what might be called a *psychological* color space. Of this, Munsell's is a characteristic example.

Let us consider two points representing two different colors in this Munsell color space. The linear distance between these two points we may attempt to associate with the magnitude of color difference between them. Such direct evaluation, however, will not agree with visual perception of color differences when compared with another set of two points whose distance of separation may be the same. The reason for this is that one step in hue is not equal in perceptibility to one step in value or chroma. In fact, Nickerson's formula is an attempt to introduce a certain multiplying factor for two of the coördinates so that numerically all three color attributes are placed on the basis of equal perceptibility.

It would be possible also to arrange in space different colors according to their apparent reflectances as determined physically from the spectrophotometric curves. In this system, the word "color" could be used, if at all, only in a very limited sense devoid of all implications in reference to human vision. Instead of a luminosity curve as the basic factor in color perception we could substitute some function characteristic of spectral sensitivity of a certain photoelectric cell. Such a spatial arrangement based entirely upon physical measurements may be referred to as a *physical* color space.

By combining the concepts of human eye sensitivity together with those of physical measurements we arrive at a *psychophysical* color space. The I.C.I. specifications, indeed, allow us to determine a triad of numbers which assign unique position to any given color in such a space. It is valuable in so far as the position of every point represents a definite color based upon physical measurements and expressed in terms of visual perception. It disregards entirely, however, the magnitude of color differences as related to the linear distances.

For the purpose of evaluating color differences we must evolve, what might be called, a *homogeneous* and *isotropic* psychophysical color space. Such a space would satisfy the following requirements. (1) Equal linear distances between points representing any two color-pairs correspond to equal sensations of color differences. (2) A larger linear distance between two points corresponds to a greater perceptible color difference. (3) The ratio of linear distances between two pairs of points agrees numerically with the visual estimation of color differences of these two color-pairs estimated by the ratio method.

Whether such an homogeneous and isotropic color space can be represented in three dimensions, or whether it will have an Euclidean metric cannot be as yet definitely answered. Indeed, there are some like Boring,[17] who propose to extend the number of independent variables from three to five. The possibility of using Euclidean geometry in a color domain is also questioned by Silberstein.[18] There is no question about the desirability of limiting color specification to a space of three dimensions and retaining in it the simple relationships of Euclidean properties.

The investigation on industrial color tolerances described in this paper was, in fact, an attempt to deal with a color space as defined by Judd's equation, in terms of color measurement data of Hunter's reflectometer. The expression for ΔE given in Equation [4] becomes, therefore, a metric in a color space referred to three rectangular axes of α, β, and G. The chromaticity plane is a plane of constant G, where various values of α and β determine the hue and saturation of a color. The coördinates α_1, β_1, will determine the position of a point in the chromaticity plane and G_1 is plotted along a perpendicular erected from the point α_1, β_1. In a chromaticity plane where $1\ \Delta\alpha \times 10^2$ plotted graphically as 100 cm., the proper factors to be used for ΔG so that a linear distance between two terminal points will represent a color difference equivalent to 1-judd are shown in Fig. 6.

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[17] E. G. Boring, Isochromatic contours, this JOURNAL, 49, 1937, 130-134.

[18] L. Silberstein, Investigations on the intrinsic properties of the color domain, *J. Opt. Soc. Amer.*, 28, 1938, 63-85.

The geometrical method of evaluating color differences is applicable to problems where color differences are not large or where reflectances are high as can be seen from Fig. 6. This satisfies the conditions found in most of the problems on industrial color tolerances. The two samples, E_1 and E_2 are measured in Hunter's reflectometer and their reflectances in the spectral transmission regions of blue, amber, and green filters are determined. From these α_1, α_2, β_1, and β_2 are determined and plotted on the chromaticity plane. ΔG is then computed and multiplied by a factor, m, as given in the graph of Fig. 6, corresponding to an average reflectance $G = (G_1 + G_2)/2$. From a point (α_1, β_1) which represents a color with greater G, a perpendicular is erected equal to $m\Delta G$. The distance between the terminal point of the perpendicular to the point (α_2, β_2) in the chromaticity plane is then equal to color difference in judds. The linear distance in cm. equivalent to 1-judd for different average reflectances can be found from the graph in Fig. 6.

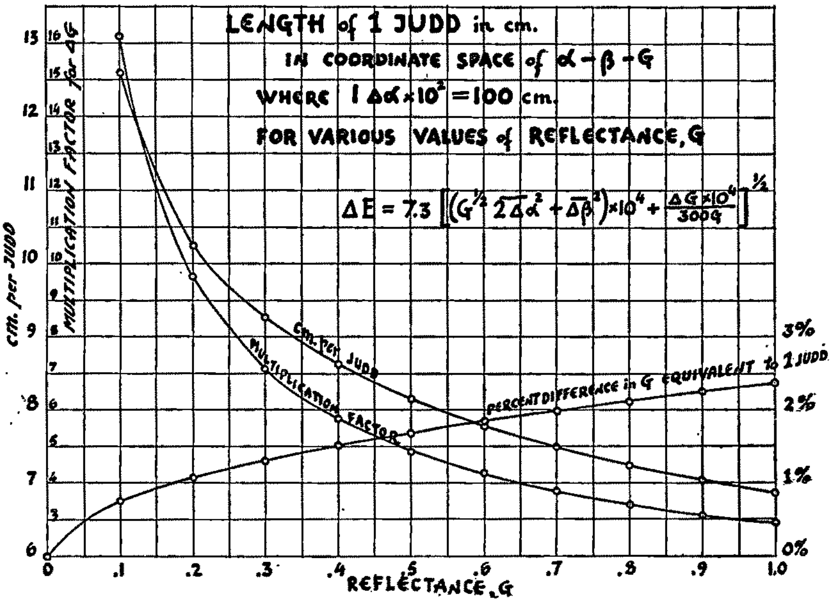

FIG. 6. TRANSFORMATION OF COÖRDINATES FOR NUMERICAL EVALUATION OF COLOR-DIFFERENCES

This geometrical method of evaluating color differences is particularly well adapted to problems where color differences are to be evaluated between pairs of every possible combination of many colored samples. The procedure, then, involves a construction of a color space model in which every color is represented by a terminal point of a steel needle set to a proper height above a chromaticity plane. Multiplication factor, m, is determined by the average reflectance of all colors. A special measuring stick is graduated directly in judds, the length of a judd being determined again by average reflectance. Such a physical model gives at a glance the relative distribution of colors in a limited domain of a color space. The linear distances between terminals of any two colors within this domain are proportional to the sense perception of color differences.

(5) Tolerance boards. From the standpoint of consumer's acceptance of maximum color variation, the most direct method of finding out the proper color tolerance, would be to present for their approval properly selected samples in which the range of maximum color variation would vary within predetermined limits. The consumers are then to be asked what maximum color tolerance they would consider acceptable. Each exhibit is to be prepared in such a form as to simulate the actual conditions under which these color variations are to be seen when the materials or goods are in actual use.

In certain instances the variation in color may be limited to two extremities within a single attribute of color, such for example as the variation in brightness of roasted coffee beans. The samples could be arranged giving a standard shade and different gradations from light to dark. After a careful statistical analysis certain material standard tolerances could be established giving the color tolerances for maximum permissible variations from standard toward a lighter or darker shade. Such material standards of color tolerances are in daily industrial and commercial use. Very frequently they are set more or less arbitrarily without any resort to a direct statistical study of consumer's reactions.

In the field of a glazed ceramic wall tile the variations are usually found in all the three attributes of color. The presentation of three or four tiles would be entirely inadequate and it was therefore decided, in order to carry out this direct method of finding consumer color tolerances, to use at least nine tiles arranged in the form of a square with three tiles on each side. These "tolerance boards" were made of carefully selected tiles which gave a uniform color variation within a maximum color difference which was selected to be: one, two, three, four, and five judds. Whereas the color differences between two adjacent tiles A-B, B-C, C-D, etc., up to H-I were equal, they progressively increased from tile A up to tile I, to wit:

$$A\text{-}B = B\text{-}C = D\text{-}E \ldots\ldots\ldots\ldots\ldots\ldots H\text{-}I$$
$$A\text{-}B = (A\text{-}C)/2 = (A\text{-}D)/3 = \ldots (A\text{-}I)/8$$

If the maximum color variation between two extreme tiles of a tolerance board is, say, 4 judds, the variation from tile to tile is to be only ½ judd, the interval variation being one eighth of the maximum color variation. The properly selected tiles are mounted on Masonite boards in the following relative possitions.

B C D
I A E
H G F

It is possible to arrange these 9 tiles in such order that the appearance of the board from the standpoint of color variability is either enhanced or weakened. The above arrangement was selected because it gave a relatively true representation of color variations as a whole and it was simple in showing an orderly and gradual change in color from tile to tile.

According to specifications described above, the preparation of tolerance boards calls for tiles with uniform interval of color variation so that a group of nine tiles would give a maximum color range of one, two, and up to five judds. Although, at first, the possibility of making such selection did not appear to be practicable, later on, the development of the geometrical method of representing colors and evaluating color differences, made the solution of this problem feasible.

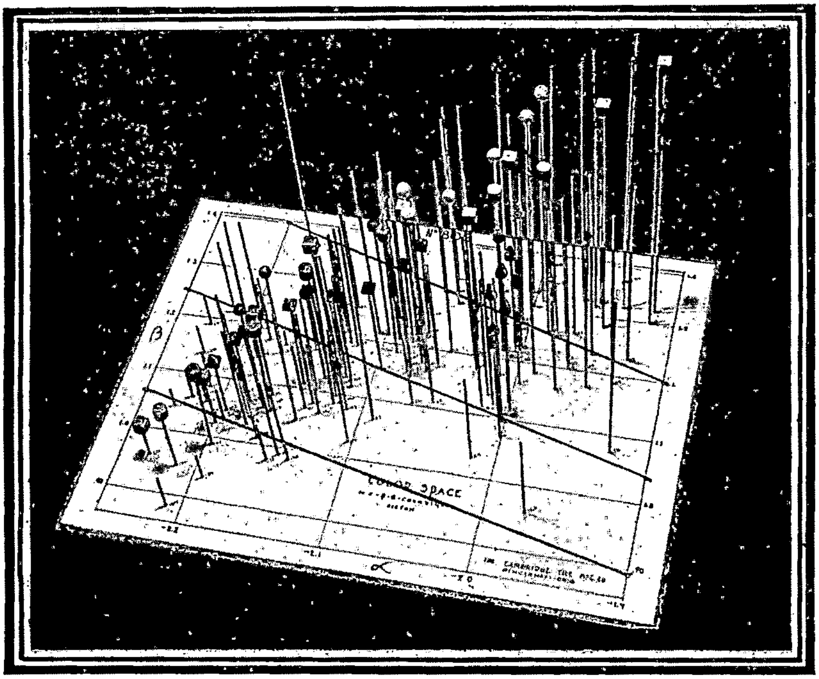

FIG. 7. A MODEL REPRESENTING AN ARRANGEMENT OF COLORS BY POINTS IN A COLOR SPACE OF α-β-G

Each group of nine selected tiles is shown by differently shaped figures attached to the terminal points. The following shapes designate tiles that were selected for different tolerances: cones—1 judd; pyramids—2 judds; balls—3 judds; cylinders—4 judds; and cubes—5 judds.

About 200 tiles of a desired color were picked at random from different production runs and each assigned a number. The color of each tile was measured on the reflectometer and the chromaticity components α and β were computed. These points were then plotted on the α-β plane and designated by their respective numbers. Pins were then inserted into the chromaticity plane to represent the reflectance of every tile. A "yardstick" for measuring color difference in judds was made in accordance with the average reflectance of the whole group of tiles. It was surprising to find that within a group of two hundred tiles there were very few tiles that showed identity in all three color attributes. By visual inspection nine consecutive points, all lying approximately equidistant along a straight line, were selected to give the necessary maximum color variation for each tolerance board. A photograph of an actual model built for selecting tiles is shown in Fig. 7.

The surprising thing about this procedure was the ease with which 45 tiles for five different tolerance boards could be selected. It is not to be expected that an exact equality in the spacing could be achieved. The accuracy of selection was sufficiently high that, when numbered tiles were actually laid out in a row, the color differences from tile to tile appeared to be of the same magnitude. In three or four instances visual observations indicated a discrepancy in the gradation of color differences. These were later on traced to an error in computations or the geometrical construction, determining the location of the terminal point for that particular color. By the use of such color space models, tiles were selected to make 5 tolerance boards for three different colors.

TABLE III

Color Tolerance in Judds
(46 Os)

	Glaze number		
	74	80	82
Most desirable	1.3	2.2	1.9
Max. acceptable	2.4	2.8	2.8

The availability of tolerance boards offers the manufacturer a very specific way of finding out what the consumer wants and will tolerate as far as the color variation in the product is concerned. A set of 5 tolerance boards, numbered at random, is exhibited under proper illumination. A selected group of people is asked to write down the number of a tolerance board which shows either the most desirable or the maximum acceptable color variation. The Color Variation Ballot for three different colors reads as follows:

Granting that variation in color from tile to tile is one of the distinctive features of the product, and assuming that the factory is able to maintain and control the variations as shown on the board.

(1) Which in your opinion represents the *most desirable* color variation?

74	80	82
Dusty Pink	Robins Egg	Cafe au Lait
Board no. ( )	( )	( )

(2) Which board in your opinion represents the *maximum* possible color variation, which would be acceptable to you as a consumer?

Board no. ( )	( )	( )

The results of voting by 46 *Os* are given in Table III. The average tolerances, T_a for different colors were computed by the following formula:

$$T_a = (\Sigma^5_{K=1} \cdot K \cdot V_K)/(\Sigma^5_{K=1} \cdot V_K), \text{ where}$$

V_K is the number of votes cast for a tolerance board with a maximum color difference of K judds.

These preliminary results indicate that there is a very good consistency in correlating visual estimations of color differences with the numerical evaluations of these differences by Judd's equation. There seems to be an indication, however, that color differences in the spectral range of red corresponding to glaze 74 are too small when computed from Judd's equation. Tolerance boards can be used, therefore, also as a check on the homogeneity of different domains within a color space.

It would be also possible to select tolerance boards within any given domain so that colors differ by the same amount in hue, brightness, or saturation. These boards could be intercompared and conclusions drawn whether a variation in one attribute is equivalent to the same numerical variation in another color attribute. This will constitute a test for isotropy of any color domain.

(6) Numerical values of color tolerances. The procedure outlined above allows the manufacturer to obtain valuable information regarding either desirable or maximum permissible color variations. The discrimination between these two types of color variations does not necessarily exist in all commercial products. Whenever such distinction can be made it will lead to a better satisfaction of consumers' wants. It is of interest to record a spread of approximately one judd as voiced by a group of voters in regard to color variations in ceramic wall tiles. This fairly narrow limit of color variability may appear to be more severe than the actual use of the product calls for, yet such was the response of the observers. As pointed out by Judd, "the manufacturer would be only too glad to use larger tolerances, but the purchaser in many cases either does not understand the necessary connection between size of color tolerance and cost per accepted unit, or else he uses the color tolerance as a loophole through which to escape an unfavorable contract when there is no other way to escape it."[19] If the size of color tolerance could be definitely specified and accepted, it would serve well to guard the interests of both the manufacturer and the consumer.

The numerical magnitude of color tolerance must depend upon the specific usage or utility of the goods where color differences serve as indicators of variations in certain desired properties. It is, therefore, futile to expect that a single color tolerance could ever be established. Within a specific industry, or in related fields, definite standards of color tolerances would be most desirable.

It might be well to classify different groups of color use which may affect the magnitude of color tolerances. (1) Color in reproduction of objects as they occur in nature. Color in art, printing or photography. (2) Color as indication of quality, chemical composition or desired properties. Color in agricultural products, colorimetric analysis or pH determinations. (3) Color as a means of identification. Color of signal glasses, trade marks, various color designations as in army or navy. (4) Color standards as used for references in various systems of color classification. Munsell color atlas, Ostwald sytem, Merz and Paul dictionary of color. (5) Color as an element of fashion or in compliance with conventional custom, style or usage. (6) Color as related to other colors to produce pleasing aesthetic effects or harmonious combinations.

Numerical values of color tolerances may be of course different within each group and for each article depending upon the condition of use, manufacture, and price. Although in some industries the general idea of a proper working color tolerance may be fairly well established by the use of material standards, this situation is far from universal. It is reasonable, however, to expect that the size of color tolerances will be generally much smaller in one group than in another. For example, we can hardly expect that a color match in a trade mark requires the same degree of reproducibility as would be desirable to have in material color standards. Speaking in terms of numerical color differences, 3 or 4 judds might be acceptable in the first instance and a fraction of a judd would be desirable in the second.

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[19] Judd, Letter to the editor, *Bull. Amer. Cer. Soc.*, 17, 1938, 379-380.

Sometimes it may be even necessary to establish different sizes of color tolerance for different spectral regions. In color photography the appearance of flesh would certainly require a smaller departure from the actual color, than the color of a dress. In other instances a larger color deviation from a standard in a direction of one hue may not be as objectionable as an equal color deviation toward another hue. As an illustration we may cite the example of butter standards, where a shift in hue from yellow to orange could be tolerated to a greater degree than a shift toward green. These two instances are mentioned to show that every product requires a careful study before color tolerances can be established. In both of these cases there is no reason why a numerical specification of color tolerances could not be set up to take care even of these exceptional problems. This can be done by varying the size of color tolerance for different spectral regions or for different directions in color space.

(7) Control of color uniformity. There are usually several factors which determine the color of a finished product. In mass-production methods these factors depend upon certain physical and chemical tolerances which are set so that the maximum color variation will not exceed a certain limit. It must be obvious, therefore, that a *consistent* color uniformity of manufactured products depends entirely upon the tolerances maintained in these physical and chemical factors. The use of the adjective "consistent" is necessary because in certain instances it is possible to achieve a uniformity in color if it so happens that the variations in any two factors are in such a direction that they neutralize each other.

It follows, therefore, that for production of articles within a certain color variability it is necessary, first, to be able to measure numerically each one of the physical and chemical agents and, second, to determine the effect of variation of these agents upon the final color. If more than one agent is involved it will be necessary to introduce a measured variation into a single agent, maintaining all the others constant. The results of such tests will supply the information in regard to the maximum permissible variation in physical and chemical agents to produce a variation in color not exceeding a certain color tolerance limit. As an approximation we may assume that the maximum color deviation from a standard is given by the sum of maximum color deviations of each one of the independent agents.

In the manufacture of ceramic tiles we will consider the effect of only two factors upon the color of a final product: (1) Thickness, a, of the fired glaze; and (2) temperature, t, at which it was fired. If ΔE represents a color difference between a standard and a sample tile because of a difference in the thickness of the two glazes and temperatures at which they were fired, then:

$$\Delta E = \Delta a\,(\delta E/\delta a)_a + \Delta t\,(\delta E/\delta t)_t, \ldots\ldots\ldots\ldots [5]$$

where, $\delta E/\delta a$ is a partial derivative of color with respect to glaze thickness, $\delta E/\delta t$

is a partial derivative of color with respect to temperature of firing, while Δa and Δt are measured differences in thickness and temperature respectively.

It can be observed from the above equation that for a maximum desirable value of ΔE, the factors under the manufacturing control are determined entirely by Δa and Δt, since δE/δa and δE/δt are the intrinsic properties of the glaze itself. It is only when the numerical values of ΔE, and δE/δa and δE/δt are known, that we can set up a tolerance for Δa and Δt. If, from the standpoint of industrial control, Δa and Δt are too large, then we have to be content with a larger value of ΔE or change the basic properties of the glaze under consideration.

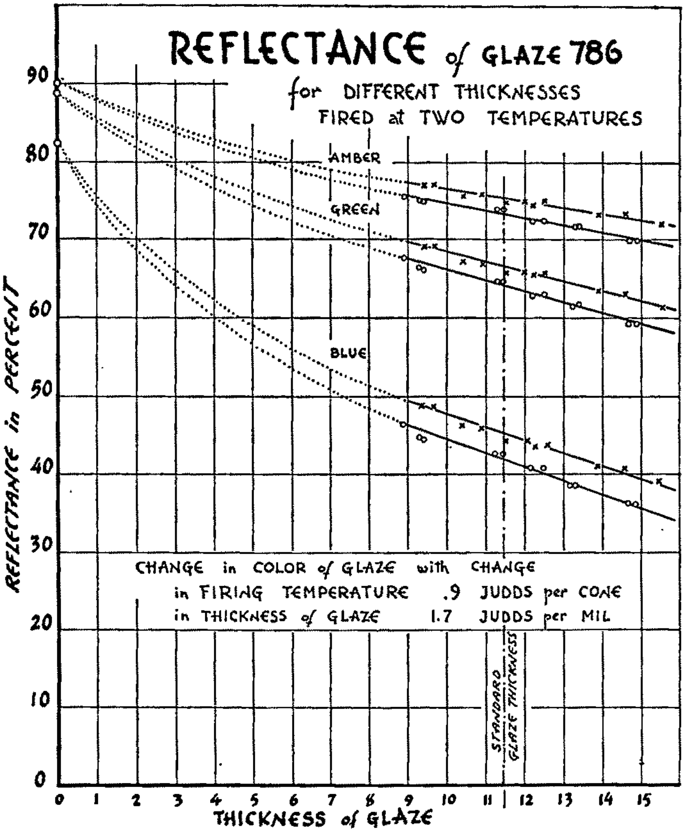

FIG. 8. REFLECTANCES OF TILES OF VARIOUS GLAZE-THICKNESSES FIRED AT TWO DIFFERENT TEMPERATURES AND MEASURED ON HUNTER'S REFLECTOMETER

The use of the Color-Uniformity Control equation [5] provides the industry with specific information upon which to establish a rigorous control of color uniformity.

The determination of rates of color variation as a function of thickness or temperature is made by the application of glaze in gradually increasing thicknesses to two sets of, say, 10 tiles. Each set is fired at two different temperatures. The color of each tile is then carefully measured on Hunter's Reflectometer and the reflectances B, A, and G plotted as shown in Fig. 8. From this graph computations are made to determine $(\delta E/\delta a)_a$, as so many judds per mil of glaze thickness, and $(\delta E/\delta t)_t$, as so many judds per degree Fahrenheit or per cone. With these values at hand tolerance limits for Δa and Δt can be set up so that ΔE would not exceed a definite amount. These values are also used to determine the degree of relative stability of one glaze as compared with another.

A numerical evaluation of rates of color variation can be applied to many industrial problems of color uniformity control. These rates become definite criteria for a general expression of color permanency of any material as affected by various measurable physical or chemical agents.

Conclusions—The results of visual tests indicate that it is possible to establish a numerical scale for visual perception of magnitude in relation to color differences. A considerable amount of additional data will be required to provide the information for various spectral regions. Persons who are definitely planning to enter this field of investigation are asked to communicate with the author. It may be possible to provide them with the necessary samples of ceramic tiles for estimations of color differences by the ratio method.

Although the degree of agreement between visual estimations of color differences and those computed from physical measurements are not satisfactory, their application to problems of consumers' acceptance and tolerance control proved of practical industrial value. The use of Hunter's reflectometer for these measurements gave fairly reproducible results within ± 0.1 judd under most careful operational technique. Further improvements are needed, however, to achieve a better reproducibility of this instrument for routine industrial use under plant operating conditions. The reduction of color measurements to differences in terms of visual perception by the use of Judd's equation is still a very tedious and lengthy process requiring at least ten minutes for a pair of samples. The possibility of constructing a nomograph for such computations is now being investigated.

All in all, this paper should be considered only as a trial step in the field of color engineering. What the industry is ultimately looking forward to is an instrument into which two surfaces could be placed for a direct numerical measurement of color differences. The indicating scales of this instrument are to be calibrated in terms proportional to visual perception of color differences in each of their three attributes and also as an integrated single number. The development of such an instrument would render a most valuable service to industry.[20]

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[20] Acknowledgment is made to the following, for only by their generous coöperation has this paper been possible: Dr. Lyman J. Briggs, Dr. K. S. Gibson, Mr. R. S. Hunter, Dr. Deane B. Judd of the National Bureau of Standards; Miss Dorothy Nickerson, U. S. Department of Agriculture; Dr. R. C. Gowdy, University of Cincinnati; and particularly Mr. Charles H. Burchenal, Cambridge Tile Manufacturing Company.

APPARATUS

VISUAL-TACTUAL ILLUSIONS FROM A CRYSTAL BALL

By WALTER R. MILES, Yale University

One hesitates to say what has and what has not been seen in crystals. There are, however, some rather matter-of-fact illusions to be experienced from holding a small crystal ball in the hand, which so far as the writer knows, have not hitherto been described and would seem to merit notice in our psychological literature. For several years past these visual-tactual illusions have been demonstrated to small classes, and it has been found that the majority of participants profess to get the effects quite readily. Undoubtedly suggestion has been present in some cases, but the illusions satisfy the criterion of persistence with repeated trials and in general tend to grow more conspicuous. They depend primarily on the unique optical properties of the sphere which may be briefly reviewed before describing the illusory experiences themselves.

The crystal ball is a poor lens because it has the maximum of spherical aberration. Parallel beams of light which strike the sphere nearly normally to its surface, that is, in a very small region around a central axis, are focussed fairly sharply, but the rays striking the outer zones are refracted more and cross earlier, forming a caustic surface that gives marked image distortion.[1] For example, if a solid glass sphere 70 mm. in diameter is placed with its center 1 meter from a bright object, such as a reading lamp surrounded by an ordinary parabolic reflector, a fairly sharp image of the circular contour of the reflector will be formed on a screen placed about 17 mm. behind the sphere. To an *O* in line with the optic axis and 50 cm. (arm's length) from the sphere, with no screen interposed, the image on close inspection will appear to be turned wrong-side out. The lamp itself seems to be in front, while the surrounding reflector shows as a plane, or as a convex surface behind it. Furthermore, the image of the reading lamp will at first appear to be at or on the surface of the glass sphere, but if a mark which can serve for visual fixation is placed on the sphere on *O's* side and next the image, the latter will then be seen not on the glass surface but standing in front of the mark at a distance of about 13 mm. A pencil or other small object may be inserted in the space between the fixation-mark and the newly identified position of the image seen in binocular vision. As the image is made to move at right angles to the line of vision it approaches the sphere along a curved or caustic line and meets it near the apparent circumference. Images formed by the crystal and the path of an image moving across the face of the crystal both suffer caustic line distortion. If, however, the crystal sphere is free from all marks that can serve as fixation-points, *O* is not aware of this distortion and has the impression that the image is in spatial register with the surface of the sphere in the same way that a map fits the surface of a globe. The image-field and its movement are accepted as characteristic of the translucent sphere as a self-contained optical instrument. This perceptual misinterpretation of

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[1] See Helmholtz's *Physiological Optics*, 1, 1924, 66.

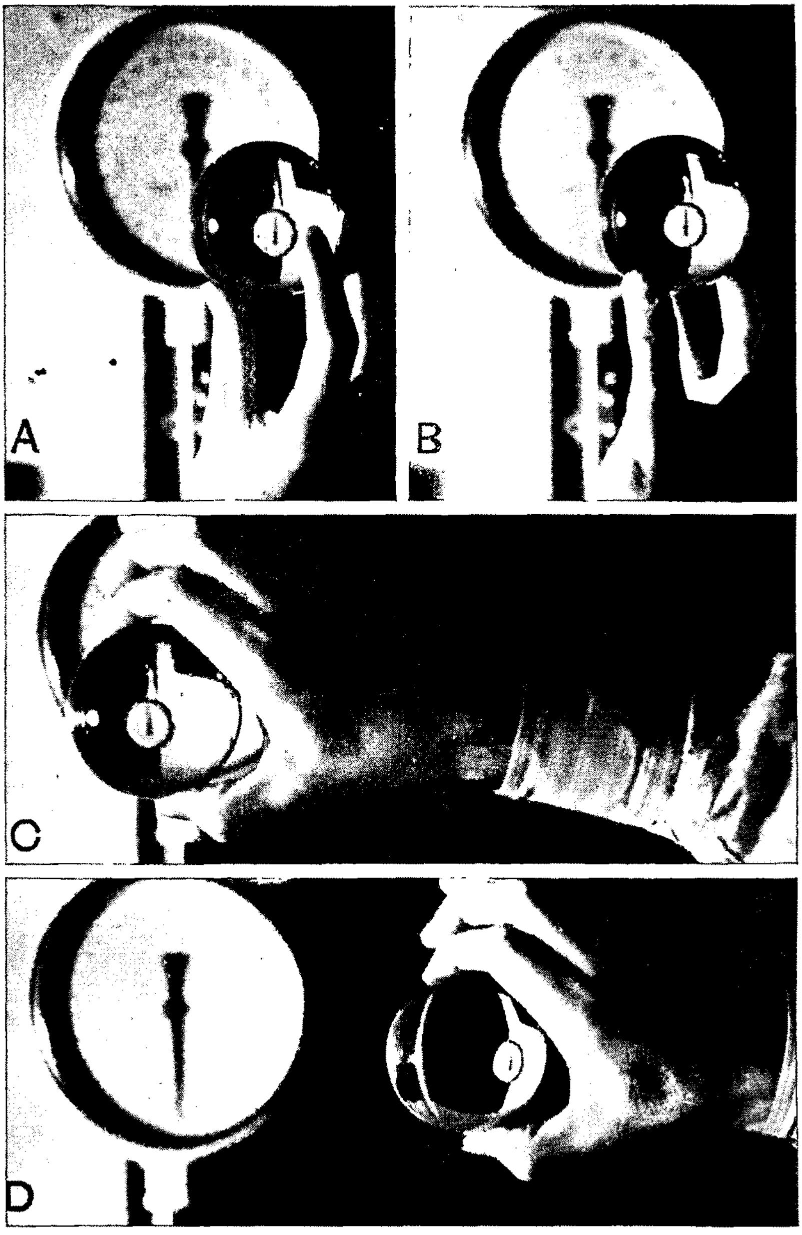

FIG. 1. VISUAL-TACTUAL ILLUSIONS FROM A CRYSTAL BALL

position is in itself a visual illusion that escapes notice and supplies a datum of experience around which other illusions involving vision and touch may occur.

No very special arrangements are requisite for observing visual-tactual illusions in connection with the crystal ball. It is desirable but not necessary to use a complex object field so that the visible convex hemisphere will appear to be well filled with image forms without too much transparent space between them. *O* should grasp the crystal lightly, placing his fingers in such a manner as not to cover the central portion on either side. It is important that the crystal should be spherical without distorting streaks in the glass, and that its surface be clean.

Two forms of the visual-tactual illusion may be distinguished: (1) an illusion of rest, and (2) an illusion of motion. An attempt has been made in Fig. 1 to illustrate both forms. A and B show terminal views for the illusion of immobility of the sphere, while C and D give respectively the beginning and end of an illusion of counter-clockwise turning of the sphere on the axis formed by the thumb and forefinger.

To experience the illusion of rest, *O* supports the crystal on his fingertips and revolves it about its vertical axis without displacing the sphere laterally or vertically. If the viewpoint and object field are both stationary and the ball is not displaced during revolution, the image seen at the surface of the crystal will of course appear unchanged, while *O* alternately executes small right and left rotary movements of his hand. On intently watching the unchanging image, during a few such movements of the hand, one sees the crystal suddenly appear curiously stationary in space, while the fingers are merely gliding around its lower hemisphere. The tactual impressions from the fingertips undergo a temporary sensory eclipse which is in part probably due to adaptation phenomena. The lively sense of contact may disappear and remain absent as long as the sphere "stands still," or a more active tactual impression of a positive nature may come to the foreground of experience. This may be described as a frictionless gliding of the skin areas that are in contact with the glass, as if it were lubricated with an unseen film of oil. The tactual element in the illusion is somewhat transitory and characteristically difficult to analyze. The visual component is very clear and convincing and can usually endure small degrees of displacement in the image without disrupting it. This is especially true when *O* watches the ball while it is being rotated by someone else.

The illusion of rest is fully as striking when the sphere is held with the hand and arm above it and the observer looks obliquely down at the image while rotating the glass. In this position a larger surface of the hand is usually in contact with the ball, but the fingers can be so placed as to avoid covering its visible center. When held upside down it can be rotated well and for a prolonged period without much displacement, and *O*s quickly gain the impression that the hand is gliding over a beautifully smooth stationary sphere such as might decorate the top of a pedestal on a stairway railing. The sphere of course cannot actually be rested on a pedestal during the illusion without producing the visual distraction incident to the magnification of the supporting surface.

The visual-tactual illusion of motion depends on image displacement. If a crystal ball is rolled along the table at the height of *O*'s eyes and at right angles to the line of regard, the regular movement of the image field seen on the surface of the sphere will give the crystal the appearance of spinning about its vertical axis. This visual illusion of motion is equally convincing when the ball, held between the thumb and forefinger, as in illustrations C and D, is moved laterally. When the hand is moved to the right, as from C to D, the crystal appears to revolve counter-clockwise, whereas it presents a clockwise motion when the hand moves from the right back to the left. Alternate movements of the hand provide the requisite conditions for an irresistible visual illusion of alternate partial revolutions of the sphere. Here again the tactual component provided by the stimulus of the crystal against the fingertips must either subside and take an inconspicuous place in experience or accommodate itself to what is visually apparent and dominating. No great stretch of the imagination is necessary to have faint tactual impressions that the crystal is turning, first one way and then the other, between the fingertips. The illusion is interrupted at the terminus of each swing, but quickly reëstablishes itself when motion starts in the opposite direction. The positive tactual component seemed less stable than the visual element in the illusion, but this is probably in part a result of visually dominated attention.

If the crystal is held as in C but is raised and lowered, it gives the appearance of revolving on a horizontal axis while the fingers glide along its circumference. Here the tactual illusion is rather similar to that described in connection with the illusion of rest.[2]

Illusions are interesting because they present a pseudo-pathology of perception and give oblique glimpses into the structure of experience. They are also useful as tests of rigidity in experience since they demand a high degree of adaptability on the part of *O*. Rigid perceptual patterning operates to extinguish the transitory faint cues out of which a good many illusory psychological structures are built. Therefore, mental attitude, affect and experience are important factors in the person to whom one attempts to introduce a sensory illusion. In not a few cases the attempt fails. Some people appear to be frightened by such experiences and resent any trespass of this character on their established reality domains.

The visual-tactual illusions of the crystal ball, like the size-weight and other illusions, demonstrate the strong tendency of vision to dominate perceptual situations. When there is a conflict between visual perception and the impressions mediated by other sensory modalities, vision assumes or wins the privilege of demanding that its impressions be heeded. Figuratively speaking, vision talks the other sensory impressions out of court or into tacit agreement with its convictions. Even Aristotle's illusion is frightened away if vision sticks in a 'finger.'

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[2] Other interesting effects of contradictory movement can be gotten from watching the image of one's feet and the floor in a crystal which is being carried in walking forward or backward or in pivoting, and also from placing the crystal on a support and walking around it. A group of four glass spheres attached together as a pyramid, when moved laterally, provides the conditions for a rather pretty visual illusion in which all the balls turn in the same direction in defiance of mechanical forces rigidly holding them together.

NOTES AND DISCUSSIONS

THE HARVARD *Explorations in Personality*

Explorations in Personality has recently been published from the Harvard Psychological Clinic. The authors of this striking and rather bewildering book have been engaged in a pretentious coöperative research program with three aims: (1) to formulate a dynamic theory of personality; (2) to construct new concepts and experiments for isolating the variables of personality; and (3), as the goal of everything else, to construct psychological portraits or "abstract biographies" of individual subjects. This book is the first report of the undertaking. It provides a rather impressionistic but presumably undistorted picture of the research done in the past five years at the Clinic. Woven into this account is a more extended but somewhat jumbled exposition of the conceptual thinking which was partly the inspiration of the research and partly its outgrowth.

The Harvard Clinic was founded and partially endowed by Morton Prince. Under Dr. Henry A. Murray, its director since 1928, both Prince's medical (clinical) and his psychological affiliations have been perpetuated. Dr. Murray's unusual professional career, leading through the Columbia medical school (M.D. in 1919), the biochemical laboratories of Cambridge University (Ph.D. in 1928), and Jung's institute at Zurich, has now carried him farther into psychology proper than Prince ever got. So far as we can judge from this book his interests at the present time converge squarely on personality studies of the normal individual. But Murray has brought with him to psychology the outlook of a practicing psychoanalyst. This fact makes him unique among those who hold a major position in psychology in a leading American university. It also explains how it comes about that *Explorations in Personality* is by far the most comprehensive attempt to bring Freudian psychology and experimental psychology into line with each other.

The title page of the book bears witness to the unusual spirit of coöperation which has prevailed at the clinic. After the subtitle: "A clinical and experimental study of fifty men of college age," we find authorship of the book credited to "The Workers at the Harvard Psychological Clinic" (in large type) followed by Henry A. Murray (smaller type with capital letters) and by 27 other names (same size type but no capitals), including 2 M.D.'s and 14 Ph.D.'s. Four of the group, we are told (p. 722), had been practicing psychoanalysts. From the separate chapter headings of the eight long chapters in the book it appears that Murray wrote five entirely, shared the authorship of a sixth, and contributed a good-sized introduction to the seventh. The remaining chapter on Procedures, exceeding 200 pages in length, is divided into 25 sections, of which Murray wrote four and must have had much to say about what went into the others.

The bulk of the writing, then, is Murray's. Though the book lacks the unity usually found in the report of an experiment, the consonance of the book's *tone* testifies that one man inspired and to a degree integrated the activities of many coöperating psychologists. Murray has been generous under these circumstances in not publishing more conspicuously under his own name. From his preface, and at frequent intervals later, we glimpse a vast amount of experimental activity going on behind the scenes which never saw fruition in the published report. We can also sniff, all the way along, the odors that arose from a tremendous broth of discussion stewing for years. Murray's own list of those who threw something into the pot reads: "poets, physicists, sociologists, anthropologists, criminologists, physicians; democrats, fascists, communists, anarchists; Jews, Protestants, Agnostics, Atheists; pluralists, monists, solipsists; behaviorists, configurationists, dynamicists, psychoanalysts; Freudians, Jungians, Rankians, Adlerians, Lewinians, and Allportians. To the fact that we never found a language suitable to all (sic), that some of the experimenters entertained reservations to the last, the reader can ascribe some of the annoyance or pleasure he may experience when here and there throughout the book he encounters varieties of terminology or theory" (p. xi).

Our comment is that, far from the result which this quotation might lead one to expect, all the exposition of fundamental theory in this book is written by one man. It is Murray's distillation of the broth. Those who "entertained reservations" are not explicitly heard from. In the book as it is published whatever disagreements existed are neither set forth nor argued out. There is no weighing of alternative views and no cross-fire of discussion between chapters. This, doubtless for good practical reasons, is all that could be expected in the summary of so extensive and so daring an undertaking, one that Murray himself candidly calls "excessively ambitious." More serious, however, is the fact that the experimental program is not brought sufficiently into line with what has been done outside Harvard, a fault which taints all that the clinic did with idiosyncrasy, and worse. The reasons that lie back of this we may easily infer from the temper of a passage which we shall presently quote in full wherein the standard methods adopted for the solution of such problems are dubbed "statistical, trivial and artificial." One clear fact about the clinic group is that it is working in semi-detachment from the main traditions of differential psychology. They have derived little of positive value from this field and have so far contributed little to it. In the 1939 edition of Hildreth's bibliography listing 4000 items in the field of Mental Tests and Rating Scales only two are included which stem from the Harvard Clinic.

Research at the Harvard Clinic has aimed at the development of personology, or the clinical study of persons. Most inclusively personology is the study of the life-histories of individual men and of the factors that shape them. It uses the methods of psychoanalysis, individual psychology, and the experimental psychology of personality. Murray does not like the phrase psychology of personality because it is "clumsy and tautological." "Our work is the natural child of the deep, significant, metaphorical, provocative and questionable speculations of psychoanalysis and the precise, systematic, statistical, trivial, and artificial methods of academic personology" (p. 33). Murray also expresses a more impartial view of personology, the blend of all these, when he acknowledges with modesty, humor, and (perhaps unconscious) punning that "personology is still in diapers enjoying random movements" (p. 6).

The general procedure at the Clinic was to examine a group of subjects in a series of experimental sessions and planned conferences. These subjects, 51 men of college age, were most of them Harvard students chosen by the Harvard Employment Office so that (in the rather bi-modal phrase of the book, p. xi) high and low scholarship, arts and sciences, were equally represented. Four groups were studied, each group more intensively than the one before it, as new methods were added and old ones perfected or discarded. The final group of 15 subjects gave, or rather were paid for, some 36 hours of subject service, which, it is pointed out, is much less than is required in standard psychoanalytic practice.

The unifying purpose behind the procedures was "to organize the findings from all of them into an intelligible portrait of a subject" (p. x). The word portrait deserves stress. Murray's introduction to the one case-history, 87 pages in length, for which space in this volume could be found, asserts that the proof of the personological pudding lies in the case-history and "its worth can be judged positively by its general success in ordering the facts as well as by what it reveals beyond the facts" (p. 606).

Much space would be required to detail the order of procedures in putting a subject through the Clinic. The basic ideas were "to have all experimenters study the same series of individuals with the same concepts actively in mind, and then in assembly—a meeting ('usually lasting five or six hours,' p. 29) being devoted to each case—to report their findings and collaborate in accomplishing a common purpose: the formulation of the personality of every subject" (p. vii). An atmosphere extraordinarily favorable to drawing out confidences and imaginative material prevailed in the Clinic: "we respected our subjects and became fond of them" (p. 13)—and, the reviewer adds, gave them an extremely good time in an atmosphere that must often have seemed to them more like a select social club than a laboratory. There may be a touch of significance in the fact that the subjects are referred to in the report by such extraordinary names as Bulge, Mauve, Oriol, Vulner, and Zora. Certainly the section of the book called "Conversations" which describes the use made of the periods between experiments creates a distinctly bad impression by its preciosity and la-di-da overtones.

At the start of work with every *S*, he was interviewed for 45 min. by the Diagnostic Council in a carefully planned and stereotyped conference. The conference is likened to "the prelude to an opera in that it included parts of themes (tests) which were to be fully presented in subsequent sessions." The purpose of the conference was to provide each of the 5 members of the Diagnostic Council his first opportunity for rating the subject, intuitively, on 40 character traits. A few days later Dr. Murray, as chief experimenter, summarized for the other members of the Council his analysis of the interview. This is in accord with "the principle of weighted judgments" adopted by the Clinic. "In any group of experimenters '. . . it is advisable to establish a diagnostic hierarchy. By weighting the opinions of the more competent, one gets the full benefit of *superior* judgments as well as of *many* judgments" (p. 27).

In what followed the subject wrote an autobiography, his early childhood memories were phonographically recorded, his sexual development was probed by a psychiatrist using free-association methods, he rated himself on three 200-item questionaries covering personality variables, and then as *S* he was put through some twenty different test situations or experiments. At the conclusion of all these the examiners met to exchange their findings on a given *S* and then one of them, aided especially by the judgments of the Diagnostic Council, prepared an elaborate life-history with the abstract biography or "psychograph" at the end.

Outstanding among the clinic's inventions are a series of tests devised to lay bare repressed trends. In one of them, the Thematic Apperception Test, which is described to *S* as a test of his creative imagination, he is asked to make up a story about a picture that is shown him. We are told that all of the *S*s, without exception, responded projectively, thus betraying their "circumstances, experiences or preoccupations." "For instance, in one of the early experiments six of the eleven college men who took the test said that the youth in one picture was a student; whereas none of the twelve non-college men who acted as subjects described him as such" (p. 533). Projection? Well, to a non-Freudian it may seem that the experimenters are the ones who are most surely doing the projecting; they are either projecting projection or renaming association. Can an association psychologist be denounced if he parsimoniously prefers to see in the six boys out of eleven for which the psychoanalytic concept supposedly "clicked" the simple working of "bonds," of frequency and recency, or redintegration? This instance of bias in interpretation is, of course, a trivial one, but it is cited as one example out of hundreds in this book where the validity of psychoanalytic concepts is inferred from the way a handful of straws blows by in the wind.

Like all tests employed at the clinic the Thematic Apperception Test is valued as a help in constructing for each *S* a psychological portrait that is genetic, dynamic, and psychoanalytic. This test in particular "could be most certainly relied upon to supply the necessary clues for the derivation of the unity-thema" (p. 545). The unity-thema—"the most fundamental and characteristic determinant of a personality"—is conceived to be a compound of needs, partly integrated and partly conflicting, traceable to some experience in early childhood. Such a concept is purely Freudian and congruence with it constitutes a new kind of validation for a test.

The unity-thema of one *S*, Earnst, as elaborated in the psychograph (which is the capstone of his long case history), runs like this: A frail infant, strenuously activated by inanition during the oral period helplessly seeks nourishment from his mother's body; but the father, as he comes to believe, robbed him of the nourishment due him by having sexual connections with the mother, an act which he witnessed, according to the present hypothesis, with lasting effects on his character. Deprivation, repeated in varying forms throughout a neglected childhood, leads him to form the habit of aggressive quest for provision; he struggles on, a sort of lone wolf, immersed in fantasies and anxieties, overcoming one handicap after another, until he becomes a graduate student at Harvard and eventually lands a job. If the reader cares, and dares, he may trace the clues, like those in an intricately embroidered detective story which uncover the plot of the biography and its unity-thema. And he will see how the latter when once grasped, "serves deductively in the reinterpretation of these details (*i.e. S*'s memories, fantasies, attitudes, choice of words, etc.) in the light of the unitary pattern they form." Just as the present reviewer, who is a naïve and inexpert reader of detective stories must always go back, once he gets hold of a plot's denouement, to fit in the details which meant nothing to him when he first read them.

Even a skeptic cannot withhold his admiration for the intuitive artistry with which the method is carried through. If only it were as easy for the skeptic as it is for Murray to be sure that "in the wake of intuition comes investigation directed at crucial problems rather than mere unenlightened fact-collecting" (p. 21). Murray does not doubt this. In effect he says, "Except ye first believe ye shall in no wise enter into the Kingdom" of a worthwhile personology. But the stand-offish extraceptive remains a skeptic. It is his observation that what follows oftener in the wake of intuition is more intuition, and in the wake of belief follows theology. To him a theology is exactly what a polished-off Freudian system looks like, and no art can help it to masquerade successfully as the fruit of sober inductive methods. To the skeptic, also, the Freudians seem like those theologians who find it easier to believe their creations in bulk than one item at a time, and so they make big and elaborate systems and thus insure that by sheer piling up they eventually become imposing.

Murray states that he knows of no case histories that are more scientific than the clinic's, and he objects to "the current tendency among American psychologists to become suspicious whenever there appears in the writings or speech of a fellow-scientist the slightest trace of aesthetic feeling. A psychologist who believes that he can tell the truth without being 'literary' has only to try writing a case history or biography, and then compare what he has done to a character sketch by any novelist of the first order" (p. 608 ff.).

Who can doubt it? Certainly not the reviewer, who has attempted to teach undergraduates a course in "Biographical Psychology" and has largely failed because he has strong scruples against a wholesale use of the Freudian ideology for its "resolving power" (Murray) and in order to conceptualize a life history and to make dramatic sense of it (Dollard). Why does he feel scruples? Simply because even today, almost fifty years since Freud had his first inspiration, no psychoanalyst seems to feel called upon to make a real effort to *prove* his stuff or to show that he has ever heard of scientific method as it is understood in the physical sciences and as Mill conceived it when he formulated his logic of the Joint-Method. Murray says that "depth psychology flowered because trait psychology failed to 'explain' phenomena, failed to get at the root of things." The reviewer would add that depth psychology resulted when certain psychiatrists recklessly, or out of desperation, rushed in where experimentally trained psychologists were loath to tread!

Psychoanalysis since its beginning has consisted in great part of unverified and prescientific intuition promoted by a group of persons who lack appreciation of what is required to establish the validity of sweeping propositions. Why do its supporters fail to see that instead of asserting that this or that generalized law of development is true what we need to be told is how much or how often it is true, and under what conditions; and on the other hand when it is not true, and under what conditions these exceptions appear, etc., *gradus ad scientiam?* Even now we are waiting for a few at least of the psychoanalysts to detach themselves from the group of believers who maintain faith by becoming completely absorbed in the business of taking in each other's washing and getting all excited over cleaning up first this detailed spot and then that one, who will instead make it their business to set about accumulating the *evidence* in a form that will carry conviction to non-believers.

Murray, unfortunately, can hardly be credited with doing much of the latter. Although in places he roundly attacks the shortcomings and errors of the Freudians (cf. esp. pp. 722-725), he has chosen for himself no such forbidding task as a frontal attempt to bring psychoanalysis into line with science. But Murray does wish to see some of the psychologist's techniques added to the repertory of the psychoanalyst. He is out to redress the prevailing balance between too broad speculation in psychoanalysis and too detailed measurement in the psychology of personality. He has little interest in tests as such, but he does see that tests can do something, and by using a great variety of tests and questionaries and experiments he brings the manifest measurable traits of personality into the picture as no pure psychoanalyst has done; on the other hand he has directed attention to the latent trends of the person to such a degree that his work is a real marriage, and the first one, between experimental psychology and Freudian or depth psychology.

Different judges will undoubtedly form different opinions of the success of this undertaking. To a sizeable group of psychologists interested in personality it will bring invigorating encouragement. This group will include persons whose hope of attaining a worthwhile psychology of the individual person has been flagging because they have become disillusioned with ordinary mental tests, and who, at the same time, feel no distaste for psychoanalytic theory and wide horizons in general. To another and, we suspect, a larger group of psychologists, including all adherents of rigidly controlled experimental methods and carefully used statistics, it will be a trying book. At the extreme its outlook will seem to them unscientific, its methodology undisciplined, and its immediate effect regressive. Psychoanalysis implemented and eked out with tests will to them still be psychoanalysis, and anathema. Psychology, they are convinced, will develop surely and sanely only if it throws itself into today's definite tasks and does not allow itself to become beguiled by far and enticing vistas.

Not so these authors. With courage and ardor they tolerate and even advocate the "intraceptive" approach to psychological problems. An indispensable glossary of neologisms and redefined terms is added at the end of the book. In it intraception is characterized as "an imaginative, subjective, human outlook." Because they admit the intraceptive attitude into the practice of psychology these authors hold that psychology is too young and too complicated to be as sober and meticulous in gathering data as it has hitherto tried to be. "In its present stage personology seems to call for men who can view things in the broad. . . . A man who has been trained in the exact sciences will find himself somewhat at a loss, if not at a disadvantage. He will find it difficult to fall in with the loose flow of psychologic thought. . . . And so if he continues to hold rigidly to the scientific ideal, to cling to the hope that the results of his researches will approach in accuracy and elegance the formulations of the exact disciplines, he is doomed to failure. He will end his days in the congregation of futile men, of whom the greater number, contractedly withdrawn from critical issues measures trifles with sanctimonious precision. . . . For the present the destiny of personology is best served by giving scope to speculation, perhaps not so much as psychoanalysts allow themselves, but plenty. Hence, in the present volume we have checked self-criticism, ignored various details, winked a little at statistics, and from first to last have never hesitated to offer interpretative hypotheses" (p. 21 f.).

Faced with a credo like this, we can safely surmise that all determined critics of this book will, in contrast, belong to the other camp. They will exemplify Extraception, "a disposition to adhere to the obviously substantial facts. A practical 'down-to-earth' skeptical attitude." Such phrases fit the scientist's habitual description of himself.

Now the men, or more probably man, who coined the terms Intraception and Extraception, and so ingeniously, and in so many connections, develops the contrast between them (pp. 212 ff.), and then says quite simply that in his own psychology he desires to correct for the prevalent anti-intraceptive bias, is smart enough to foresee the gauntlet of criticism which *Explorations in Personality* must run. He is aware in advance what the blows will be like and from what directions they will rain. Yet he has persevered, undismayed and uncowed by the prospect.

The result is a bold and brilliant piece of work. The present reviewer, believing that American psychologists can perfectly well take care of themselves, has felt a strong temptation to set the book up in an entirely favorable light as a first rate creative achievement on the intraceptive side, and altogether a mine of suggestiveness.

In particular, he is impressed by two arduous analytical enterprises that Murray has held himself to. The first is found in Chapter III, one hundred pages long, on the anatomy, or the variables, of personality. Forty-four variables and their sub-aspects are distinguished. Thirty-two variables are classified as needs, either manifest or latent, and the remainder as general traits or inner factors. A sub-analysis of each variable, such for example as the need for dominance, or the need for deference, indicates for each its associated desires, effects, stimulating conditions, actions, conflicts, etc. The elaborateness of the conceptual exercise thus carried through goes far beyond anything previously attempted. McDougall's analysis, in comparison, is a product of horse and buggy days. It is no wonder that Murray has needed to introduce various neologisms, some of which grate on our unaccustomed ears. Samples are Infavoidance, Blamavoidance, Press—which is both singular and plural in form and is used to designate any stimulus or evoking situation.

That Murray's dynamic of personality derives from Lewin as well as from McDougall is apparent in Chapter V, over a hundred pages in length, which supplements the analysis of the person into drives or needs by an equally necessary analysis of the environment. This analysis is undertaken in order to be able "to formulate a single man-environment event," in which "only factors that are operating at the moment can rightfully be included" (p. 284). The "press" which operate in childhood are classified under 20 main heads. And this is followed by the distinguishing of 36 types of behavioral reaction. When they had completed this difficult task Murray and his co-workers were equipped with a grammar and a vocabulary equal to any emergency. They devised a system of notation for it, memorized it, and applied it with the utmost facility in the preparation of case studies. No one will do this kind of thing better.

It will hardly do, however, to limit comment to the more praise worthy aspects of the book. Vast amounts of both system and detail in it are not given a straight-jacket form of organization partly because it may be premature to give such material that kind of treatment, partly because Murray and his colleagues at the outset rejected the idea of doing things in this way, and partly because they couldn't have done it if they had attempted to. In their experimentation they say they "tried out" almost every kind of mental test, yet apparently the criterion for retention of a test in their scheme was that its results fitted in with their intuitions, which are predominantly Freudian, or at least genetic-dynamic. Whether a test had been standardized or not did not particularly interest them. Their own experiments, without apology, are described with insufficient precision to permit other workers to reproduce them, which is the worst charge of all; often one cannot even understand exactly what was done; procedures were altered, if it seemed best, with each change to a new group of *Ss* so that comparative data were rarely secured; small numbers of *Ss* had to be employed (15 in the group studied most intensively) but no question is raised concerning the effect of limited numbers on the significance of the correlations reported; very few distributions of scores and P.E. are reported, and reliabilities though sometimes given are treated very cavalierly; simple Chi-square tests of significance were not computed, though the reader can here and there in a moment or two spent figuring them either verify or dismiss important claims; niceties of rating-scale technique which are expected today as standard practice were overlooked; surprisingly little attempt was made to reach objectivity through use of the "matching method," though the writers claim that in the mathematical treatment of results they relied chiefly upon Allport and Vernon's *Measurement of Expressive Movements* (p. 30); no results of standard intelligence or achievement tests or widely used and standardized personality inventories and questionaries are incorporated in the final portraits of the *S*s (except Allport and Vernon's *Study of Values*); vocational choices are meticulously traced to complexes, here called need-integrates, but no attempt was made to throw a more precise light on interest patterns through Strong's invaluable Vocational Interest Analysis Test; typical of the extraordinary preference in the clinic for its own creations is the fact that, though many tests of creative imagination were used, the widely esteemed and elaborately standardized Rorschach Test was administered to but two-thirds of the final group of 15 *S*s; childhood memories of college-age *S*s were tapped but no attempt was made to obtain corroborative checks on their recollections through interviews with parents or other associates; and so on, and on.

Yet at the end of the book, Murray "ventures the opinion" that his program "will eventually be most useful to the experimentalist, the very man who at first glance may find it least appealing" (p. 705). In the last paragraph of the book he makes a plea that other centers where there are "six or more competent examiners" should adopt the same general procedure. But from sheer lack of resources few, if any, groups could do things on such a grand scale as was possible at the Harvard Clinic, with the aid they received from the Rockefeller Foundation. Furthermore, the peculiar inspiration at Harvard will almost certainly not be transferable elsewhere. So our best hope is that on its own side the Harvard Clinic will henceforth become less of an asylum for "epicures of the unconscious" (p. 430), and will strive to discover why it should acquire more respect for what others have been doing; and from the outside that investigators will get busy and throw some of the techniques insufficiently worked out at the clinic into straightjackets and tame some of the hypotheses so airily formulated into manageability. As examples, see the method of confirming a triadic hypothesis concerning hypnotizability which connects Janet and Freud (pp. 486 ff.); the level of aspiration test, which, as reported, is long on interpretation and very short on empirical test; and sex differences in the dramatic production test, with results reported for only 5 college girls.

Fortunately Murray does not mean to claim that any of the Clinic's methods and achievements will be lastingly important in the form in which we are offered them in this book. "Now we have come to the end of our explorations and must . . . write our findings in the sand, since we have nothing to carve in stone, our work being merely a prelude to more substantial studies" (p. 703). Well said! And better still this remark, " 'very much at sea' will be our critics' comment." Here, and in rather numerous "asides" of a masochistic-defiant nature scattered through the book, we glimpse deference to what most investigators understand as the real meaning of scientific work—a precise methodology collectively devised and pragmatically but impersonally evaluated.

For it is by the objectivity and collectivity of what they do that psychologists will prove to the world that their science too has grown up and has left behind the days when only men who give free rein to empathy and who are startling and impressive in themselves can achieve something significant. I quote C. P. Snow: "For the point about science is not that it has to be done by archangels, but that it is foolproof, in the 'long run,' for ordinary fallible men. . . . The detached intelligence which is part of the legend and admired from outside with an almost superstitious awe happens to be supplied by the organization of the scientific process and not by the people who perform it."

Had this view of scientific work been kept in mind at the Harvard Clinic the present volume would have been quite different from what it is. A much more modest selection of problems would have been worked on with far more dogged thoroughness. A statistician would not have been called in now and then as consultant with his "instruction" and "guidance" acknowledged in a footnote, but his name not included among the 28 authors on the title page. Instead, the statistician from the beginning of the whole project would have been appointed assistant chief of staff and he would have been armed with extensive veto power. His duties would have included an especially imperative one: to provide rigorous statistical training for the graduate students in psychology who were associated with the project. He would have taught them what Pope, by changing two words, can be made to say.

"A little *statistic* is a dangerous thing
Drink deep, or taste not *Personality's* spring,
There shallow draughts intoxicate the brain
And drinking largely sobers us again."

It would also have been recognized at the Clinic that a spark of "intuition" can energize months and years of follow-up devoted to forging and validating the instruments of personality analysis; that a single instrument of this kind built into the continuing coöperative tradition is a really great accomplishment.

Explorations in Personality has not been written with these ideals in mind, yet it does something that it is fascinating to see done and does it more colorfully and daringly and with more energy than anyone would have believed possible. We must say quite plainly, however, that one book like this is enough. We wish that there were in it more signs that the promised sequel will be devoted almost 100% to the development of decent technologies dealing with a very small range of the problems of human personality—material of the sort published most naturally in the journals.

But *Explorations in Personality* as it stands should be required *weekday* reading for the psychoanalyst and all psychoanalytically inclined psychiatrists, social case workers and others who are struggling with similar problems and who will profit from the experience of a group that is exploring new vistas with new methods. And it should also be required *Sunday* reading for the psychologists whose field is personality, for they also will find in it ingenious hints of all kinds, as well as the constant vision of that far-off goal, the fathoming of *a* man. The psychologist is faced with a task too overwhelmingly great to justify him in refusing, either on *a priori* grounds, or on the grounds of his brief past experience, to make use of help that is offered him from any quarter, even by an often fantastic-seeming intraceptist. For if he does, then someday Henry Murray may be able to taunt him, or a Murray disciple in some later day may taunt his imitator, in the words used by Henry IV of France: "Hang yourself, brave Crillon, We fought at Arques and you were not there!"

University of Minnesota RICHARD M. ELLIOTT

NAFE'S VASCULAR THEORY AND THE PREPONDERANCE OF EVIDENCE

In a recent issue of this JOURNAL,[1] Dr. Nafe has replied point-for-point to my criticism of his theory.[2] Instead of a similarly detailed rebuttal, it seems better to re-weigh the evidence pro and con, adding some new points which have developed since the original criticism was written.

According to Nafe, there are no specific thermal receptors. Cold is a kinesthetic pattern from contracting blood vessels; warmth from vasodilation. The chief support for this hypothesis comes from research studies by Nafe and his colleagues. Let us examine these experiments briefly. The fact that thermal sensitivity and blood vessels are both absent from the cornea,[3] is merely argument-by-exclusion—exceptionally weak in this case because the cornea is devoid of so much else besides blood vessels. The apparently coincidental failure of vasoconstriction and cold sensitivity under prolonged stasis is arresting,[4] but it must be remembered that these painful experiments were few in number—too few to establish the exact temporal relation. The plethysmographic studies showing a parallel between vascular activity and thermal experience suggest but do not establish that there is a causal connection.[5] Deaths by drowning and the consumption of ice cream also rise and fall together, but whether either is the cause of the other might reasonably be doubted.

To Nafe's own research may be added Goldscheider's histological study showing the presence of blood vessel clusters under warm and cold spots,[6] although Nafe and I differ as to the meaning of some of Goldscheider's statements and the significance of work performed in 1886 and unconfirmed since. Another good point for the vascular theory is the 'reflex cold' from screeching sounds. It should be noted, however, that Krogh interprets a similar experience from stimulation of the pharynx in terms of a sudden sensitization of specific cold receptors by sympathetic impulses.[7] Nafe would also cite the inadequacy of Von Frey's theory of capsulated thermal receptors (Krause end-bulbs and Ruffini cylinders), which he seems to consider is the only alternative to his vascular hypothesis. Surely this is not the case. Other views are possible. In a recent publication, for example, the suggestion is made that the gradations of thermal sensitivity found in seriatim mapping depend upon density patterns of minute receptors and not upon any sort of relatively gross endings corresponding to warm and cold spots.[8]

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[1] J. P. Nafe, Dr. W. L. Jenkins on the vascular theory of warmth and cold, this JOURNAL, 51, 1938, 763-769.

[2] W. L. Jenkins, A critical examination of Nafe's theory of thermal sensitivity, *ibid.*, 51, 1938, 424-429.

[3] J. P. Nafe and K. S. Wagoner, The sensitivity of the cornea of the eye, *J. Psychol.*, 2, 1936, 433-439; The sensitivity of the cornea to heat and pain derived from high temperatures, this JOURNAL, 49, 1937, 631-635.

[4] Nafe and Wagoner, The dependency of cold upon vascular action: studies with nerve block, this JOURNAL, 49, 1937, 636-644.

[5] Nafe and Wagoner, The effect of thermal stimulation upon dilation and constriction of the blood vessels of the contralateral hand, *J. Psychol.*, 2, 1936, 461-477; The effect of adaptation upon vascular reactions to thermal stimuli, this JOURNAL, 49, 1937, 645-649.

[6] Alfred Goldscheider, Histologische Untersuchungen über die Endigungsweise der Hautsinnesnerven beim Menschen, *Arch. f. Anat. u. Physiol.*, Physiol. Abt., Supl. Bd., 5, 1886, 191-227.

[7] A. Krogh, Sympathetic innervation of the cold spots, brought about in a reflex from the pharynx to the skin in man, *Skand. Arch. Physiol.*, 71, 1934, 1-6.

The supporting evidence for Nafe's theory may now be summarized under two headings: (1) Physiologically, thermal experience parallels vascular activity, positively (Nafe's plethysmographic studies, 'reflex cold') and negatively (Nafe's stasis experiments). (2) Anatomically, thermal sensitivity coincides with the presence of blood vessels, positively (Goldscheider) and negatively (Nafe's corneal studies). In the absence of contrary data, these points would constitute strongly presumptive, though scarcely conclusive, evidence for the vascular theory.

Once we leave this narrow realm, however, doubts begin to emerge. Nafe's theory is essentially one of great simplicity. The general trend of research in thermal sensitivity is in the direction of continually increasing complexity. Let us consider two examples:

(1) Adaptation used to be a very simple affair, explained by Weber as the attainment of physical temperature equilibrium and by Hering as a shift in the physiological zero. Recent work, however, shows that it is far from simple. With large stimulus areas, adaptation is typically slow and incomplete. With very small stimulators, on the other hand, complete adaptation can occur in a few seconds,[9] even with widely spaced stimulations.[10] Rapid restoration of cold sensitivity can be effected by application of a high temperature.[11] Slowly lowering the temperature of the skin surrounding a small area causes the area to lose sensitivity to cold quite suddenly when a certain mild critical temperature is reached; whereas slowly increasing the surrounding temperature to a mild degree sometimes heightens, sometimes depresses sensitivity.[12] Not only the area, but also the form of the stimulator may be important. In some cases, the time for complete adaptation is proportional to the perimeter, in others to the square root of the area, and mixed effects can also be found.[13]

(2) The distribution of sensitivity to warm and cold was formerly a very simple matter. There were so many warm spots and so many cold spots per unit area—supposedly corresponding to individual receptors. Seriatim mapping, however, discloses many gradations of sensitivity—hills and valleys, as it were, instead of isolated warm and cold spots.[14] This sort of distribution is found even when the mapping is

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[8] Jenkins, Studies in thermal sensitivity: 9. The reliability of seriatim cold-mapping with untrained subjects, *J. Exper. Psychol.*, 24, 1939, 278-293.

[9] S. Aronoff and K. M. Dallenbach, Adaptation of warm spots under continuous and intermittent stimulation, this JOURNAL, 48, 1936, 485-490; H. A. Levine and K. M. Dallenbach, Adaptation of cold spots under continuous and intermittent stimulation, *ibid.*, 48, 1936, 490-497.

[10] Jenkins, Adaptation in isolated cold spots, this JOURNAL, 1937, 49, 1-21.

[11] *Ibid.*

[12] *Ibid.*

[13] Jenkins, Studies in thermal sensitivity: 2. Adaptation with a series of small rectangular stimulators, *J. Exper. Psychol.*, 22, 1938, 84-89.

[14] Jenkins, Studies in thermal sensitivity: 9. The reliability of seriatim cold-mapping with untrained subjects, *J. Exper. Psychol.*, 24, 1939, 278-293.

done with a 1-mm.-diam. stimulator. Alternate mapping with stimulators of different sizes likewise shows complex relationships in the matter of summation effects.[15]

To explain these complicated phenomena in terms of simple vasoconstriction and dilation, taxes the imagination. A more logical inference is that the underlying processes are also complex. The vascular theory is not definitely disproved by such facts; it is simply rendered far less credible.

Let us take another broad viewpoint. Nafe's theory postulates a single anatomical mechanism (blood vessels) for the mediation of warmth and cold. Most research points to a dual mechanism. The simple fact that some parts of the skin show only cold sensitivity, others only warm sensitivity, makes it probable that there are separate receptors for the two modalities. Abnormal stimulation (paradoxical warm and cold; excitation by electrical, mechanical and chemical means) also implies separate receptors which are specific in the same manner as those of vision, audition, etc. That is, cold receptors which always give cold, warm receptors which always give warm—no matter how they happen to be stimulated.[16] This is the natural outcome of a dual system, but with the single anatomical unit of the vascular theory, some difficulty is encountered. Either this apparent specificity must be proved fallacious, or else blood vessels must mimic the action of specific receptors. That is, some of them must always constrict and others always relax, regardless of the kind of stimulation. Nafe cites evidence to show that reversals of normal vascular action do sometimes occur (dilation when we should expect constriction and vice versa), but this is scarcely proof that blood vessels act consistently in the specific manner called for.

Also hard to explain in vascular terms is Lanier's finding that warm sensitivity is regenerated much more slowly than cold, following nerve block by alcohol injection.[17] Such dissociation seems incredible with an anatomically-single system. In terms of Nafe's hypothesis, it would have to mean that the blood vessels in the affected area had recovered the ability to constrict, but not to relax—or else that the impulses from constriction are carried by different nerve fibers from those of dilation. Neither view is particularly plausible.

Finally, the factor of *time* must be briefly considered. We may agree with Nafe that the matter of reaction time should be discarded, because of uncertainty as to some of the most critical components.[18] There remains, however, the problem of the brief critical flicker frequency for intermittent warm stimulations. Geblewicz[19] has recently confirmed Kastorf's figures by more exact methods. Successive warm stimulations, he shows, separated by only 0.15 sec. can be sensed as discrete. If intermittent radiant stimulation caused *alternating* warmth and cold experiences, what support this would provide for Nafe's hypothesis! Unfortunately, such is not the case. Only flickering *warmth* is experienced. To interpret this rationally in vascular terms seems almost impossible. Not only must a cycle of relaxation and constriction occur within 0.15 sec. (unless we assume that blood vessels can keep on relaxing without intervening constrictions)—but the *contraction phase must occur too slowly to be sensed as cold!*

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[15] Unpublished studies by the author.

[16] Nafe uses the term 'specific' to mean that a receptor can *receive* only one kind of stimulation. Thus he argues that paradoxical experiences, electrical stimulation, etc., show that thermal receptors are *not* specific. This misundertanding is unfortunate. No one would undertake to maintain that thermal receptors are specific in Nafe's sense. The whole point is that they can be stimulated in a number of ways—but always with the same end result. Actually, these abnormal forms of stimulation are the best evidence that the experience depends upon the receptor stimulated and not upon the nature of the stimulus.

[17] L. H. Lanier, An experimental study of cutaneous innervation, *Proc. Ass. Res. Nerv. & Ment. Dis.*, 15, 1935, 437-456.

[18] Reaction-time still remains a potentially lethal enemy for the vascular theory. Obviously, if there is not time enough for the necessary events to occur, the theory must be false, no matter how plausible the arguments in its favor. Nafe's own computed figure of 250 ms. for the minimum in terms of vascular action is still too large to check with some of the reaction times to cold reported in the literature.

If we now step back and view the evidence as a whole, the overwhelming weight on the negative side becomes apparent. Nafe may justly maintain that no one of these facts absolutely proves his theory to be wrong. That is true. It is not the individual difficulties but the everlasting succession of them that is damning. To justify its acceptance, a theory should have the majority of experimental findings in its favor. In this case, the *preponderance* of evidence is strongly against the vascular hypothesis.

Lehigh University WILLIAM LEROY JENKINS

THRESHOLDS OF VISUAL INTENSITY-DISCRIMINATION IN PHYLOGENY

A recent paper by Ghiselli established the point that the principle of encephalization of function does not appear to hold in phylogeny regarding intensity discrimination in vision.[1] It was pointed out that in the mammalian series the cortical centers for brightness discrimination were of as great functional importance in lower (rat[2]) as in higher (cat,[3] dog,[4] monkey[5]) forms. In all mammals, except man, the subcortical visual centers seem able to mediate the brightness habit to some extent in the absence of the striate area of the cortex. Since there seems to be little change in the functional importance of the retinal projection centers in the mammalian series, although anatomically there is a gradual telencephalization of structure,[6] it is of interest to examine the behavioral data regarding the discriminative capacity of different vertebrate organisms in an effort to determine whether this structural modification enables the animal to differentiate brightnesses more keenly.

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[19] E. Geblewicz, L'influence de l'intensité sur las persistance apparente des excitations thermiques pour deux stimulations successives, *C.r. Soc. Biol.*, 127, 1938, 173-175.

[1] Edwin Ghiselli, Encephalization of brightness discrimination in mammals, *Science*, 86, 1937, 618-619.

[2] K. S. Lashley, The mechanism of vision: II. The influence of cerebral lesions upon the threshold of discrimination of brightness, *Ped. Sem.*, 37, 1930, 461-480.

[3] K. U. Smith, Visual discrimination in the cat: V. The postoperative effects of removal of the striate areas upon intensity discrimination, *ibid.*, 51, 1937, 329-370.

[4] D. G. Marquis, Effects of removal of the visual cortex in mammals, with observations on the retention of light discrimination in dogs, *Proc. Ass. Res. Ner. Men. Dis.*, 13, 1934, 558-592.

[5] Heinrich Klüver, An analysis of the effects of removal of the occipital lobes in monkeys, *J. Psychol.*, 2, 1936, 49-61; D. G. Marquis, Phylogenetic interpretation of the functions of the visual cortex, *Arch. Neurol. Psychiat.*, 33, 1935, 807-815.

[6] Marquis, *op. cit.;* C. U. Ariëns Kappers, G. C. Huber, and E. C. Crosby, *The Comparative Anatomy of the Nervous System of Vertebrates*, 2, 1936, 1061-1205; J. F. Fulton, *Physiology of the Nervous System*, 1938, 347-365.

Differential brightness limens have been determined for a number of different organisms using the Yerkes-Watson apparatus,[7] or some modification of this experimental arrangement. This is fortunate for the present discussion because the visual intensity thresholds may be directly compared. In the accompanying table all thresholds reported for the infra-human forms, except Frolov's for the dog,[8] were obtained with roughly comparable experimental set-ups. The difference threshold has been expressed in a number of different ways in the literature, and the writer has thought it expedient to convert all thresholds to a common expression, namely, the Weber ratio. An asterisk (*) in the $\Delta I/I$ column indicates that the writer has calculated $\Delta I/I$ from the data given by the investigator. The last column of the table is the brightness level at which the ratio was found. In those cases where the threshold was found at a number of different intensity levels (Crawford,[9] Mead,[10] Moody,[11] Munn-Slater[12]) the intensity at the minimum limen is given. In the experiments by Lashley[13] and Ghiselli,[14] wherein a large number of animals were tested, the minimum threshold found is presented.

Beginning with the mammals, and disregarding the results of Yerkes' experiment with the dancing mouse in which a somewhat easy criterion of discrimination was used,[15] it will be seen that there is a gradual decrease in the threshold value as the phyletic scale is ascended. Among mammals the ability to discriminate brightness differences increases according to the phylogenetic position of the animal in the series. Among the studies included, the mouse and the rat have the poorest vision. These organisms are exceeded in capacity by the raccoon, cat, and dog, whereas the discriminative ability of the monkey lies between this group and that of the human individual, the latter exceeding by far all other animals. The extremely low threshold reported by Frolov on the dog was obtained by a conditioning method and with reflected light.[16] The other infra-human experiments in the table made use of a discrimination method and transmitted light. The superiority of the human is undoubtedly partly a function of the bipartite test field with which the thresholds were determined, these conditions presenting an optimal state for brightness discrimination.[17] In those cases where the human threshold was measured under the same conditions as that of the animals, although the human subject remained superior, the superiority was not as marked.

We may conclude, from an examination of Table I, that there seems to be an increase in the capacity for brightness discrimination from lower to higher mammalian forms. Thus, although the extirpation studies indicate that there is no gradual shift in function from subcortical to cortical visual centers for this visual capacity in mammals, the anatomical encephalization of structure is accompanied by increased brightness discrimination capacity. The writer suggests that it may be the changes in the retina rather than the neurological modifications which determine this increased visual capacity in phylogeny.

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[7] R. M. Yerkes and J. B. Watson, Methods of studying vision in animals, *Behav. Monog.*, 1, 1911, No. 2, pp. 90.

[8] G. P. Frolov, Reaction of the central nervous system to changes in intensity of illumination, *Proc. Petrograd Soc. Nat. Sci.*, 69, 1918, 3-19.

[9] M. P. Crawford, Brightness discrimination in the Rhesus monkey, *Genet. Psychol. Monog.*, 17, 1935, 72-162.

[10] L. C. Mead, The curve of visual intensity discrimination in the cat, *Psychol. Bull.*, 34, 1937, 517; and other results as yet unpublished.

[11] P. A. Moody, Brightness discrimination in the deer-mouse—Peromyscus maniculatus gracilis, *J. Exper. Zoöl.*, 52, 1929, 367-405.

[12] N. L. Munn and J. E. Slater, A note on brightness vision in the white rat, *J. Comp. Psychol.*, 13, 1932, 273-277.

[13] Lashley, *op. cit.*

[14] E. E. Ghiselli, The superior colliculus in vision, *J. Comp. Neurol.*, 67, 1937, 451-467.

[15] R. M. Yerkes, *The Dancing Mouse,* 1907, 91-132.

[16] Frolov, *op. cit.*

[17] J. R. Smith, Spatial and binocular effects in human intensity discrimination, *J. Gen. Psychol.*, 14, 1936, 318-245.

TABLE I

Comparative Summary of Brightness Discrimination Thresholds

Subject	Investigator†	Minimum $\Delta I/I$	Intensity
Sunfish	Reeves	3.0*	?
Sparrow	Tugman	.18*	.098 cp.
Mouse	Yerkes	.11*	4.1, 16.6, 66.4 cp.
Deer-mouse	Moody	.83*	36.11 cp.
Rat (hooded)	Lashley	1.0*	?
Rat (white)	Munn-Slater	.59*	3.2 ml.
Rat (white)	Slater	.47*	?
Rat (white)	Spencer	.43*	?
Rat (both)	Ghiselli	1.00*	?
Raccoon	Munn	.27*	13.45 ml.
Cat	Smith	.23*	.49, 50.7 ml.
Cat	Mead	.19	.126, 26.1 ml.
Dog	Frolov	.02	?
Dog	Stone	.16*	1 cp.
Dog	Marquis	.20*	?
Monkey	Crawford	.098	.77 ml.
Human	König-Brodhun	.02	10–100 ml.
Human	Steinhardt	.01	1000 ml.
Human	Craik	.01	4000 equiv. fc.

† The references from which the data for this table were obtained are: C. D. Reeves, Discrimination of light of different wave lengths by fish, *Behav. Monog.*, 4, 1919, 1–106; E. F. Tugman, Light discrimination in the English sparrow, *J. Anim. Behav.*, 4, 1914, 55–109; Yerkes, *op. cit.*; Moody, *op. cit.*; Lashley, *op. cit.*; Munn and Slater, *op. cit.*; J. E. Slater, Brightness vision in the albino rat, *Univ. Pitt. Bull.*, 7, 1931, 394–395; L. T. Spencer, Central inhibition in the albino rat, *J. comp. Psychol.*, 3, 1923, 389–408; Ghiselli, *op. cit.* (footnote 14 above); N. L. Munn, Pattern and brightness discrimination in raccoons, *Ped. Sem.*, 37, 1930, 3–34; K. U. Smith, *op. cit.*; Mead, *op. cit.*; Frolov, *op. cit.*; C. P. Stone, Notes on light discrimination in the dog, *J. Comp. Psychol.*, 1, 1921, 413–431; Marquis, *op. cit.*, (footnote 4 above); Crawford, *op. cit.*; A. König und E. Brodhun, Experimentelle Untersuchungen über die psychophysische Fundamentalformel in Bezug auf den Gescihtsinn, *Zitzber. d. Akad. d. Wiss.*, 1889, 641; J. Steinhardt, Intensity discrimination in the human eye: I. The relation of ΔI–I to intensity, *J. Gen. Physiol.*, 20, 1936, 185–209; K. J. W. Craik, The effect of adaptation on differential brightness discrimination, *J. Physiol.*, 92, 1938, 406–421.

University of Rochester LEONARD C. MEAD

A DEFINITION OF BOREDOM: A REPLY TO MR. BERMAN

In the April issue of this JOURNAL,[1] Berman distorted a definition of boredom which I had suggested,[2] and proceeded to challenge my views within the framework of this distortion. I defined *boredom:*

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[1] A. Berman, The effect of benzedrine sulfate (amphetamine sulfate) on satiation, this JOURNAL, 52, 1939, 297-299.

[2] J. E. Barmack, Boredom and other factors in the physiology of mental effort, *Arch. Psychol.,* 31, 1937, (no. 218), 1-83; The effect of benzedrine sulfate (benzyl methyl carbinamine) upon the report of boredom and other factors, *J. Psychol.,* 5, 1938, 125-132.

"as a state of conflict between the tendency to continue and the tendency to get away from a situation which has become unpleasant principally because of inadequate motivation resulting in inadequate physiological adjustments to it. Accordingly, a state of boredom is initiated by inadequate motivation during the operation of a task set and results in a tendency for the physiology of the subject to revert back to the sleep level. The inadequate vital adjustments to the task are unpleasantly appreciated as the feeling of monotony or fatigue. If the task set is weak, the subject may go off to sleep or abandon the task. If the task set is sufficiently strong, the subject struggles to remain awake or partly escape from the depressing task. These latter objectives are achieved usually unconsciously, by shifting attention away from the task, daydreaming, creating extrinsic goals, modifying the procedure, etc."[3]

This view of boredom is elaborated in an early publication.[4] Berman, without explicitly stating that he is using his own terminology, substitutes the word *satiation*[5] for *boredom* in his article which opens as follows:

"In a recent paper *satiation* was defined by Barmack as 'a state of conflict between the tendency to continue and the tendency to get away from a situation.' "[6] (Italics mine.)

There are at least two objections to this type of substitution. First, although the substitution may be quite harmless in one context, it can be unfortunate in another. For example, it is embarrassing to find attributed to me the view which Berman's following statements suggest.

"This is certainly true in certain forms of satiation which we consider primitive; where the satisfaction of a basic need is involved, and is well illustrated by the work of Katz on hunger and appetite. Barmack adopts just this point, *but errs when he generalizes this restricted view to all satiation*."[7] (Italics mine.)

Secondly, the substitution of the word 'satiation' for 'boredom' is contra-indicated by the procedure of the experiments to which he refers. In belated correspondence, Berman attempted to justify the substitution on the grounds that Lewin and Karsten use the term, 'psychic satiation,' in describing similar experiments.[8] A basic difference in procedure between the experiments of Lewin and Karsten and my own is that in the former the subject is relatively free to stop an activity against which he has developed a negative valence,[9] and in the latter the subject is acting under the constraint of the experimenter's instructions to complete a specified period of activity. In the Lewin and Karsten experiments, the rejection of the activity may be considered a manifestation of satiation. In my own studies and other studies in which boredom has been the focal point of interest, the initial activity may be described conceivably as satiating, but the subsequent activity may be more effectively described as a not too happy *toleration* of an unpleasant task. In this situation, *S* conforms outwardly to the instructions of the *E*, but expresses a negative valence to parts of, or to the whole of the task by a diminished or variable rate of work, daydreaming, alterations in procedure, etc. 'Psychic satiation,' therefore, may be regarded as but one aspect of the state of boredom.

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[3] *Ibid.*, 125 f.

[4] *Op. cit., Arch. Psychol.*, 67-74.

[5] I do not use the word "satiation" in either of the publications to which Berman refers.

[6] *Op. cit.*, 297.

[7] *Ibid.*

[8] K. Lewin and A. Karsten, Untersuchungen zur Handlungs- und Affektpsychologie: V. Psychische Sättigung, *Psychol. Forsch.*, 10, 1928, 142-154.

[9] Lewin describes the technique as follows: "the subject must do a certain task repeatedly; he is, however, free to stop as soon as he has enough of it." *A Dynamic Theory of Personality*, 1935, 254.

Again, the substitution of the word (and concept of) 'satiation' has led Berman into what may be regarded as one of the more serious misstatements of my views. He writes:

> "When it is observed that an individual is highly motivated, then with continued activity becomes disinterested, and finally rejects the activity entirely, it is plausible to consider that the motivation has somehow disappeared; that in some way the tissue needs, states of agitation, or other conditions which are the basis of motivation (drive) have become appeased, and then saturated to the point of aversion. . . . Barmack adopts just this point, but errs when he generalizes this restricted view to all satiation."[10]

In the quotation from my Benzedrine study appearing in this article, and to a more detailed extent in my other study,[11] it is clear that I refer to the physiological *consequences* of certain motives rather than to their antecedents. These *results* of motivation favor an alert attitude during the work. According to our hypothesis, then, after the exploratory drive is gratified (of which the relative novelty of the task may be considered the incentive), and under certain other conditions such as the lack of the operation of other strong motives, and with the restriction of attention to the task,[12] a condition develops which is unfavorable to the maintenance of the alert state. Out of this tendency stem two main effects, a desire to get away from the task or other environmental condition which, in a sense, had produced it, or a desire (usually unconscious) to correct the unpleasant state by introducing new methods of work, thinking of more strongly affective situations, which are, in a way, means of staying alert, etc. Through conditioning, the range of stimuli to which the rejection may appear is extended.

On the basis of the foregoing analysis, it follows that certain antihypnotics should allay or minimize the appearance of these two effects.[13] In fact, the antihypnotic, Benzedrine, was found effective in retarding (a) a drop in the rate of addition over a two-hour period, (b) the development of boredome, strain, irritation, fatigue, sleepiness and inattentiveness, as indicated by ratings on ten-point subjective rating scales.[14] Subsequently, we obtained evidence that 60 mg. of ephedrine hydrochloride and 2 gr. of caffeine (alkaloid form)[15] have similar properties, although, under the conditions of the experiments, to a lesser degree than the 10 mg. of Benzedrine.

Berman reveals still another confusion from his substitution of the word 'satiation' for 'boredom' as is apparent in the following:

> "As is well known, the requisite for sleep, or a sleep-like state, is the exclusion of sensory stimulation. An abundance of stimuli in the field, as would be required to satiate an individual, would tend to reduce the tendency to go to sleep."[16]

It is not true that the requisite for sleep is the exclusion of sensory stimulation. So far as I know, no study has been performed in which the relative importance of motivation or other internal conditions has been compared to that of the exclusion of sensory stimuli, and to describe the conditions which facilitate the development of boredom as "an abundance of stimuli in the field" is not altogether warranted.

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[10] *Op. cit.*, 297. [11] *Op. cit.*, *Arch. Psychol.*, 67-74. [12] *Ibid.*, 71.

[13] Those in which there are no other intrinsic factors which might induce a negative valence to the task.

[14] *Op. cit.*, *J. Psychol.*, 5, 1938, 125-133.

[15] Unpublished material. [16] *Op. cit.*, 298.

Towards the end of his paper Berman states:

> "This note is in part directed to just this recognition of the confusion between the motivational and physiological aspects of satiation on the one hand, and the psychical on the other. It does not posit a strict dualism between motivation and satiation (for this is obviously incorrect), but attempts to indicate *that all aspects of satiation cannot be explained on the grounds of basic motivational states, and tendencies to revert to a sleep level.*"[17] (Italics mine).

Berman, I was informed by correspondence, bases the italicized statement on the following quotation appearing at the end of my Benzedrine study. "A state of boredom is caused principally by a physiological reversion to the sleep level due to inadequate motivation during the operation of a task set."[18]

Berman criticized the use of the subjective rating scales. "We doubt," he writes, "whether any *S* has the ability to discriminate accurately over a 10-point scale. It was found in one of our experiments that responses to a 5-point scale were not sufficiently reliable to be the sole criterion of satiation."[19]

I have not used the subjective rating data as the *sole* criterion of boredom, but have used them in conjunction with changes in rate of addition. In the light of correspondence with Berman, it appears that his statement on the 'unreliability' of the 5-point rating scale which he used was based on an observed inconsistency between certain verbal reports of some individual *S*s and their ratings. That the ratings of some individuals may not be too valid an expression of their subjective states is common knowledge. In the Benzedrine study, I dealt with *average ratings* of *many* *S*s rather than with a consideration of individual cases. It is true that the critical ratios of differences between the means of the subjective ratings for Benzedrine and placebo at specific periods are only of the order 0.95 to 2.5. The duplication, however, of these values for a series of four sets of differences during the last hour of the 2-hr. work-period for each of the dependent variables gives most of them added statistical significance. Certainly the lowest qualitative evaluation that may describe such data is that they cannot be ignored. Further, essentially the same findings were obtained in another experiment using 15 mg. of Benzedrine on 10 *S*s engaged in an entirely different task, operating the Poffenberger pursuitmeter.[20]

In a better founded criticism, Berman inquires why the data of the Benzedrine and other experiments using sympatho-mimetic compounds should be used to support an hypothesis on boredom rather than a more obvious fatigue interpretation. A feeling of fatigue is, in fact, usually associated with boring work. Because of its transient character, however, it is generally referred to as a pseudo-fatigue or feeling of monotony.

For the following reasons, the data may be more accurately integrated about a boredom concept than about a fatigue concept: (1) The *S*s themselves verbalized post-experimentally their experiences as predominantly boring, rather than fatiguing, and this was particularly true after the 2-hr. pursuit-task. (2) In another study, *S*s added numbers under the same conditions for 4 hr. without appreciable difficulty.[21]

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[17] *Op. cit.*, 299. [18] *Op. cit.*, *J. Psychol.*, 132. [19] *Op. cit.*, 298.

[20] *J. Exper. Psychol.*, (in press).

[21] Barmack, The length of the work-period and the work-curve, *J. Exper. Psychol.*, 25, 1939, 109-115.

It is hardly likely, therefore, that the *Ss* in the Benzedrine experiment "were probably forced to exhaustion" within 2 hr., as Berman suggests.[22] (3) In still another study, with pursuit activity as the task, the introduction of a small added financial incentive after one hour of work resulted in a marked improvement in accuracy of performance for the second hour which surpassed that of any period of the first hour.[23] Accompanying the marked improvement in pursuit performance were equally striking changes in report of boredom, fatique, sleepiness, etc., in the more favorable direction. It is improbable that such changes would occur as readily in fatigued *Ss* as in bored *Ss*. This finding is relevant here because the observed effects of Benzedrine on the report of boredom and other factors were duplicated with this type of task.

The inference is compelling that Berman's article arose chiefly out of a number of confusions which resulted from his unwarranted substitution of both the word and concept of 'satiation' for 'boredom.'

College of the City of New York JOSEPH E. BARMACK

SATIATION OR BOREDOM? A REJOINDER TO DR. BARMACK

In a former paper appearing in this JOURNAL,[1] I attempted to demonstrate that the concept of satiation cannot entirely be explained in terms of a reduction in motivation and return to a sleep level, as suggested by Barmack.[2] The arguments presented are attacked by Barmack.[3] The principal criticism leveled against me is that I accepted the equivalence of the concepts of satiation and boredom—even to the point of a substitution of terms. Before the validity of such a procedure can be determined the operations governing the two concepts must be investigated. The term *psychical satiation* was originally used (as far as it is possible to tell) by Karsten and Lewin.[4] I objected to the term *psychical* because it implies that satiation is purely a mental process. It is easily shown that such a distinction cannot safely be made, for satiation is a behavioral phenomenon and not solely mental. Consequently the appendage *psychical* was dropped and the term *satiation* itself was employed as denoting behavioral satiation. It was recognized, in addition, that satiation has as many manifestations as there are behavioral activities. Thus one can be satiated with eating a certain food, or with all food (*i.e.* hunger satiation); with sexual activity; with listening to music; with adding numbers; with adjusting a bolt on a moving assembly line; etc. In fact, satiation may appear for any task or activity when it is repeated sufficiently to induce a negative valence and rejection of the task. It is not known whether the satiation which arises with the continuous activity of inserting pegs into a board is the same as that which develops with the repetitive ingestion of a single food, or class of foods. It is reasonably certain, however, that in general the principle of the satiation of behavior holds for all levels of activity. In my paper, the satiation of basic drives, *i.e.* hunger, sex, etc., was called primitive as compared to the behavior manifested in the higher levels. In the studies under discussion satiation is confined to behavior above that of the simple drives.

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[22] *Op. cit.*, 299.

[23] Unpublished material.

[1] A. Berman, The effect of benzedrine sulphate (amphetamine sulphate) on satiation, this JOURNAL, 52, 1939, 297-299.

[2] J. E. Barmack, The effect of benzedrine sulphate (benzyl methyl carbinamine) upon the report of boredom and other factors, *J. Psychol.*, 5, 1938, 125-133.

[3] *Supra*, 467-471.

[4] K. Lewin and A. Karsten, Untersuchungen zur Handlungs- und Affektpsychologie: V., Psychische Sättigung, *Psychol. Forsch.*, 10, 1928, 142-254.

The tasks employed by Dr. Barmack (adding 6-place numbers) and by me (maze activity, peg insertion, and drawing faces)[5] are not generically different from those used by Karsten and Lewin,[6] Curran and Schilder,[7] Fritsche,[8] and Reichenberg[9] in their work on satiation. Yet Dr. Barmack maintains that his experiments deal with boredom and not with satiation.[10] He bases his claim upon the point that his *S*s were given an arbitrary time limit of 2 hr. to perform; whereas in my experiments the *S*s were permitted to stop of their own accord; and in later experiments forced to continue until the task was rejected two or three times. He states:

> In my own studies and other studies which boredom has been the focal point of interest, the initial activity may be described conceivably as satiating, but the subsequent activity may be more effectively described as a not too happy *toleration* (italics his) of an unpleasant task. In this situation, the subject conforms outwardly to the instructions of the experimenter, but expresses a negative valence to parts of, or to the whole of the task by a diminished or variable rate of work, day-dreaming, alterations in procedure, etc. Psychic satiation, therefore may be regarded as but one aspect of the state of boredom.[11]

It seems that the entire distinction involves one of degree. When an *S* persists through an arbitrary experimental requirement beyond the point where he would normally reject the task, Dr. Barmack would have us call it boredom; but if *S* rejects the task of his own accord before the time limit, Dr. Barmack would call it satiation. Certainly the task becomes more intolerable as satiation progresses, and *S* musters all his energy and ingenuity to persist, but does this create a new process, or is it merely the same phenomenon carried to an unusual degree, *i.e.* over-satiation? It seems that Dr. Barmack has introduced an extraneous factor. Surely he has not demonstrated that *satiation* is *"but one aspect of the state of boredom"* (italics mine). In the final analysis, an examination of the operational aspects of satiation and boredom reveal that both processes may be elicited in the same manner and show similar reaction patterns within similar frames of reference.

If it is accepted that satiation and boredom are similar processes, then there is no rigid incompatibility in the actual operational facts of Dr. Barmack's experiments and mine. The difficulty arises in the deductions drawn from the facts and the bias of the interpreter. Not a small part of the difficulty seems to be the level of description or explanation preferred for the interpretation. Since our knowledge of the physiological basis of satiation is scant, I prefer a more psychological description. This does not mean, however, that physiological facts are to be ignored when they are known. On the contrary, motivation (drive) does seem to play a part in satiation but it does not explain all the facts as the theory of a return to a sleep level would imply. There is no disputation of the action of benzedrine sulphate on satiation, but only in the putative justification that this action has for Dr. Barmack's theory.

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[5] A. Berman, The relation of time estimation to satiation (*in press*). Successive reproductions of visually perceived forms, (*in press*). The influence of social factors on the persistence of satiation in pre-school children (*in preparation*).

[6] *Op. cit.*

[7] F. J. Curran and P. Schilder, Experiments in repetition and recall, *Ped. Sem.*, 51, 1937, 163-187.

[8] L. Fritsche, Über psychische Sattigung, *Zsch. Psychiat. Neurol.*, 97, 1937, 291.

[9] W. Reichenberg, An experimental investigation on the effect of gratification upon effort and orientation to reality, *Amer. J. Orthopsychiat.*, 9, 1939, 186-203.

[10] *Supra,* 467-471.

[11] *Op. cit.*

It is beyond the limits of this note to respond at great length to the variegated ramifications of the problem under discussion. I can, however, delineate some of the arguments under the following rubrics: (1) Some scattered evidence and personal experience indicates—as mentioned in my previous paper—that sleep, or a a sleep-like state, is best fostered by an exclusion of heterogeneous sensory stimuli.[12] In the distal stages of satiation, the mechanisms (*substitutions*) created by *S* to persist at the task provide the heterogeneous stimulation. Not one of the 122 *S*s employed in a variety of experiments on satiation has ever succumbed to sleep during the trials, or immediately after them. (2) The use of statistical measures, *e.g.* the mean as used by Dr. Barmack, does not invalidate the argument that subjective rating scales cannot be the sole criterion of the presence of satiation. Such a procedure must always be accompanied by non-resumption of activity, characteristic variability in performance, the presence of *substitutions,* as day-dreaming. (3) Forcing the *S*s to unusual degrees of over-satiation complicates the picture by increasing the fatigue factor, which should be kept at a minimum in satiation studies. In addition, such abnormal periods of activity introduce extraneous quasi-needs and ignore the individual differences of the *S*s in the capacity for satiation.

It is gratifying that satiation is assuming more and more a place in the experimental psychological literature. Particularly is this true in light of the fact that satiation has been so surprisingly neglected in favor of the concept of aspiration level. When additional facts become available, it will be possible to speak with greater emphasis about satiation.

University of California ARTHUR BERMAN

THE TENTH ANNUAL SPRING MEETING OF THE EASTERN PSYCHOLOGICAL ASSOCIATION

The tenth annual spring meeting of the Eastern Psychological Association was held at Bryn Mawr College, Bryn Mawr, Pennsylvania, on Friday and Saturday, March 31 and April 1, 1939. There were 660 registered for the meeting which is the largest attendance in the Association's history. Of these, 351 were members and associates of the American Psychological Association, and therefore members of the Eastern Association. A total of 309 guests registered. Bryn Mawr College opened the dormitories and dining halls for the use of the Association from Thursday evening until Sunday morning. As all meetings were held on the campus, the facilities were most excellent for so large a group.

A total of 113 papers were presented. On Friday morning the following sessions were held: Educational Psychology, Sensory Psychology, Brain Function, Attitudes, Applied Psychology, and Experimentally Induced Conflict. A special luncheon, sponsored by the A.P.A. Committee on Displaced Foreign Psychologists, was held on Friday noon. Gordon W. Allport, President of the A.P.A., presided. In the afternoon, sessions were held on the following topics: Abnormal Psychology, Vision and Audition, Animal Motivation, Tests and Measurements, General and Experimental, and Genetic Psychology.

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[12] David Katz, *Animals and Men,* 1937, 34.

Round tables were held late Friday afternoon. The group discussing the "Functions of the School Psychologist" was led by T. E. Newland of the Pennsylvania Department of Public Instruction. W. S. Hunter of Brown University was chairman of the round table on "The Law of Effect and the Laws of Conditioning." The Society for the Psychological Study of Social Issues sponsored a discussion on "Critical Problems in the Formation and Change of Attitudes." Daniel Katz of Princeton University was the chairman of the group. The round table on "Mechanisms in Visual Perception—Interrelations of Constancy, Contrast, Conversion and Adaptation" was led by Harry Helson of Bryn Mawr College.

The President of the Eastern Psychological Association, Karl M. Dallenbach, Cornell University, addressed the group in Goodhart Hall on Friday evening. His topic was "Pain: History and Present Status." Following this meeting Bryn Mawr College held a reception for members and guests.

On Saturday morning, papers were presented in the fields of Physiological Psychology, Conditioned Response, Personality, Human Learning, Intelligence, and the Rorschach Tests. At noon a special luncheon was held by the Psychologists' Committee for Aid to Spanish Democracy.

Saturday afternoon was taken up with the annual business meeting and a general session. The President presided at both. D. G. Marquis, Yale University, read a paper on "Recent Advances in the Conditioned Response." O. A. Oeser from the University of St. Andrews, Scotland, presented a paper on "Field Work in Social Psychology—The Study of Unemployment in a Scottish Industrial City." George H. Gallup of the American Institute of Public Opinion addressed the group on "Some Problems in the Measurement of Public Opinion."

At the business meeting of the Association, the following recommendations of the Board of Directors were approved: (1) to hold, in the future, annual meetings two days in length; (2) to authorize the appointment of a committee to study the question of membership and of the organization of the Association (the President appointed the following: D. H. Fryer, Chairman, H. S. Langfeld, from among the general membership, and Theodora M. Abel, Harry Helson, and H. S. Oberly from among the members of the Board of Directors); (3) to empower, since an invitation for the 1940 meeting had not been received, the retiring president, the retiring secretary, and the incoming president to select a time and a place for the next annual meeting; and (4) to contribute $50 to the American Psychological Association's Committee on Displaced Foreign Psychologists.

The Association elected F. L. Wells, Harvard University, to serve as President for 1930-1940; Harry Helson, Bryn Mawr College, to fill the unexpired term of one year as Secretary-Treasurer; Theodora M. Abel, Trade Extension Classes, New York City, and George W. Hartman, Teachers College, Columbia University, to serve as members of the Board of Directors for the period 1939-1942; H. S. Oberly, University of Pennsylvania, to serve as a member of the Board of Directors, during the one year remaining in the term of Harry Helson.

The Board of Directors announced the appointment of the following Committees for 1939-1940. Program Committee: E. Heidbreder, Wellesley College, Chairman, Ross A. McFarland, Harvard University, and L. W. Max, New York University; Nominating Committee: George Kreezer, Cornell University, Chairman, M. B. McGraw, Columbia Medical Center, O. H. Mowrer, Yale University.

The eleventh annual meeting of the association will be held, as was subsequently decided by the Committee appointed for that purpose, at Atlantic City, New Jersey, on Friday and Saturday, April 5 and 6, 1940.

University of Pennsylvania H. S. OBERLY

THE FOURTEENTH ANNUAL MEETING OF THE MIDWESTERN PSYCHOLOGICAL ASSOCIATION

The Midwestern Psychological Association held its fourteenth annual meeting at the University of Nebraska, Lincoln, Nebraska, Friday and Saturday, May 5 and 6, 1939. The register was signed by 362 members and visitors who attended the meeting. On this occasion the University of Nebraska commemorated the 50th anniversary of the founding of its psychological laboratory by Harry Kirke Wolfe.

At the annual business meeting on Friday afternoon, 54 new members were elected, bringing the present membership list to 469. Announcement was made of the election of J. P. Guilford to the presidency for the year 1939-40; of Robert H. Seashore to the office of secretary-treasurer for the term of 1939-42; and of Norman R. F. Maier to the executive council for the term 1939-42. It was voted to contribute $25 to the American Psychological Association's Committee on Displaced Foreign Psychologists. It was announced that the next annual meeting of the Association will be held at the University of Chicago, the exact dates to be announced later.

Ten sessions were held at which 65 scheduled papers were read. The emphasis, as indicated by the numbers of papers, was upon studies of attitudes which occupied two sessions. Other sessions were on the conditioned response, mental development, personality, sensory and nervous phenomena, applied psychology, learning, and mental tests. Symposia were conducted on clinical psychology, personality, mental hygiene, motivation, constancy of the IQ, contemporary social problems, factor analysis, learning, and on principles of development. The customary "reports from laboratories" was held as a symposium on the first morning, with prepared summaries of certain studies in progress.

At the annual banquet held on Friday evening, Arthur G. Bills presided. Dean J. E. Le Rossignol, of the College of Business Administration, gave the address of welcome, livened with reminiscences of his study of psychology with G. Stanley Hall at Clark University and with Wundt and others at Leipzig. Walter B. Pillsbury delivered an anniversary address on the Nebraska Psychological Laboratory, relating particularly its early history under Dr. H. K. Wolfe. Edmund S. Conklin gave his presidential address on "The Status of Academic Psychology." In it he deplored some recent trends toward the dismemberment of departments of psychology in a few instances, even in universities where psychology has enjoyed a long and distinguished history. He issued a call for the type of leadership among psychologists that will gain respect, and for the minimizing of inconsequential differences which give outward impressions of inner lack of cohesion.

A special lecture on "Orientation of Organisms in Time and Space" was given by Karl Bühler as a contribution of the local chapter of Psi Chi, national honorary society in psychology, to the program on Saturday afternoon.

A resolution passed at the business meeting requested the secretary to thank the University of Nebraska, the host department, and the members of the local committee, composed of D. W. Dysinger, chairman, George R. Thornton, Warren

R. Baller, Leland H. Stott, Wilbur S. Gregory, Winona M. Perry, and Edmund E. Dudek, for providing facilities for an enjoyable meeting.

University of Nebraska J. P. GUILFORD

THE 1939 MEETING OF THE AMERICAN ASSOCIATION OF APPLIED PSYCHOLOGISTS

The American Association for Applied Psychology will hold its annual professional conference Friday through Sunday, December 1-3, 1939, in Washington, D.C. The Association had voted to meet with the International Congress of Psychotechnology should it be convened in America but that has proved impracticable. The By-Laws of the Association provide that "in so far as possible the Association shall coördinate its program with that of the American Psychological Association." It did not seem the part of wisdom, however, to take the annual meeting of so young an association so far from its center of population as would be necessary if it met this year with the American Psychological Association in San Francisco. The Applied Association voted, therefore, to meet for this year in the East at a time not in conflict with the American Psychological Association. There is no implication that the two associations shall continue to meet separately.

Ohio State University H. B. ENGLISH

Psychosomatic Medicine: Experimental and Clinical Studies

An introductory statement (pp. 3-5) in the first number (vol. 1, no. 1, January, 1939) of this new periodical explains that 'psychosomatic medicine' is not synonymous with psychiatry (a subject "concerned with the study and therapy of the disturbances of the mind whether these disturbances are the results of emotional experiences or of anatomical changes . . . of the central nervous system"), but "designates a method of approach to the problems of etiology and therapy." It is the "psychological approach into general medicine," which means, to the editors of the new magazine, "the understanding and the therapeutic utilization of the psychic component in the disease process and in the emotional relationship between physician and patient." That the psychoanalytic context shall not be confused with the old psychology, "so common in the last century," . . . "the medical reader" is warned that in "reading the expression 'psychosomatic medicine' he may be reminded of scientific or philosophical writings . . . as for example Wundt's 'Psychophysiologie' (sic) or 'Völkerpsychologie,' " books "devoted to scientific disciplines which were essentially nonexistent." [Who acquainted with the history even at second-hand would have blundered over Wundt's 'psychophysiologie,' would have thought the main psychologies of that time mere programs for the future and not "a report of facts already developed on the basis of observations," or would have said that the "main occupation of psychologists and philosophers consisted in writing introductions to scientific disciplines that did not yet exist."]

Over and above the exploitation of psychoanalytical concepts, methods, and theories, which the initial issue may well lead subscribers of the new journal to expect, the magic term 'totality' is obviously to be made fundamental. The division of "medical disciplines [why 'medical'?] into physiology, neurology, internal medicine, psychiatry and psychology" is said to be a mere convenience without biological validity. While the editors avow that there is "no logical distinction between 'mind and body,' mental and physical," they nevertheless demand that "psychological phenomena should be studied in their psychological causality with intrinsically psychological methods and physiological phenomena in their physical causality with the methods of physics and chemistry." Powerful is the magic of the words 'unity' and 'psychosomatic totality'!

Fifteen years ago the present reviewer found the compound 'psycho-somatic' permitted in the larger English dictionaries as a formal adjective for mind-and-body but seldom if ever brought into psychological service. He used it systematically in *The Field of Psychology* (1924) to imply moments inherent in the psychological functions. It proved to be a bad concept, for it opened the way to psychical forces and agencies, entities which are never observed and had better not be assumed in scientific contexts. The term was presently withdrawn and condemned, with the remark that it failed "to avoid the intermixture of magical with natural causes, and it failed also to relieve psychology once and for all of the monstrous 'mind-body problem'" (*The New Field of Psychology,* 1934, p. vii). Soon, however, the word 'psychosomatic' began to reappear in medicine, in 'psychobiology,' and in those psychiatrical theories and cults which revive old speculations about the unconscious and about a vitalistic libido. Now that ancient pair 'body-and-mind,' made one by the abracadabra of 'unity,' is again current, its articulated pluralities artfully concealed under such phrases as 'the total organism,' 'the whole man,' 'the synoptic vision,' the 'he' or the 'she,' and 'psychosomatic medicine,' all phrases incident to a widespread and violent reaction against the analytic *many* and favorable to the primacy of the *one.* The violence of the movement, as well as its pendent character, is well indicated by the recent name of M. M. Black's book *The Pendulum Swings Back.*

Besides the introduction the first issue contains three writings by F. Alexander. The chief contents of the number fall under (1) a review of recent contributions on the hypothalamus and (2) a symposium on hypertension. W. R. Ingram writes a competent and useful review of the morphological and functional investigations into the hypothalamus, a review which is notably free from the speculative constructions which have multiplied of late in this difficult field. The final section, reserved for 'reviews, abstracts and correspondence,' will doubtless acquire substance and significance as the journal gets under way.

The concept of emotion as 'dynamically' understood by physiological and medical theorists seems destined to supply a major theme for the new periodical, although psychology (one of those 'invalid' division of the 'medical disciplines') takes its modest place along with psychoanalysis, internal medicine, neurology, physiology, psychiatry, comparative physiology, and pediatrics.

The new periodical is to maintain a mailing address at the building of the New York Academy of Medicine and to conduct its business through the sponsoring committee at the National Research Council in Washington.

Library of Congress, Washington MADISON BENTLEY

MONOGRAPHS OF THE SOCIETY FOR RESEARCH IN CHILD DEVELOPMENT

The Society for Research in Child Development has added this monographic series to its quarterly journal, *Child Development,* and its bi-monthly issues of *Abstracts and Bibliography* in the same field. Three volumes of 6 monographs each are now complete. They "consist primarily of detailed critical reviews and seriatim studies" in the wide field of child development, which is advertised to include "anatomy and physical growth, physiology and biochemistry; nutrition; dentistry; pediatrics; physical health and disease; public health; mental hygiene and psychiatry; mental development and behavior; heredity, eugenics, evolution; education; sociology and economics." Of the first eighteen monographs, 14 are studies, reviews, and critiques in the large subject of physical and behavioral growth and development; 2 investigate mental aspects of childhood, 1 is devoted to personality of the child, and 2 are broad studies of methodology. Many of the studies are written by competent and experienced persons, who review and assay a large amount of existing research, chiefly American research. F. K. Shuttleworth and his associates have made further notable contributions to general somatic growth and sexual maturing; F. N. Freeman has recorded his evidence from repeated tests upon intellectual ability; N. Bayley has brought together a large amount of material on motor activities of the first three postnatal years, and J. W. Macfarlane has greatly aided all guides of the child by her wise and exhaustive survey (vol. 3, no. 6) of methods available for collecting and organizing fundamental data.

Those who have responsibly followed the checkered fortunes of the Society for Research in Child Development, of the antecedent committee of the National Research Council, and of the many agencies which operated under the Spelman Fund, will find high satisfaction in this monographic addition to the publications of the present Society. Let us hope that the Society's courageous efforts toward support and expansion will be sustained by all who profit from its competent labors.

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Thomas Russell Garth: 1872-1939

Thomas Russell Garth, Professor of Experimental Psychology at the University of Denver, died unexpectedly as a result of an attack of asthma, on April 20, 1939. He was born in Paducah, Kentucky, on December 24, 1872. He received his Bachelor of Arts degree from the University of Denver in 1909, and his Master of Arts from the same institution in 1910. His work for the doctorate was carried on at Columbia University, and the degree of Doctor of Philosophy was conferred in 1917. After serving as Adjunct Professor of Psychology at the University of Texas, he was appointed to the faculty at the University of Denver in 1922, a position which he held until the time of his death.

Garth's earlier work included a study of the psychological effects of the hook worm, an investigation carried on by the Rockefeller Foundation in which he coöperated with E. K. Strong. After he came to Colorado his interest centered in the study of the American Indian. At first he drew attention to the differences between Indians and whites as measured by psychological techniques. In his studies of the full-blood and mixed-blood Indians, he found that, the greater the percentage of Indian blood, the lower the score made on intelligence tests. He also showed that, generally speaking, the Indian showed no appreciable superiority in sensory acuity over the white. Hampered by a lack of funds he made use of many small grants and drew heavily on his own means in order to carry on his studies. His students were always enthusiastic in their coöperation, and he said frequently that without them he could not have made a beginning in his work.

In 1935 he came in contact with several American Indian students who had been raised in white families and who showed a much higher degree of mental ability than he had observed before. Thus he determined to make a general study of foster Indian children raised in white families, and he spent his summers to find well over a hundred such cases. In a number of these instances he was able to test the siblings that had been reared in the Indian environment. His results showed that the mentality of foster Indian children in a white home is about equal to the mentality of the white members of the same home. Dr. Garth at the time of his death seemed to be approaching the hypothesis that the differences in mental ability between races are largely dependent on environment and the opportunity for racial advancement rather than on racial stock.

Dr. Garth was a member of the American Psychological Association and was President of the Rocky Mountain Branch in 1932 and 1933. Among the best known of his psychological books are *Race Psychology* and *Educational Psychology.* He recently published *The Life of Henry Augustus Buchtel,* former Chancellor of Denver University, an appreciative and well written biography.

George T. Avery

Colorado State College

BOOK REVIEWS

Frustration and Aggression. By JOHN DOLLARD, LEONARD W. DOOB, NEAL E. MILLER, O. H. MOWRER, and ROBERT R. SEARS, with the collaboration of C. S. Ford, C. I. Hovland and R. T. Sollenberger. New Haven, Yale University Press (for the Institute of Human Relations), 1939. Pp. ix, 209.

This book promises more than it achieves. Introducing itself as a sample of coöperative research at Yale's Institute of Human Relations, it promises to illustrate the initial stages of an inductive-deductive method of science applied to this special problem of social psychology and to go about it with logical rigor. Ultimately it achieves five interesting essays which estimate and evaluate the variety, prevalence and inevitability of frustration in human society and, much less fully, of aggression. These essays attempt to present aggression as an immediate consequence of frustration and thus to support what is the regnant hypothesis of the book. Let us consider the essays first and the logic afterwards.

First there is Chapter 4, *Socialization in America,* which gives a genetic inventory of American frustrations from weaning to death. It is interesting to think of America as a Great Frustrator, and the list of frustrations is impressive. Here one learns something too about American aggressions, but the exposition neither strengthens nor weakens the common belief that frustration always leads to aggression. The discussion assumes this proposition; it does not demonstrate it.

The chapter on *Adolescence* and its frustrations (Chapter 5) is like a review of the literature in the *Psychological Bulletin.* It gives many facts in a rather uninteresting manner.

The best chapter in the book is *Criminality* (Chapter 6), a long essay which considers the dependence of criminality upon frustration with respect to twenty items, like educational status, intelligence, age, physical size, race, home conditions, drug addiction. The chief author here—the authors of the respective chapters are not specified although different authorship is indicated by different styles—is definitely trying to marshal the evidence that frustration leads to criminality. He is the more convincing because he finds doubtful eight of the twenty suggested conditions, and only twelve sure.

Almost as good is the chapter on frustration and aggression in modern America, Germany, Italy, and Russia (Chapter 7). Here it becomes clear how different are the frustrations of different cultures, how a nation may direct and displace the aggressions of its people, and thus how race prejudice, even though unreasonable, may stabilize a frustrated group. Although there is still no proving of the basic hypothesis, the description is plausible and convincing.

The last chapter takes the warlike and bold Ashanti and shows what frustrations are induced by their culture and how aggression apparently results. The Ashanti act as if they had planned their culture in the light of the major thesis of this book. They have, for instance, an annual cathartic festival in which for eight days anyone can tell anyone else, even the king, what he thinks of him!

So much for the essays. Now for the logic.

The book claims to be devoted to the consideration and the establishment or revision of the hypothesis "that the occurrence of aggressive behavior always presupposes the existence of frustration and, contrariwise, that the existence of frustration always leads to some form of aggression" (p. 1). This statement is put forth as the "basic postulate" in a chapter of definitions. The authors are aware of the danger of circularity; they know that, if all consequents of frustration are labelled *aggression* simply for that reason, they will have begged the question. Thus they seek "independent" definitions. "Frustration is independently defined as *that condition which exists when a goal-response suffers interference.* Aggression is independently defined as *an act whose goal-response is injury to an organism* (or *organism-surrogate*)" (p. 11).

The meanings of the chief concepts are made clear in the following manner by their psychological relationships. (a) Goal-directed behavior must start from some cause, be initiated by some situation. This first term is called the *instigator,* a neutral term. (b) The *goal-response* to the instigation is activity that is terminated when the goal is attained. There can be no doubt that this is the valid definition of purpose. The goal is what terminates an activity that leads to it; the direction of activity toward the goal is nothing other than the fact that it will be terminated by the achievement of the goal. (c) Activities nevertheless come in time to lead fairly directly to goals because of the *reënforcing effect* (law of effect) of previous successful achievements. (d) "An interference with the occurrence of the instigated goal-response at its proper time in the behavior sequence is called a *frustration*" (p. 7). Good. "In order to say that a frustration exists, then, one must be able to specify two things: (1) that the organism could have been expected to perform certain acts, and (2) that these acts have been prevented from occurring" (p. 7). (e) In the face of interference with activity, there is most often a *substitute response* in which some similar goal (raisin pie when there is no mince) is reached and the goal-activity terminated or diminished. (f) If there is interference with the activity and no substitute response, then the frustration leads to *aggression:* the organism tries to injure another organism, or itself, or a surrogate. (g) Often the aggressive activity is *inhibited*—usually by punishment in organized society—and the aggression has to find some substitute. (h) Thus aggression may turn in upon the self, but *self-aggression* normally occurs only as a last resort since it is painful. Its occurrence usually means that all other avenues for aggression are strongly blocked. (i) The *displacement of aggression* and its discharge upon a surrogate may terminate the instigated activity, the whole process thus constituting *catharsis.*

Dr. May, the Director of the Institute, says in the *Foreword* that the authors are using a well known scientific procedure: seeing a *problem,* they set up an *hypothesis,* criticize it in the light of available data, refine it, and repeat the process. The ultimate goal is mathematical statement and the predictive value that it yields, but Dr. May does not pretend that the social sciences get so far nowadays.

Now let us agree to this definition of frustration and admit further that frustration is frequently followed by aggression and that aggression when blocked tends to be displaced upon some surrogate. So much fact is supported by common sense and a wealth of psychological evidence, and the five essays of this volume increase its plausibility. The book, however, is not content with such a loose generality. It says: if Frustration, then Aggression, always; and conversely. But what about the negative cases? Can not one find aggression without frustration? Frustration without aggression? Are there not alternative hypotheses to be examined? The history of science is full of wrong hypotheses which were plausible enough and which were used for ages simply because they worked for a description of the phenomena, so long as the possible negative cases were not vigorously examined.

So we may ask: Is there not aggression without frustration? Now it can be argued that all mental life is aggressive, that striving (cf. McDougall) is exactly what it is to have a mind. In that case aggression could not always depend upon frustration unless frustration were universal too—as it might be if "divine discontent" is the essence of all motivation. This view of mental life is a very useful one. The organism is always striving for change; when it succeeds, it begins to strive for another change; when it fails, it persists and strives for a substitute; its persistence is noteworthy for it keeps on in spite of continued failure until it either achieves a vicarious success or is distracted. Aggression thus becomes the rule of life, but such aggression does not depend on frustration, although frustration depends on it.

Our authors, however, will object. They will remind us that aggression is defined so as to include only those activities that are injurious to another organism or its surrogate. To make injury the goal of aggression is definitely to limit the concept, but will such a limitation work? Is it aggression to strike a child? to spank a child? to spank a child for his own good? for your own good? for both his and your own good? Is it aggression to injure a child by giving him too much candy because you love him? Is it aggression to ride hard because you are angry, and then what is the surrogate object? My guess is that the authors are really appealing here to the outmoded introspective psychology of motivation, that for them activity is aggressive only when the subject *intends* harm, when he *hopes* for injury. That criterion is subjective and intuitive. It is not actual injury that is the goal of aggression because the aggression can be discharged without coming into direct contact with its object. Moreover, it is not clear what constitutes an adequate surrogate. If I denounce my enemy to the echoing hills, what have I injured? All in all it seems to me much clearer to say that aggression is any striving toward a goal, with the effort increased when there is interference until the interference is overcome or until the organism is redirected.

Now for our other question: Is there frustration without aggression? The answer would have to be *No* if all mental life is aggressive. On the other hand, if we try to keep to the narrow meaning of *aggression* which our authors had in mind, then there would seem to be exceptions to the Yale rule in the case of apathy and of distraction.

Lewin and associates (Patterns of aggressive behavior in experimentally created "social climates," *J. Soc. Psychol.*, 10, 1939, 271-299) have found that children, organized for collective endeavor under the restraints of an autocratic leadership, may experience frustration and react with aggression—often displaced aggression. On the other hand, they also find that, under certain conditions, the restrained children become apathetic and lose interest. In everyday life admission of defeat, withdrawal from the field, is common enough. What else is the meaning of *He shrugged it off?* Perhaps our authors will say that withdrawal from the field is the turning of the aggression in upon the organism's self, but surely that is nonsense. Do I never want food and put the thought aside without abusing myself? Of course, there are the traumatic frustrations which can not be put aside so easily, but there is nothing in this book to distinguish the simple interruptions of the goal-responses from dominating frustrations that will not be dismissed. The psychoanalysts think that fatigue is a symptom of repression. Is fatigue self-aggression or is it more like apathy?

Distraction is another normal escape from frustration. Our authors say that every frustration leads to aggression, although common sense would have it that the frustrated man is easily distracted. Suppose the young man, who had just been humiliated by the traffic officer, had then met his charming girl friend and she had started to admire him (p. 12), would his aggression (desire to injure the officer or his surrogate) necessarily have persisted? Have women never distracted men from aggression? Does a girl never recover from a frustrated love when her parents take her abroad? The truth would seem to be more nearly this: Activity tends to persist until the goal is achieved; interference heightens the activity; if progress toward the goal still fails, there is frustration, which results in (a) displacement of the activity to another goal, (b) apathy or (c) distraction of the subject to another activity. Do these authors really maintain that the activity of the salesman, who, having just achieved frustration in his attempt to make a sale, finds the building on fire, is in any discoverable degree dependent on the frustration, that there is no such thing as distraction and only substitute response?

Such a view is too universal—that all distraction is displaced aggression and all apathy is self-aggression. Then it would be only after perfect attainment of a goal or after complete catharsis that unaggressive activity could take place. It would become conceivable that the neonate, frustrated by his birth, would spend the rest of his life working out that perpetually reënforced original aggression!

Dr. May says that these five authors found the coöperation of writing this book together frustrating. In fact he makes his statement general: "coöperative work of an intellectual nature which penetrates beyond the level of superficial collaboration is itself frustrating" (p. vii). That statement I challenge. Coöperative work can occur in serious undertakings among intellectuals without frustration (a) when there is accepted leadership, (b) when there is clear division of function and the acceptance of a separate leader for each function, and (c) when there is so strong a desire for coöperation that the individual experiences a sense of achievement in surrendering his initial thesis for the ultimate synthesis. If the Institute wants coöperation it might have it on these terms, but my own advice would be for it to stick to competent individualists and to let coöperation go except when it happens spontaneously without frustration.

In a word, then, this book enlightens us with many interesting facts about frustration and aggression in social life, it seduces us (almost) with a plausible but defective logic of the universal concomitance of frustration and aggression, and it warns us to scrutinize the omens with great care before we venture upon coöperation.

Harvard University EDWIN G. BORING

Foundations of Education Psychology: Nature's Gifts to Man. By PETER SANDIFORD. New York, Longmans, Green & Co., 1938. Pp. xv, 464.

The reviewer had mixed feelings as he read this work which the author tells us in his preface is the first of a series designed to give a general survey of the foundations of education psychology for advanced students of education. The field covered by this first volume, he tells us, is approximately that which Thorndike called, "The

Original Nature of Man." A second volume, he promises, will deal with the "Psychology of Learning."

The writing of an up-to-date two or three volume treatise on educational psychology which will supplant Thorndike's out-of-date three volumes is a much needed work. It would seem, however, that Professor Sandiford has not gotten off to an exceptionally good start in this task, nor has he succeeded in writing a thoroughly acceptable book for the purpose stated in the preface. There are a number of prominent lacunae occurring in the discussion of certain topics, which omissions are really inexcusable in a book written for graduate students. Before pointing out some of these omissions let us point out some features of the treatment which pleased the reviewer.

In his first chapter on, "The Nature of Educational Psychology," there is an especially valuable summary, for the space allotted to it, of the main schools of psychology. The criticism of Gestalt and Organismic psychology and of certain fadish ideas and procedures which *some* progressive educators have attempted to justify by reference to these schools of psychology is particularly apt. The chapter on "Personality" is a valuable summary. Throughout the book many valuable researches are summarized in a helpful way with the aid of tables and charts; and the significance of the results well integrated. The reviewer would have liked more discussion of the educational applications of the various findings; but the author has chosen to leave that for the reader to supply in any large quantity.

In his discussion of heredity and environment it is unfortunate that Newman, Freeman, and Holzinger's study of twins is accepted at face value and in toto. The publishers have assured the reviewer that Sandiford's book went to press before McNemar's significant criticism of the Newman, Freeman, Holzinger study appeared in the April, 1938, *Psychological Bulletin.* It is unfortunate that such was the case. Otherwise a publicizing and approving of a study whose concluding chapter does not seem to follow from a careful analysis of what lies between the first and last chapters might have been avoided.

In the discussion of the conditioning of the emotions he stresses Watson's first experiment on conditioning fear in an infant and fails to mention. Bregman's work at Columbia and the observation of individual infants by two or three others, which show the difficulty of conditioning emotional responses, under certain conditions, by the technique which Watson used. The discussion of the training of the emotions overemphasizes the re-conditioning technique for removing fear responses and fails to mention the recent findings of Jersild and Holmes which suggest other techniques as more practical. The latter study is mentioned by title only, some pages later; but the findings and their significance are ignored.

The section which this reviewer found most unsatisfactory was the one on "Intelligence: Its Nature and Measurement." The first two lines read, "Intelligence is innate. Studies of heredity show that intelligence is innate, that it actually has its basis in the genes." What is this innate intelligence? A few pages later he tells us that the commonest features he finds in scores of definitions in his collection are, "(1) the ability to adapt oneself to novel situations; (2) the ability to carry on the higher processes of thought, especially abstract thinking; and (3) the ability to learn" (p. 337). Further on, regarding Spearman's theory of intelligence he says, "For this two-factor theory, the evidence supplied by Spearman and his students over a period of two decades seems to be overwhelming. . . . It should also be noted that the new techniques of factor analysis, instituted by Thurstone, Hotelling and others, lead to the discovery of factors such as language ability, motor ability, etc., when intelligence and other forms of tests are investigated. . . . Yet after examining the evidence, the balance is undoubtedly on the side of a unitary conception of intelligence" (p. 343-4). In an earlier section of the book twenty pages are devoted to the theory of the gene and the mechanisms of heredity. The reviewer, however, fails to find any evidence there, from psychologist or geneticist, nor any evidence in the author's discussion of family history and correlation studies, that would support the statement with which this section on intelligence begins; and which implies that a "mystical" g and the three "commonest features" of scores of definitions are the innate trait of intelligence. (He cannot help reminding the author of the discussion of the scientific method and principles, especially that of parsimony, which is so well presented in the introduction.) Among psychologists there is all too much confusion of this "mystical" intelligence with the "intelligence" which is what intelligence tests measure. What we can really talk about in any than a speculative way, are the test scores themselves and the effects of hereditary and environmental factors upon these scores. Discussions for the educator should be confined largely to this approach. Too much damage has already been done by the educator's misinterpretation of the psychologist's IQ as something accurately determined from one paper test, 'mystical,' innate, and unchangeable.

Even though he accepts Spearman's theory on the nature of intelligence that does not excuse him, in a book "for advanced students of education," for omitting, in the discussion of intelligence, any specific mention or even bibliographical reference of Thurstone's criticism of Spearman, appearing in 1934, nor for omitting Thurstone's more recent theory of seven primary abilities which was reported in print in 1936 and more extensively this past year.

The section on intelligence should have contained a much more critical discussion of the constancy of the IQ, mentioning the variations between individual and group tests and between various group tests. A much more extensive and critical discussion of the environmental influences upon IQ should have been given. In this connection, even though the author may not accept the conclusions, he is bound in a book of this type to mention the studies of this problem that have been coming from Stoddard and Wellman and their associates since 1932 and which culminated in the study of "Mental Growth from Preschool to College," appearing in December 1937.

In discussing intelligence tests for preschool children he fails to mention at all the Minnesota Preschool Scale; and although mentioning the Revised Stanford-Binet in other connections, fails to mention it as a test for preschool children. This is most unfortunate since they both appear to be much better tests of what we usually test by intelligence tests than the Merrill-Palmer Scale which he does go to the trouble to describe.

There is running through the book a careless tendency to omit, at times, the dates from the volume and page references to articles. This provoking omission is certainly not to be condoned.

It would seem that the text assumes a good bit of biological and physiological background which many education students may not have or be able to get readily. I wonder if we really should force this type of material upon them, or whether we should instead, give them the implications of these studies, with the references to the studies themselves. Then the student with the background and interest may look into them for himself. Rather than twenty pages on the mechanism of hereditary the reviewer would have liked to have found twenty pages discussing the best opinions of biologists and environmentalists concerning the contribution which eugenics and euthenics can be expected to make to the welfare of society.

Sandiford has written a valuable book. The advanced student whose knowledge enables him to recognize the lacunae will profit much from reading it. It would be too bad, however, to put it into the hands of the student just beginning his graduate work in educational psychology unless it is studied with warnings and critical guidance from the instructor.

Pennsylvania State College EDWARD B. VAN ORMER

The Achievement of Personality. By GRACE STUART. New York, Macmillan Co., 1938. Pp. 192.

To the typical American reader, including college students, the title of Grace Stuart's book will be misleading. To the psychoanalytically and theologically uninitiated it will no doubt be unintelligible. The title, *The Achievement of Personality,* probably suggests to most people a work devoted to a definition of the concept of personality and to intelligent ways and means of developing those desirable qualities that make for social acceptance and personal security.

Actually the book is concerned with a commendably ardent discussion of what is apparently believed to be the only logical avenue to the rejuvenation of man and the revamping of society. The author proposes an "air tight" philosophy, the essence of which is that to be happy and to resist the ravages of neuroticism man must be imbued with the spirit of love and must in turn be beloved by both his fellows and his God. This is of course a basic truism that, in its more prosaic and psychologically objective aspects, has been known to clinical workers for some time. The principle has not, however, been any too widely understood and applied by a frequently faltering society which could certainly benefit by its exemplification.

The impression one gets from reading this book is that it is more a psychology of religion than a psychology of personality, at least in the functional sense. It is purely subjective, quoting only other subjective excerpts from the writings of such countrymen (English) of the author's as McDougall, Hadfield, Brown, and Jacks, from American therapists such as Hocking and Pratt, and from the three psychoanalysts, Freud, Adler, and Jung. The book is really an integration of subjective psychology, Christian philosophy, deistic religion, and psychoanalysis, with strong accents on the latter two. Objective data of the more earthly variety as developed by Gesell, by Hartshorne and May, and by J. B. Watson are conspicuous by their absence. This omission was obviously in the intent of the author but it means that her book is decidedly abstract, idealistic, and far removed from the concrete channels of everyday living.

A brief résumé of some of the author's "pegs" on which she hangs her concept of the union of egoism and altruism as the answer to man's quest for happiness should be enlightening to the reader. First of all man is actuated by a dynamic "life force" which partakes of the nature of Schopenhauer's "will to live," Bergson's "Elan Vital," McDougall's "instincts," and the psychoanalysists' "libido." This life force is personalistic rather than mechanistic since it constantly reaches out to get the things that the human organism needs. The personality becomes organized by developing emotional dispositions in connection with objects and people in the environment as per Shand's law of "sentiment-formation." Personality is a "seed" or "soul" which is to be so conditioned and nourished that the sentiments accruing will favor the Good, the Beautiful, and the True. The psychophysical life force (and here enters dualism) is to be given the "out-turned" direction that makes for magnanimous love and that obviates neurotic self-concern.

But the personality has driving needs which must be reasonably well met if it is to resist disintegration. To the author these needs are summed up, with some reservations and amplifications, in Freud's "need for love," Adler's "need for significance" (will to power), and Jung's "need for security." The need for love and the need for significance are said to be somewhat synonymous but the need for security as propounded by Jung is broader and more fundamental in that it introduces the religious principle that man thrives best when he struggles valiantly through adversity and tribulation toward his precious goal of self-realization through sacrifice of self-concern. Neuroses can only result when love, recognition, and security are not realized. But love, recognition, and security cannot be sought directly, they must be won by way of an outflow of love for humanity.

Man's energy, which is originally inclined to satisfy the selfish desire for pleasure (Freud), power (Adler) and indolence (Jung), must eventually come to flow *out* toward sacrifice and social altruism. This means moral conflict for the growing personality. The noble ends of living are best but the immature self-desires are hard to give up. If and when the sacrifice is completely made sin, cruelty, war, prostitution, and other such miseries will fade away like a bad dream. Unequivocal love with its righteous behavior is entirely possible when man sets his affections on "lovable objects."

The author speaks of God rather gingerly. There may not be a God but psychologically man needs him. Psychotherapists should not expose their doubt on this point lest the therapeutic value of "God belief" be lost for their clients. But be God as he may, man's most imperative and irreducible need is an environment which provides him with love and in which he can love others. Finally, a world in which man must struggle for material and spiritual things is good for him, but a situation in which he cannot be, or is not, loved will inevitably lead to his destruction. Personality is strictly relational. To live man must be a lover and he must be beloved. This is the balance that good adjustment demands.

In terms of her purposes and her philosophy the author has certainly expressed herself logically and adequately. Each of her chapters leads naturally to the climax presented at the very close of the book. However, when one considers the dualisms, the "mind" and "soul" implications, the emphasis on instincts and psychoanalytic concepts, and the neglect of modern descriptions of concrete personality organization and development, it seems to the reviewer that the author is going through a stage of development in the personality field that characterized most American psychologists some fifteen or twenty years ago. Perhaps Mrs. Stuart is aware of this fact but wishes to eschew the objective monism which seems to be so dear to our temper.

University of Southern California LOUIS P. THORPE

Methoden der Psychologie des höheren Gefühlslebens. By G. Störring. Berlin and Wien, Urban & Schwarzenberg, 1938. Pp. viii, 522.

The present volume was originally a contribution to Abderhalden's encyclopaedic handbook on methods of biological investigation, but is now issued as a separate work in order that it may be available to wider circles. It is the first work in any language which attempts to assemble, explain, and evaluate, all of the methods which have been employed in the investigation of feelings, emotions, and effective processes in general, both as analytic phenomena and in their relation to other psychological processes.

For the execution of this task the author is particularly well qualified, because of his distinguished contributions in this field, as well as his lifelong study of the relations between psychology and psychopathology. Indeed as early as 1900, Professor Störring produced the first systematic treatise on the significance of abnormal mental phenomena for normal psychology, a work translated into English in 1907, by Thomas Loveday, under the title of "Mental Pathology in Its Relation to Normal Psychology."

The present work on the study of affective processes of a higher order, is at once encyclopaedic and systematic. This is the happy result of a comprehensive aim, an admirable mastery of materials, and a gift for logical organization. It is therefore possible to study any particular section with profit, apart from the whole, although the book is organized in strict accord with a definite system of affective psychology.

The author divides the total field of affective psychology into general and special disciplines, devoting the larger portion of his work to the general field. In pursuing investigations of this kind, four methods have been employed; *viz.* (1) the psychopathological method, (2) the experimental method, (3) the introspective method and (4) the method of folk psychology. Of these the psychopathological method is fundamental according to the author. The other methods, however, are of great importance as independent means of confirming or illuminating the results of the basic method, while in some situations they offer the only means of discovering relevant affective elements and their laws. This is especially the case with the method of folk psychology which is indispensable in the investigation of the complex affective processes involved in ethical and esthetic situations, and indeed in all situations where human values are in question. It is notable that Dr. Störring true to the tradition of the Great Age of German psychology of which he is one of the last representatives, does not disdain the introspective method. He devotes more than one hundred pages to its uses and results, and suggests that affective consciousness is a form and energy which may find expression in overt behavior of profound significance to the individual and society. The direct study of such affective consciousness, by introspective methods, must be indispensable to any really constructive human psychology.

The first and longer section of the work is concerned with the general theory of affective processes and comprises three sections dealing successively with the nature and manipulation of the psychopathological, experimental, and introspective methods of investigating affective processes of all degrees of complexity and consciousness.

Full justice is done to the recent contributions of psychoanalysis and allied schools, while none of the classical psychologists or traditional schools are neglected.

The second part of the book treats of the special theory of affective processes, and presents a discussion of feelings of value and specific emotional judgments of worth as these occur in ethics and esthetics. The second section of the discussion presents a schema of the investigation of ethical value judgments by means of the techniques of folk psychology, and the work closes with a chapter on the logic of valuation in the normative sciences.

In his treatment of an extensive literature, Dr. Störring exhibits a fine historical perspective and a rare sense of the relative importance of different contributions to affective psychology. He summarizes briefly the methodological contributions of all important writers from every school of psychology whether old or new. Without wasting time in polemics, he indicates the value and limitations of the work and whatever contradictions may exist with the works of others. He is apparently considerably impressed by certain correlations between Ach's recent findings in the investigating emotional objectivation, and the typological theories of C. G. Jung. Capacity for emotional objectivation (Objectionsfähigkeit) appears to be correlated with extraversion and increased suggestibility, while a weaker capacity for emotional objectivation seems connected with introversion, reduced suggestibility, and psychic resistance. If true, this generalization may justify the theory of an emotional basis for civilization as proposed by certain historians and social psychologists.

Although the author gives precise references to all authorities in footnotes, a general bibliography would add to the value of this splendid work, which merits translation into other languages, as a general guide to investigators of the affective life of man.

University of Cincinnati CHARLES M. DISERENS

"Truth" as Conceived by Those Who Are not Professional Philosophers. By ARNE NESS. Oslo, I Kommissjon Hos Jacob Dybwad, 1938. Pp. 178.

The writer of this dissertation in English from the Norwegian Scientific Academy in Oslo is motivated by the laudable desire to find out exactly what those who are not professional philosophers mean by "truth." He is impressed by the glib way in which philosophers speculate as to the plain man's view of truth without any empirical evidence to back up their formulations thereof. "Why," he asks, "is observation systematically neglected" in this matter, and "sweeping, vague, general statements" offered instead? Feeling convinced that it would be of value to know definitely what non-philosophers really do understand by truth, our author proposes to determine this in a scientific way, by constructing situations calculated to arouse verbal reactions adapted to the solution of this problem.

The method used is that of the questionary. Some 250 "test-persons" were selected for examination, and about 6000 hours were expended in the prosecution of the study. The subjects varied in age from 7 to over 30 years, and in educational background correspondingly, and there was a slight preponderance of males among them. But the author is not at all clear or systematic in his presentation of the essential data. He employs also a quite outlandish system of abbreviations, and the list of them which appears at the beginning of the book is incomplete and not particularly helpful.

The central question under investigation is the following: "What is the common characteristic of what is true?" But a number of other questions are also propounded, such as these: "Give an example of something that is true." "Is there something absolutely true?" "What is a fact?" "Mention something you know with certainty." "How do you explain that you know these things with certainty?"

The answers are about as diverse as there are subjects to affirm them, but they are grouped by the author in a number of ways too numerous for us to recount here. Typical answers are such as these: Truth is "agreement with reality," "something that happened," "that which can be proved," "that which cannot be doubted," "what is arrived at by using one's senses," "something that must be thus and thus." Many of the subjects are skeptical as to whether truth has any common characteristics at all.

The author has not formulated his conclusions in any systematic fashion, and all we seem justified in saying about them is that whatever results *do* issue from these elaborate questionnaries are of a purely tentative, not to say negative, character. Mr. Ness himself estimates that 30,000 hours would be necessary for accomplishing anything of positive value—five times the paltry 6000 hours already actually expended by him. "No marked, real correlation" is found between the character of the answers and the age or educational status of the subjects—or between any other two factors, so far as we can discover.

As to the fundamental question, What is the notion of truth common to all non-philosophers? the only possible answer, if our investigator is right, is this: There is no such common notion of truth among non-philosophers, any more than there is agreement as to the nature of truth among philosophers themselves. On these and like matters, indeed, our author finds little difference between these two groups of persons: philosophers, we are told, like to stress their own cleverness by calling non-philosophers "stupid," but after all there is not much to choose between them. Nor, finally, is there any more justification for calling the common sense view of truth "realistic" than there is for calling it non-realistic.

This book, in the opinion of the reviewer, suffers from an excess of complex methodology quite out of proportion to the meager results finally obtained. The author tries desperately hard to be "scientific," but seems to have little to show for it when all is done and recorded. His English is not always above reproach, but it is, nevertheless, generally speaking commendably free from solecisms, and there are fewer misprints than are usually found in works of this character.

Western Reserve University JARED S. MOORE

Analysis of Parergasia. By GLADYS C. TERRY and THOMAS A. C. RENNIE. New York, Nervous and Mental Disease Monograph, 1938. Pp. 202.

As those acquainted with the work of Adolph Meyer and the psychobiological school will recall, *parergasia* has been proposed as a more plastic term for schizophrenia. This monograph proceeds from the concept of schizophrenia as dependent on a special constitution and personality characterized by habit disorganization. The authors emphasize a genetic-dynamic understanding of the patient through a study of his total life record and of the factors entering at various integrative levels. By such an approach alone may we comprehend the chaotic schizophrenic reactions and their meaning to the individual patient himself. In contrast to a fatalistic acceptance, the focus is on potential re-synthesis "in recreating the genesis and dynamics of the life record" and by management of developments in the psychosis itself.

The material analyzed comprises 77 case histories selected from a preliminary survey of approximately 250 cases diagnosed as parergasia at the Henry Phipps Psychiatric Clinic. The selection was made for the purpose of singling out items that have proved significant in the life records of the parergasic group. A key to these individually coded and charted histories presents an extensive list of topics under nine major headings. Seven of these headings deal with items of import in the prepsychotic lives, the other two with features of the psychotic development and outcome. In any study of parergasia this key should be of crucial value; particularly when one realizes the time element involved in assembling the material, the completeness in telling items, and the fact that the services of a large, varied staff were utilized.

For the purpose of differential diagnosis the monograph offers six contrasting clinical reaction-patterns: aggressive, conversion, passive, deliroid, affective, and defeatist with their respective sub-groupings and descriptions. Each pattern is illustrated with case material. It is a credit to the scientific prudence of the authors that nowhere do they attempt rigid categories or unwarranted statistical refinements. Frequencies of items sifting out from the material studied are adequately tabulated under topics taken from the key. These tables serve as a frame of reference for subsequent discussion.

According to a basic formula psychobiological happenings are conceived as *somatogenic, endogenic, exogenic,* and *neurogenic.* The resultant analytic summary of this heterogeneous material suggests the following as characteristic of the parergasic group: (1) *in the organism,* we find striking incidence of instabilities and oddities in parents and siblings; asthenic habitus, dysplaic features; early unstable or inadequate behavior, fears, solitary trends, poorly directed aggression; (2) *in the subject,* disturbances in menstrual, gastrointestinal, cardiovascular, and skin-functions; dependence upon parents, poorly organized sex trends, indecision in love and martial problems; exaggerated need for approval, feelings of inadequacy, shyness, interests limited, self-centered and lacking in spontaneity; (3) *in the situation,* unstable parents, particularly mothers, over-attachment to the mother; domineering or overprotective family, unfavorable contrasts with more successful siblings; rarely achieved extra-familial adjustments, sensivity to opinion, incongruous appearance of indifference; increasing asocial trends; (4) *in the brain,* events inducing convictions of inadequacy, conflictual strivings, loss of inhibition, overreaching goals, fantasies, suspicions, uncritical thinking with acceptance of delusions and hallucinations, loss of planning, scattering.

In short, the parergasic personality is fundamentally a dependent and passive personality whose basic developmental features show an insufficiency reaction making for discrepancy. The life record manifests much *Sturm und Drang,* with cataclysms along the way determining the direction of the upheaval. In adolescence particularly attempts to fuse sexual and social needs are poorly integrated. For mental hygiene the implications are clear-cut and imperative. Unfavorable familial and extra-familial factors, early manifestations of sensitiveness and unfavorable comparisons must be recognized and guarded against. Thinking in the concrete is to be encouraged, as are modification of goals where necessary, desensitization of unfortunate sex experiences, and expansion of ego-centric interests. Alove all, security and understanding must be provided.

Although written primarily for a psychiatric audience *Analysis of Parergasia* will generously repay the serious student of abnormal psychology and mental hygiene. The point of view is constructive, never fatalistic. Throughout the material is handled in an orderly fashion; the cards are allowed to fall which way they will. The psychobiological approach developed by Adolph Meyer is reëmphasized and sustained. Wherever interpretations are made a high degree of empiricism and a will to see the interdependence of dynamic factors in the life history go hand in hand.

University of Maryland WALTER M. SPARKS

Modes of Thought. By ALFRED NORTH WHITEHEAD. New York, Macmillan Co., 1938. Pp. viii, 241.

Professor Whitehead's latest volume is a collection of chapters from various sources: the first six, a series of lectures delivered at Wellesley College in 1937-1938, on the general subjects of "Creative Impulse" and "Activity," and now constituting Parts I and II of our book; then two lectures on "Nature and Life," delivered four years earlier at the University of Chicago, and already published in separate form by the Press of that University; and a final epilogue on "The Aim of Philosophy," adapted from an informal address delivered to Harvard and Radcliffe students in 1935. In consequence of this method of composition, it is hard to trace any single thread of continuous thought running through the entire book, and the already-published lectures on "Nature and Life" are certainly clearer and of more general interest than the portions that are new. If one is pressed, however, to say what is the main purport of the book one might assert that *Modes of Thought* is a study of dualities, of pairs of contrasted ideas which must be joined together in any adequate world-view—such ideas as matter-of-fact and importance, particularity and generality, variety of detail and unity of significance, temporal process and eternal form, the actual and the potential. The world, Professor Whitehead seems here to tell us, is a vast conglomerate of constantly changing processes, ever pushing ahead to newer and newer goals; and it is the function of intelligence, not so much to guide the direction of these processes, as to choose between the more and the less significant ones, and to see them all in their proper perspective and in their relation to one another.

As of special interest to the psychologist, the following points stressed by the author may be noted: (1) the distinctive nature of man; (2) the importance of consciousness; (3) the function of language; (4) the nature of understanding; and (5) the superficiality of sensory experience. I will briefly elaborate each of these points. (1) Human mentality differs from that of the lower animals chiefly in its twofold capacity to conceive ideal possibilities and to understand the structure and formal relations of things: "to be human requires the study of structure, to be animal merely requires its enjoyment" (p. 105). (2) Consciousness is not an otiose accompaniment of bodily functioning, and still less a mere synonym for cerebrally initiated behavior, but a true "agent of stimulation" (p. 44), an emphasizing and selecting activity in the affairs of life. (3) Language has the function not only of communicating one's experiences to others, but also of expressing one's own past to one's present: it "is not the essence of thought," though without it thought would be "gravely limited" (p. 49). (4) "Understanding is not primarily based on inference," but rather is a matter of "self-evidence," though inference is a "means for the attainment of such understanding as we can achieve" (p. 69): the function of understanding is the twofold one of seeing things in their unity and in their relations to other things. Finally (5) we call attention to Professor Whitehead's emphasis on the superficial character of sense-experience as revelatory of the nature of reality, notwithstanding its prominence in our conscious lives and its practical importance as a stimulus of activity.

The behaviorist, we see, will gain little consolation from the reading of this volume. If my exposition of its contents seems somewhat scrappy, this is a reflection, unless I am very wrong in my interpretation, of the disconnected structure of the work. As previously intimated, only Part III, containing the two Chicago lectures on *Nature and Life,* reveals any very clear unity and continuity of theme, and the book as a whole is really two (or even, perhaps, three) books and an epilogue. The final chapter constitutes a quite charming *envoi,* but it is hardly more.

Western Reserve University JARED S. MOORE

Psychological Foundations of Personality. By LOUIS P. THORPE. New York, McGraw-Hill Book Co., 1938. Pp. xvi, 593.

It is a rare week that does not bring a publisher's circular heralding the latest and most scientific (and therefore the best) book in this or that special field of psychology. Not a few of the brethren seem willing to affix their rubber stamps of approval to these circulars, though most of the books so certified, as the reader is disheartened to discover, are dilutions of dilutions of old wine. This *camaraderie* among psychologists does a service, possibly, to the cause of humanitarianism, but a disservice, certainly, to the cause of psychology. Perhaps the universities, pressing the notion that 'productive scholarship' should be the one criterion for preferment, are responsible. Anyhow the truth is there are too many undifferentiated duplicates on the market; and in my opinion an academic man should be rewarded for *not* adding to the surplus. (Just as the Government has seen fit to pay farmers for not raising hogs.) Having recently committed the offense of writing a book myself, I am entitled to express this view—though not without a blush.

As it happens there are not so many academic books whose title includes the word "personality," and one hopes for the best in opening a volume which the author describes as an attempt "to organize, to refine, and occasionally to re-interpret the many references to personality matters" ("matters" here makes one bristle) that have appeared in articles and books on other topics. Unhappily it is exactly in these respects—upon which the success of a book that offers no new material must hang—that the attempt collapses. Beyond the headings of the different sections, this reader, at least, failed to find any appreciable organization, refinement or re-interpretation. The author has been as democratic generally as a Salvation Army-man, opening his portals to every theory or opinion, employed or unemployed, that happened to be straggling along the highway.

Vowing his allegiance to objective scientific psychology, Dr. Thorpe sets out to treat personality genetically, beginning with the superior amateurs, Mendel and Galton, and ending with a horde of inferior professionals. In the name of science the instinct theory is ejected at the front door, but in a moment its brother, the drive (or need) theory, is admitted at the rear. There is a full, though loose, discussion of the concept of traits, to which the author gives wavering assent; and a fairly complete account of paper and pencil tests of character and temperament. As usual the intelligence quotient receives prolonged consideration. Finally we come to the "wholesome personality."

The author of *Psychological Foundations of Personality,* being a professor of education as well as a practical therapist, is quite naturally interested in the "educability of personality;" and his book is advertised as a "handbook on the craft of inspired and purposive teaching." But there is no evidence that Professor Thorpe believes that "inspired" education and therapy must necessarily be based upon a philosophy, or system of values: matters which are still outside the province of objective scientific psychology. One must be honest about one's inspiration, and not use the masquerade of scientist while implicitly affirming moral preferences, personal or conventional. For one thing it leads to superficiality, which can be as misleading and lethal to the human spirit, as the worst fumblings of depth psychology. Perhaps Man is not such a riddle as we supposed. Perhaps the grievous mental ills of which he complains today are manageable. But only an irreclaimable optimist will believe that he can be fathomed by the unrelated categories or cured by the shotgun prescriptions which constitute this book.

Harvard University H. A. MURRAY

The Speech Personality. By ELWOOD MURRAY. New York, J. B. Lippincott Co., 1937. Pp. xii, 517.

This is a textbook and guidance manual for class and individual instruction in speech at the college level. A chapter is devoted to each of the following aspects of speech: (1) mental objectivity; (2) poise and emotional control; (3) spontaneity and emotional responsiveness; (4) audibility and distinctness; (5) vocal quality; (6) physical bearing; (7) gestures; (8) vocal flexibility; (9) enunciation, articulation, and pronunciation; (10) oral grammar and diction. These characteristics are classified as the *grosser speech skills.* They are antecedent to the development of the more refined speech techniques. Briefly, a speaker may be said to be mature and possessed of refined speech techniques when he is capable of leadership in speech situations and, also, when he is, by virtue of his speech skills, capable of making creative contributions to the social order.

The book appears to be a substantial contribution to the field of speech correction. The facts are presented clearly, the instructions and assignments for speech improvement are specific, and the practice exercises are well selected. A significant amount of practical, usable information is to be found in the chapter on enunciation, articulation, and pronunciation. The appendices contain outline sheets for history taking, personality improvement, hearing test, and voice analysis. In general, the more relevant aspects of speech correction are either included or conveniently cited so that the reader may have ready access to the sources. Although the book was mainly intended for class instruction and individual guidance, the clinical psychologist may find it of value in adult psychotherapeutic work whhere a speech defect is symptomatic of personality distortion. Unfortunately, its serviceability in such a direction is seriously handicapped by inadequate treatment of examination methods, an almost total neglect of clinical procedures, devoting but scant space to the specific speech disabilities.

It is our opinion that the title is not appropriate to the topic. It is neither easily nor correctly defined, nor is it likely to be accepted as convenient usage among those in the field of speech therapy, especially if they be psychologically oriented. According to Dr. Murray, the speech personality "is the personality both as built up and as expressed through speech." If there is a "speech personality" then why not an "athletic personality," a "singing personality," a "business personality," etc. The term is loosely used according to accepted psychological terminology. Its generic significance is lost in its specific applications.

Dr. Murray's opinion that "speech development parallels personality development" is necessarily hypothetical. Certainly there are too many exceptions to this generalization. There is, moreover, no clear-cut experimental evidence to support the statement. Under the circumstances, it might have been advisable to minimize the point of view on personality development and to have referred the reader to the accepted standard works on personality and adjustment.

The factors which make this book a good text are possibly responsible for preventing it from becoming a relatively permanent contribution to the field of speech correction. The style, which is not always felicitous, is dogmatic. There appears to be ever present the implication that the student is being instructed and that the instructor's opinion is *fiat*. This proves to be, as a matter of fact, a really good method of presentation: the reader is rather readily convinced, especially since the words carry an inspirational enthusiasm.

Rochester, New York ANTHONY J. MITRANO

Experimental Foundations of General Psychology. By WILLARD S. VALENTINE. New York, Farrar & Rinehart, 1938. Pp. xviii, 377.

This book is another offering in aid of the growing tendency to introduce beginning students to psychology by way of its experimental literature. The value of such a digest rests upon its general agreement with a particular user's own aims and problems in teaching his elementary course. The following quotations from the three introductory statements by the author indicate his plan of presentation. "In the selection of the material that appears here my criterion has been its utility to a young person who in all likelihood will have only one course in Psychology." Appeal to the student has been kept in mind to the end that "some relatively trivial experiments were included because they have proved over a period of years to be of interest to beginners." Definitions are avoided but the author is obviously concerned with factors that determine a person's conduct and these are roughly divided into "(1) a person's heredity," "(2) a person's biography" and "(3) a person's surroundings." This classification largely determines the interpretation of the various selections.

The body of the text consists almost wholly of re-written descriptions of experiments and interpretations of the results of investigations, with occasional excerpts from original articles. The selections embrace a broad conception of psychology as the study of behavior and are collected under such customary chapter headings of a textbook of psychology as Emotion, Perception, Learning, Remembering, Reasoning and Intelligence. Considerable space is given also to borderline topics such as Inheritance, Drives, Market Research, Hypnosis, Conflict. There is no attempt to be systematic or complete in the treatment of the whole field or of particular topics. The author states at the beginning that the student is not expected to get a "rigorous system of psychology," nor to be prepared "for an advanced course in any special branch of psychology." He will be interested at times and informed on many points psychological and otherwise. If such is the aim of an elementary course, the book answers that purpose. The material is well presented and sufficiently illustrated so that many instructors will find it useful supplementary reading.

Hobart College FORREST L. DIMMICK

The Rediscovery of Man. By HENRY C. LINK. New York, Macmillan Co., 1938. Pp. ix, 257.

This book portrays man as potential master of himself and his environment. In it the reader will find an extension of many of the ideas advanced in the author's *Return to Religion.* Personality is defined as "the extent to which the individual has developed habits and skills which interest and serve others." Link's measurement of it is predicated on four basic components: Habits of Self-Determination, Social Initiative, Economic Self-Determination, and Adjustment to the Opposite Sex. Within this frame of reference the author vigorously attacks a mechanistic concept of modern man as the helpless victim of forces revealed by the physical and social sciences. In his treatment he neglects none of the pluri-dimensional settings of personality be they individual, economic, social, recreational, religious, or political.

Many psychologists will probably neither approve nor agree with some of the statements contained in this book. They may object to such over-simplifications as: "Psychologists have found, however, that personality, far from being intangible, consists of definite habits and skills. These habits and skills can be acquired in the same way that people acquire the habits of reading and writing, that is, by practice and training." They may further protest at finding the solution to universal problems in development by the individual of correct habits. They will find unwarranted generalizations, will not be slow to point out that integrated personalities may greatly suffer from mis-directed attempts toward socialization, and will strongly resent the frequent labelling of personal opinion as fact discovered by Psychology.

The author, however, addresses laymen and not psychologists. Chapter I appeared last spring in the *Saturday Evening Post* under the title "Man in Chains" and presages the general content of the book. Two large classes of people form the author's main concern: those who, in effect, believe that 'by taking thought they can add a cubit to their stature' and those who blame their failure upon everything but themselves. With examples from case histories, he proceeds to demonstrate that there are no short cuts to the abundant life—that any solution may only be found through individual development of disciplined habit patterns. His plea is for the discovery and codification of the axioms of personality; "this book represents a step in that direction."

The author's prestige and forceful style should make this book effective in combatting the insidious paternalism with which some of our educational and governmental institutions are honeycombed. Employment counsellors who have not arrived at a stage of dangerous sophistification or become imbued with defeatism should find it valuable.

University of Maryland ALAN M. KERSHNER

A History of Philosophy. By B. A. G. FULLER. New York, Henry Holt & Co., 1938. Vol. I, pp. ix, 417; Vol. II, pp. 675.

Professor Fuller's *History of Philosophy* is one of the most readable ever written. Without pretending to add to the scholarship of the subject, it covers, in a vivid, modern, and, at times, intimate and racy style, twenty-five centuries of culture. Its graphic figures of speech and numerous analogies are drawn from all branches of knowledge and all walks of life.

This work does not contain formal sketches of the philosophers, which characterize many histories of philosophy, and footnotes and references are very rare. The few quotations that it does contain are not specific and refer to a volume rather than to a definite page. Yet the work is immense in size. At the end of the second volume there is appended for the convenience of the student a brief bibliography, a chronology, and a glossary of terms based on Lalande's *Vocabulaire de la Philosophie.*

While the author admits his leanings towards naturalism and materialism, he had no object in mind beyond writing an interesting history of philosophy. There is no attempt to treat thinkers out of their historical sequence in order to give a more logical development of the history of philosophy. Continuity is achieved by giving connecting links between thinkers and by introducing the thought of lesser philosophers, like Leonardo Da Vinci and Darwin, whose influence is usually relegated to footnotes or only casually mentioned and not featured as an essential part of the text.

The first five chapters, which are a condensation of the author's three volume *History of Greek Philosophy* are the most scholarly, and the section on medieval philosophy extending from the patristic philosophers to the fall of scholasticism is disproportionately brief. On the other hand, the accounts of Plotinus and Schopenhauer, which the author took over from his articles on the two men in the *Personalist,* are longer and fuller than is justified.

In addition to these lacks of balance, the work suffers from too many concessions to popular taste in style. For the psychologist who wants a readable history of philosophy rather than a handy reference book, this volume has, however, no equal.

Auburn, N.Y. SALVATORE RUSSO

L'angoisse et l'emotion. By RENÉ LACROZE. Paris, Boivin & Cie., 1938. Pp. x, 291.

This theoretical discussion of the evolutionary growth of the affective life presupposes that anguish, pain, or suffering furnishes the starting point for the whole range of feeling. Emotion is the generic name for the essential characteristic of this type of experience which develops into a host of variable phases depending upon the interplay of organic movements, the infiltration of ideas, and the expressions of the entire personality toward many environmental situations. Conflicts arouse many varieties of such emotional responses and thus occasion the criteria by means of which these experiences are practically or systematically classified on a qualitative basis, such as joys, disgusts, sadnesses and the like. In the ascending development of nervous patterns the discharge of ever more complicated, sometimes playful, activities are made possible. Finally the ideational functions provide further patterns of emotional experience, making new delineations permissible.

The book is provocative of critical thought and shows a comprehensive grasp of the literature on the subject. Its thesis deserves careful and serious consideration. The text is well documented. It is followed by an extended bibliography and an index of authors cited. The first part of the treatise deals with the pattern or structure of emotion; the second with the lesser or subordinate types of affective experience including the abnormal manifestations. The doctrine advanced by the author goes one step beyond the reviewer's phylogenetic theory in that vague uncomfortableness alone accounts for the beginning of the affective life, instead of starting with both pleasantness and unpleasantness as the sources.

Chicago, Ill. CHRISTIAN A. RUCKMICK

Guiding Human Misfits. By ALEXANDRA ADLER. New York, Macmillan Co., 1938. Pp. viii, 88.

To some readers, the sub-title of this book, "A practical application of individual psychology," will seem more fitting than the main title. Eighty-eight pages, although direct, condensed, and well written, is all too few pages in which to deal with the enormous social and psychological problem of guiding human misfits. The author has confined her efforts mostly to presenting material which she obtained while observing and treating patients. No attempt is made to review the abundant material upon the subject.

The book is written from a modified psychoanalytic viewpoint; the author duly acknowledging her indebtedness to her father, Alfred Adler, for much of the theory and methodology. Having studied neurology, psychiatry, and psychotherapy she has obviously had a rich background of practical experience from which to draw illustrative material.

Emphasis is placed upon the significance of early experiences and training. A few case histories are given to illustrate how neurotic tendencies in childhood may ultimately lead to definite neurotic and psychotic conditions in later life. Possible preventives and treatments are suggested.

While many psychologists will question the validity of the general approach, and consequently the interpretations and conclusions, yet, it is believed that even these will admit considerable sound judgment is indicated in diagnosing and treating individual cases. The book is apparently intended for practical use by those working with misfits. It is readily comprehensible to the layman and will undoubtedly prove of interest to a wide variety of people.

Berea College LAWRENCE M. BAKER

Mental Conflicts and Personality. By MANDEL SHERMAN. New York, Longmans, Green & Co., 1938. Pp. vii, 319.

In this book, Dr. Sherman brings to a focus upon the understanding of mental conflicts the points of view of psychology, psychiatry and sociology. Psychiatrists and psychologists have, in the past, frequently exchanged ideas upon this subject, but the views of the medically trained person have far too often been widely separated from those of the sociologist. The result has been that social and cultural factors in adjustment have tended to be lost sight of in psychiatric interpretation. This book goes far to remedy the defect. Due regard is given to the information which psychology has to offer, to the formulations of psychopathologists of various schools, and to the work of sociologists.

The study commences with a consideration of the genesis of mental conflicts. Both the innate drives and those of secondary origin receive recognition. The relation of attitudes to conflicts is then discussed. Next comes a more detailed study of the three main types of conflict. It is noteworthy that conflicts due to readjustment to new and varying cultural patterns are first considered, followed by conflicts of inferiority and insecurity, and eventually by sex conflicts. The last portion of the book relates the material concerning conflicts to, first, neuroses and, secondly, antisocial behavior in children. The balance between the three types of conflicts is maintained here, without the tendency to base every manifestation upon a single conceptual foundation, a tendency which is unfortunately far too common among those who deal with the complexities of human behavior.

Dr. Sherman has given us a lucid, readable book. While it contains no startlingly new ideas, it does present the material in a valuable new setting. His interest in children keeps him from going to the extreme of dealing only with adult manifestations and also from treatment the early period of life merely as the psychologist or psychiatrist wishes to remember it in an attempted projection of himself into it.

Milwaukee County Mental Hygiene Clinic GILBERT J. RICH

Introduction to Methods in Experimental Psychology. By MILES A. TINKER and KENNETH H. BAKER. New York, D. Appleton-Century Co., 1938. Pp. 222.

This is a spiral-ring-bound laboratory manual in psychology. Besides being bound in this inexpensive loose-leaf form the pages are perforated and punched for binding in the student's notebook. The material covered is modern, and is not limited to experimental psychology in the older sense but covers psychophysics, learning, including one experiment on animal learning, demonstration of the presence of various psychological traits such as "The Development of Concepts," "Estimation of Temporal Intervals," and "Measurement of General Ability." There are several experiments which are intended to bring out methods of statistical treatment of laboratory observations. The student is shown how to organize group data, to apply measures of variability, and there is some discussion of measurements of central tendencies, and other statistical material. The last eleven experiments deal with such topics and studies as "Musical Aptitude," "Reading Ability," "Art Appreciation," "Mechanical Ability," "Manual Dexterity," "Free Association," "Diagnostic Association," "Adjustment Inventory," "Self-Rating of Personal Traits," and "Individual Diagnosis by Means of the Profile." There are three appendices dealing with the terms and data needed for some of the experiments, and also some statistical tables. The last appendix consists of pictures for the experiment in "Judging Intelligence from Photographs." All the material needed for the study is present in this manual: the recording sheets, including cross-section paper, printed forms for tables, and other material for graphic and tabular recording are all present so that the student does not have to go outside of the manual for the materials in a course in experimental psychology.

Recorder's Court, Detroit, Michigan LOWELL S. SELLING

An Evaluation of Visual Factors in Reading. By H. A. Ismus, J. W. M. Rothney, and R. M. Bear. Hanover, N.H., Dartmouth College Publications, 1938. Pp. 144.

This monograph is a description of the diagnostic and remedial reading program at Dartmouth College. In Chapter I are stated the specific problems to be investigated. Chapter II gives an exact description of the eye examinations, the procedure with the eye movement photography, the aptitude and reading tests, and the criterion of academic success that were used. The interrelationships between the various tests are given in Chapter III. An excellent critical appraisal of the Opthalm-O-Graph is given in Chapter IV. The reliability of the eye movement photography is studied, as are the relationships with the other variables considered. The plan and instructional procedure are given in Chapter V together with an evaluation of the results. In a final chapter a summary is given and conclusions are cautiously drawn.

The mass of data which was obtained is clearly presented in a liberal number of figures and tables. The special tests that were used are given in an appendix, together with the more extensive tables.

This study can well serve as a model for programs concerned with the experimental evaluation of diagnostic techniques and methods in remedial reading. The completeness with which the authors present their data will be of great aid in helping other workers plan similar projects. The whole study is remarkable for the extreme care taken in working out methods and procedures, the completeness with which the data were analyzed, and the unwillingness to generalize beyond the findings.

University of Maryland EDWIN E. GHISELLI

—And the Stutterer Talked. By A. Herbert Kanter and A. S. Kohn. New York, N.Y., Bruce Humphries, 1938. Pp. 236.

Cast in the form of fiction, this book was written to and for the layman. The hero goes through many struggles in his search for normal speech. The origin of several cases of stuttering are also recounted with their course of development through unwise parental tutoring, ridicule from companions and teachers, and treatments at the hands of numerous and persistent quacks.

Stuttering is defined as hesitative repetitive speech characterized by clonic or tonic spasms. It is emphasized that stuttering is a psychological phenomenon; a manifestation of obsessions and inhibitions. Stammering, with which the book is not concerned, is defined as speech impairment due entirely to anatomical or organic abnormalities of the speech mechanism. This distinction, which is not usually recognized by psychologists nor by the Warren *Dictionary,* is held to steadfastly and perhaps has its value.

While frequently referring to the importance of a defective inherited nervous constitution, which is claimed to be characteristic of the stutterer, much stress is given to the significance of learning and habits. "Speech is an acquired attribute, never inherited . . . the mental aspects of the condition are acquired step by step through the years" (p. 177). It is said that an obsessive state of mind has been reached which resulted from a series of accumulated bad speech experiences, and "Cure depends solely upon the extent to which the obsession can be eradicated. This means complete rehabilitation of the individual" (p. 176).

In general, the book fulfills quite well the purpose of focusing attention upon the prevalence, the importance, and the lack of dependable treatment available to stutterers. Because there are no references, no indication of the amount or quality of the research upon which the conclusions are based, and a failure to isolate more adequately the various factors which presumably led to the successful cure, the book has limited scientific value for psychologists.

The authors were not attempting a scientific treatment, however, and judged as a popular book written upon an involved psychological subject, it is more acceptable than most of them.

Berea College L. M. BAKER

The Study of Human Abilities. By J. K. SHRYOCK. New Haven, American Oriental Society, 1937. Pp. x, 168.

A translation and critical analysis of a book dealing with human nature and human abilities, written between 240 and 250 A.D. by a retired Chinese official named Liu Shao, should be of interest to modern students of human nature. The Chinese title is the *Jen Wu Chih,* and the author presents the materials with which it deals in the preface. He says, "The sages admired wisdom more than all else. Of all wisdom, there is nothing more valuable than to know men, and those who know men are certainly wise." The title of *Jen Wu Chih* as translated is *The Study of Human Abilities.* This manuscript is in reality an analysis of the text through a translation, interpretation and application of the contents in the light of both the period in which it was written as well as present day knowledge.

"The *Jen Wu Chih* is primarily a work on the applied psychology of character. It deals with the abilities of men. These abilities are mental traits which can be observed only through their manifestations in behavior and Liu Shao might be called a behaviorist" (p. 39). It is such a treatise that Shyrock makes of this study. The book begins with a study of the history and philosophy of the three kingdoms, followed by a short biography of Liu Shao. The history and characteristics of the materials are given some consideration, followed by a presentation of the philosophy, morals, science, psychology and conceptions of the state of Liu Shao as revealed in his writings. The analysis of *Jen Wu Chih* makes up the major portion of the book, and it is this part that the reader may find repetitious, and at points confusing, due to the continuous analysis and classification of all human abilities and characteristics. These materials reveal, however, the nature that Liu Shao used in analyzing man. The book would be more interesting to the reader if some of these materials had been presented more generally in a descriptive manner, rather than as definitions or in an analytic fashion. This study should certainly serve to give a more complete insight into the philosophy of this period in Chinese History.

North Carolina State College K. C. GARRISON

Introduction to General Psychology. By JAMES B. STROUD. New York, Prentice-Hall, 1938. Pp. xv, 681.

In a day of popularizations, this general textbook is distinguished by the inclusion of basic material of a type that the fashionable text often omits. Thus, chapters on the neuro-muscular system, sensory processes, work, fatigue, sleep, and pleasantness and unpleasantness, present experimental results which appear, refreshingly enough, to have been selected for educational rather than entertainment value.

It is unfortunate, therefore, that the potential usefulness of a presentation of a wide range of information has been impaired by a lack of clarity and organization. One may seriously question whether a student could obtain a balanced and informed concept of the general field of psychology from data which are often scattered through the pages with little attempt at interpretation and analysis. While the selection of many of the topics suggests a functionalistic bias on the part of the author, the tendency never serves to integrate the great mass of material. When interpretations are offered they too often consist in naming a theory or viewpoint and presenting a somewhat hazy portrait of its proponent. Other interpretations are sometimes proffered in complete isolation from the remainder of the material, a procedure which leads to the inconsistent use of undefined terms.

In short, this attempt has more of the characteristics of a handbook than of a general text. Its usefulness as a handbook is destroyed, however, by the frequent employment of secondary sources, and by many misinterpretations and murkily written sections.

The quality of the printing, binding, and indexing of the volume is consistent with the carelessness of its written construction.

Tulane University S. RAINS WALLACE

Kleine Logik der Geisteswissenschaften. By FRANZ SCHMIDT, Munich, Ernst Reinhardt, 1938. Pp. 128.

Those who have followed the philosophical development of German science before the present "extirpation of the German mind in favor of the German race" (a development predicted by Nietzsche in 1871) must regard this little book as a tragic rudiment of a great work. It is concerned with the logic of the mental sciences—the development of which started with Hoeffding and Dilthey. Schmidt's book is a survey of the persisting problems; it offers little that is new. Maintaining that psychology has not fulfilled the promise to give a basic interpretation of the mental sciences, Schmidt calls upon logic and tries to find the basic principles in the notion of co-intention, a concept which goes back to Husserl's intentional philosophy. The discussion is divided into three parts: "The Mental Experience," "The Order of the Mental World," and "The Border of the Mental World." Of most interest to the psychologist is Schmidt's logical interpretation of the psychological types and of the rôle of typology (p. 58). The most original part of the book is his interpretation of freedom (p. 96).

Washington, D.C. ERNST HARMS

Psychology and the Nurse. By FRANK J. O'HARA. Philadelphia, W. B. Saunders Co., 1939. Pp. 252.

This book contains material touching upon the sense organs and sensation, dreams, memory, emotion, conflicts, the art of thinking, attention, tests and measurements, personality, and mental disorders. The writing is simple and the analysis superficial. Applications of each topic to nursing are discussed and a section at the end of each chapter is devoted to questions for review.

The author tends to use the definite article too freely, to devise questionable terms, to use others in a slipshod manner, to employ obsolete categories, and in general to oversimplify the discussion.

University of Maryland LESTER P. GUEST

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INTELLIGENCE LEVEL AND OCCIPITAL ALPHA RHYTHM IN THE MONGOLIAN TYPE OF MENTAL DEFICIENCY

By GEORGE KREEZER, Cornell University

The study reported here is one of a series undertaken to determine the empirical relation between variations of intelligence and properties of the electro-encephalogram.[1] The term electro-encephalogram (E.E.G.) refers to the record that may be obtained of fluctuations of electric potential of cerebral origin by means of electrodes attached to the intact scalp. The report by Berger in 1929 that such electrical potentials may be detected without the need of operative procedures opened the way to a wide variety of studies upon the cerebral basis of psychological phenomena.[2] The present series of studies is concerned with the application of this technique to the investigation of cerebral factors on which intelligence may depend. The first step in determining the utility of the electro-encephalographic technique as an index of such cerebral factors would seem to be the determination of whether or not significant correlations may be found between intelligence and properties of the electro-encephalogram.

In the investigation of this question the feebleminded offer important methodological advantages: a wide range of variation in intelligence level, and the possibility of independently controlling the variables of mental age and chronological age. In the use of the mentally deficient in this

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[1] From the Research Department, Vineland Training School, and the Psychological Laboratory, Cornell University. This investigation was conducted during 1937 at the Vineland Training School. It was assisted by Grants in Aid from the Penrose Fund of the American Philosophical Society and from the National Research Council. Appreciation is expressed to Mr. Franklin W. Smith, laboratory technician, for his assistance in this study.

[2] For a discussion of the logical basis involved in the use of the E.E.G. as a psychophysiological indicator and for a survey of recent literature, see G. Kreezer, The electro-encephalogram and its use in psychology, this JOURNAL, 51, 1938, 737-759.

series of studies, it has been considered advisable to deal with different clinical types in separate investigations. Such a procedure may be expected to decrease the danger of heterogeneous material masking and so preventing detection of any real relations that may obtain for any given type. The present study limits itself to the mongolian type of mental deficiency.[3]

One of the chief types of pattern exhibited in the human electroencephalogram is that which has been designated by Berger as the alpha rhythm.[4] It consists of rhythmical fluctuations of potential difference occuring at a frequency of about 10 waves a second, though with a certain range of variation in frequency for different individuals. This rhythm is generally most prominent at the occiput. In the present study, the specific problem undertaken was to determine what significant correlations obtained, if any, between intelligence level and certain properties of the occipital alpha rhythm in the mongolian type of mental deficiency. Reports of preliminary studies by the writer upon this question and incomplete reports upon the present investigation have been made in a number of earlier papers.[5] A detailed statement of experimental conditions, statistical treatment, and results has not, however, been made previously.

I. EXPERIMENTAL CONDITIONS

The conditions involved in the present investigation may be conveniently summarized under the headings of subjects, apparatus, technique, analysis of records, and statistical procedures.

(A) Subjects. The *Ss* in the experimental group consisted of 50 individuals of the mongolian type of mental deficiency. These individuals were carefully selected

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[3] For a summary of data on this type of mental deficiency, see K. Brousseau, *Mongolism*, 1928, 1-210.

[4] H. Berger, Über das Elektrenkephalogram des Menschen, I, *Arch. f. Psychiat, u. Nervenkr.*, 87, 1929, 527-570.

[5] G. Kreezer, The electric potentials of the brain in the mongolian type of mental deficiency, *Psychol. Bull.*, 33, 1936, 607; Electric potentials of the brain in certain types of mental deficiency, *Arch. Neurol. u. Psychiat.*, 36, 1936, 1206-1213; Electrical phenomena of the brain among the feebleminded, *Proc. Amer. Ass. Mental Deficiency*, 42, 1937, 130-141. Other reports on the E.E.G. of the mentally deficient have been made by Berger (H. Berger, Über das Elektrenkephalogram des Menschen, *Arch. f. Psychiat. u. Nervenkr.*, 94, 1931, 49 f.; 98, 1932, 250-254) and, quite recently, by Rahm and Williams (W. E. Rahm and A. C. Williams, Aspects of the electroencephalogram in epilepsy and feeblemindedness, *Psychiat. Quart.*, 12, 1938, 230-235). Berger obtained exploratory records on a total of about 10 *Ss*. There is no indication that any of the *Ss* was of the mongolian type. In the interesting study of Rahm and Williams, a group of 35 unselected *Ss* were used, only two of whom were mongols. Since the bulk of the *Ss* were apparently of the undifferentiated type, the results of the study by Rahm and Williams may be more appropriately discussed in a forthcoming paper of the author's on the relation of intelligence level and the E.E.G. in the "non-differentiated familial type" of mental deficiency.

from the population of the Vineland Training School and the Vineland State School[6] to meet certain criteria with respect to evidence of mongolism, chronological age, and absence of certain pathological traits. Details concerning the criteria used and the composition of the group with respect to mental age are given below.

(1) Chronological age. *Ss* below 16 yr. of age or greater than 56 yr. of age were excluded from the experimental group. The upper age limit of 56 yr. was arbitrarily adopted as an aid in the exclusion of *Ss* who might have suffered pathological brain changes associated with senility. The lower limit of 16 yr. was adopted for two reasons: (1) to exclude possible effects of differences in chronological age on the alpha rhythm of the E.E.G.; and (2) to keep constant the factor of chronological age in intelligence level.

The only property of the alpha rhythm that has been reported as varying systematically with chronological age has been alpha wave frequency. This property has been found in normal children to increase as chronological age increases up to an age of between 8 and 11 yr., at which point it reaches the value characteristic of adults.[7] The adoption of a lower age-limit for our experimental group which is well above 11 yr. can be assumed to effectively exclude any influence of chronological age on alpha frequency.

Two indexes of intelligence level, derived from the Binet type of intelligence test, have conventionally been used by psychologists: the mental age—as an index of absolute level of achievement in the test, and the intelligence quotient (I.Q.) as an index of level of achievement relative to chronological age. In the computation of the I.Q. for adults, it is customary to take as the value for chronological age a standardized value not greater than 16 yr.[8] Consequently, the restriction of our experimental group to *Ss* older than 16 yr. means that the I.Q. of different *Ss* will be proportional to their mental ages, and independent of differences in chronological age. The classification of our *Ss* in terms of absolute mental age-level will, therefore, correctly represent their distribution in terms of intelligence quotient as well.

The larger proportion of the *Ss* in the experimental group fall between chronological ages 16 and 35 yr. The distribution in the total group was as follows: 16 to 25 yr., 27 *Ss*; 25 to 35 yr., 17 *Ss*; 35 to 45 yr., 4 *Ss*—one in each of the M.A.-groups indicated in Fig. 2, except the 5- to 6-yr. M.A.-group; and 45 to 56 yr., 2 *Ss*: one, 47.7-yr.-old, in the 1.5- to 3-yr. M.A.-group, and one, 56.0-yr.-old, in the 5- to 6-yr. M.A.-group.

(2) Criteria of mongolism. One of the principles employed in the current series of investigations of the relation of intelligence level to the E.E.G. has been that of dealing with different types of mental deficiency in separate groups. By this means we hoped to increase the chances of homogenity for any given experimental group,

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[6] The Vineland Training School is a private institution for the care of mentally deficient individuals of both sexes. The Vineland State School is a New Jersey state institution for the care of mentally deficient girls. We are indebted to Mr. George W. Thorn, Superintendent of the Vineland State School, for his coöperation in this study.

[7] R. J. Smith, The electroencephalogram during infancy and childhood, *Proc. Soc. Exper. Biol. & Med.*, 36, 1937, 384-386; The electroencephalogram during normal infancy and childhood: II. The nature of the growth of the alpha waves, *J. Genet. Psychol.*, 53, 1938, 455-469; D. B. Lindsley, Brain potentials in children and adults, *Science*, 84, 1936, 34; Electric potentials of the brain in children and adults, *J. Gen. Psychol.*, 19, 1938, 288-291.

[8] R. Pintner, *Intelligence Testing*, 2nd ed., 1931, 84 f.

and to minimize the possibility of any real relations between the E.E.G. and intelligence level being obscured by the fact that different relations might conceivably obtain in different types of mental deficiency. As an extension of this principle, it was considered desirable to adopt some means of ensuring the selection of *Ss* all of whom could be regarded as good instances of mongolism. In most institutions, individuals may be found who might be regarded as representing intermediate or transitional forms of mongolism. Such individuals look like mongols in some respects, while in others they do not, and it becomes relatively difficult to state whether or not such individuals are to be classified as mongols.

To help exclude such 'intermediate' cases of mongolism and assure inclusion of only relatively marked cases of this clinical type, the following criteria were adopted: (1) the individual must have been designated as a mongol by the clinical staff of the institution of which he was a resident; and (2) he must show six or more of a set of 16 traits found in a study of Doll and Kuenzel to be most symptomatic of mongolism.[9] These 16 traits were: almond eyes, epicanthus, puffy eyelids, flat occiput, depressed nasal bridge, drooping mouth, thick lips, lips transversely fissured, dry facial skin, fissured tongue, thick tongue, pointed tongue, broad tongue, short fingers, dry nails and habitual use of "tailor posture" in sitting (hyperflexibility of joints). The examination of the various *Ss* with respect to these traits was made by the author of the present paper.

Since the report of the study by Doll and Kuenzel has not yet been published, it may be found helpful to state here those results of their study which suggested the use of the above criterion. In a survey of the literature of mongolism, these authors found a total of about 100 traits which have been reported as associated with mongolism by various writers, and a considerable lack of parallelism in the traits mentioned by different writers. They, therefore, set out to determine empirically which of this large set of traits could be considered most symptomatic of mongolism. Two groups of 30 individuals were selected, one group of mongols, and a control group of non-mongolian mental defectives, in which the individual *Ss* were equated for chronological age, life age, and sex with the mongolian group. These *Ss* were rated for presence or absence of the 100 odd traits by each of 4 judges familiar with the mentally deficient. The original list of traits was then reduced to 16 by the retention of only those traits which were found in 50% or more of the mongols and absent in 90% or more of the control *Ss*, by a total of any 3 out of the 4 judges. This group of 16 traits is listed in the preceding paragraph and was adopted in this study for rating of the mongols. Using this set of 16 traits, Doll and Kuenzel had the 4 judges rescore all the *Ss* in the mongolian and control groups in order to determine, among other things, the number of these 'mongolian-traits' which were present in all individuals of each of the two groups, as determined by agreement of 3 out of the 4 judges. They found, by this criterion, that every one of the mongols had 6 or more of the 16 traits, and that every one of the control-*Ss* had less than 2. On this basis, we have adopted the procedure of requiring all *Ss* in our experimental group to show six or more of the 16 'mongolian-traits' in order to be included in the group.

It is of interest that those *Ss* whom we found it necessary to drop from our experimental group (2 out of a total group of 52), on the basis of this criterion, were also those who seemed less distinctly of the mongolian type on the basis of total general impression. This selection of *Ss* for the experimental group was, of course, performed before an E.E.G. was taken for any given individual. The average number of mongolian-traits per individual for the total group was 8.9, with a range from 6 to 13.

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[9] Private communication. Appreciation is expressed to Dr. Edgar A. Doll for making available to us the results of his investigation.

(3) Pathological defects. All individuals who were blind, epileptic, or crippled in any way were also excluded from the experimental group.

(4) Sex. *Ss* of both sexes were included in the experimental group. The mongolian population of the institution was not such as to permit a constant proportion of each of the two sexes to be selected at each mental age-level. Statistical devices were therefore employed to adjust for any possible influence of sex upon the results. These are considered in the section on statistical techniques. The number of individuals of each sex at each of the mental age levels was as follows: M.A.-group 1.5 to 3 yr.: 2 males, 10 females; M.A.-group 3 to 4 yr.: 1 male, 10 females; M.A.-group 4 to 5 yr.: 4 males, 8 females; M.A.-group 5 to 6 yr.: 4 males, 6 females; M.A.-group 6 to 7.5 yr.: 3 males, 2 females; and total experimental group: 17 males, 33 females.

(5) Intelligence level. The intelligence level of all the *Ss* in the experimental group was measured by the 1915 form of the Stanford-Binet scale, supplemented by the Kuhlman-Binet for mental age test-levels under 3 yr. The mental age-scores for all individuals were on file as part of the regular clinical records of the two institutions. In cases in which a Binet mental age-score based on full-scale testing of the *S* had not been obtained within a period of 2 yr. prior to the taking of the electro-encephalogram, the *S* was retested by a member of the staff of the psychological clinic[10] within a period not greater than 0.4 yr. (5 mo.) after the taking of an electro-encephalogram.

In the case of all but 4 *Ss*, the chronological age of the *S* at the time of the intelligence test was greater than 16 years. In the case of these 4 *Ss*, the C.A. was between 14 and 16 yr., 2 of these being in the 3-4 yr. M.A.-group and 2 in the 5-6 yr. M.A.-group. The average interval between the Binet examination and the taking of the E.E.G. was 0.48 yr. for the group as a whole with a range between 0 and 2 yr. Since this interval was thus quite small, and in no case greater than 2 yr., and since available evidence indicates that only very small, if any, changes may occur in the M.A. of feebleminded *Ss* after a C.A. of about 14 yr.,[11] it was considered justifiable to use as the M.A. score for every *S* the score obtained at the time of the nearest Binet examination. These scores were thus regarded as correct at the time of the taking of the E.E.G., within the limits of errors of measurement of Binet mental age. The probable error of measurement of the M.A. scores obtained for the individuals in the experimental group may be assumed, on the basis of a recent study by Kreezer and Bradway,[12] to be of the order of 0.15 yr.

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[10] I should like to thank Dr. Anthony J. Mitrano and Miss Dorothy H. Basset, clinicians at the Vineland Training School and State School, respectively, for their coöperation in retesting subjects for whom retests were considered desirable.

[11] R. Pintner, *op. cit.*, 84 f.; E. A. Doll, *The Growth of Intelligence*, 1920, 1-130; F. Kuhlman, The results of repeated mental reëxaminations of 639 feebleminded over a period of ten years, *J. Appl. Psychol.*, 5, 1921, 195-224; L. Moore, Mental growth of low grade feebleminded, *Training School Bull.*, 26, 1929, 88-95.

[12] G. Kreezer and K. P. Bradway, The direct determination of the probable error of measurement of Binet mental age, *J. Educ. Research* (in press).

In estimating the extent to which changes in M.A. may occur after a chronological age of about 14 yr., 2 lines of evidence are relevant: (1) the studies indicating the most appropriate chronological age to be used in the calculation of I.Q. of adult *Ss*; and (2) the studies of changes in M.A. as a function of chronological age in feebleminded individuals. Pintner concludes, from a consideration of available evidence, that it is best to use 14 years as a basis for calculation of I.Q. for all individuals above this age.[13] The implication is thus that, on the average, growth in mental age stops at about 14 years of age. Special studies on the growth of intelligence in the feebleminded have been made by Doll, Kuhlman, and Moore.[14] Doll's study indicates that mentally deficient *Ss* whose final mental age does not exceed 8 yr. do not continue to show a significant growth in mental age beyond a chronological age of 12 yr., and that the age at which growth in mental age stops is still lower for the lower levels of the feebleminded. Moore's study of idiots confirms Doll's conclusion in indicating that the mental age of idiots ceases at a very early age, possibly under 10 yr. of age. Kuhlman concluded that the age of final arrest occurs somewhere between 15 and 18 yr., the lower grade groups tending to cease development earlier than the groups of higher intelligence level. The tables and graphs given by Kuhlman for the idiot and imbecile levels, which are the levels represented in the present study, indicate that the rate of increase beyond 14 yr. is extremely small, the maximum in any of the year-intervals represented being less than 0.16 yr. change in mental age in a single chronological year.

As a check on the correctness of our assumption that possible changes in M.A. occurring between the administration of the Binet scale and the taking of the E.E.G. record might be regarded as negligible and within the limits of errors of measurement, an effort was made to estimate the magnitude of the M.A. change that might have occurred in the interval. Kuhlman's data, as given in his Table I was used as a basis,[15] since these data suggest some possibility of significant M.A. changes at the chronological age levels represented by our *Ss*. In making these calculations, an effort was thus made to assume the worst possible situation with respect to the occurrence of possible errors. The procedure used in making these calculations consisted in first, determining from Kuhlman's Table I the increment (or loss) in average mental ages at successive chronological ages, for the idiot and for the imbecile groups. Kuhlman's data shows that for ages beyond that at which mental age-growth ceases, there may be slight losses in mental age-level. Ages greater than those given by Kuhlman were assumed to be associated with losses of the magnitude associated with the oldest age class given by him. On the basis of these data, the possible M.A. change that might have occurred for every subject in our experimental group, in the interval between M.A. examination and E.E.G. record, was calculated.

These changes we may refer to as "hypothetical interval errors." The average absolute magnitude of these "hypothetical interval errors" was 0.05 yr. with a range from 0 to 0.46 yr. Both plus and minus values were represented. In the case of only one *S* was the relation of the *S*'s M.A.-score and the hypothetical error such as would have led to the displacement of the *S* from one class interval to another in the scatter diagram used in calculating coefficients of correlation. In the case of this *S*, the hypothetical error was —0.46 yr., the largest error found, and if assumed to be correct, it would lead to the displacement of the *S* from the 2.5 to 3.0-yr. class-interval to one class interval below. The effect of such a displacement on the coefficient of correlation of alpha index and M.A. would be to raise it very slightly, from a correlation of 0.31 to 0.32, a change that may be regarded as negligible. Equally negligible effects would be produced on other correlations. If we now consider the magnitude of the "hypothetical interval errors" indicated above in relation to the probable error of measurement of 0.15 yr. suggested earlier, it is apparent that they will fall within the limits of the errors classified as accidental errors of measurement. These considerations suggest, therefore, that the "hypothetical interval errors" involved in the measurements of M.A. fall within the limits of experimental error of measurement and that they do not lead to any appreciable effect on the results.

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[13] Cf. footnote 11.
[14] Cf. footnote 11.
[15] Kuhlman, *op. cit.*, 199.

(B) Apparatus. The procedure involved in the recording of the electro-encephalogram is in principle simple. Extremely weak currents, generated presumably in the cerebral cortex of an individual are picked up by electrodes attached to the *S*'s head and transmitted to an amplifier to there produce fluctuating currents of like form but increased magniture. These magnified currents are conducted through an oscillograph and the form of the fluctuations recorded. The essential parts in the system are the electrodes with attached leads, the amplifier, and finally the oscillograph. Additional devices were used for purpose of calibration and exclusion of possible artifacts. This necessary equipment consisted of a photo-cell oscillator with attenuator for calibration of amplifier-oscillograph system, a filter for reducing the amplitude of muscle action potentials in certain subjects, and a 'Faraday cage' for shielding *S* from electrical currents of external origin. The details of the equipment used are given below.

(1) Electrodes. The electrodes used were of two kinds, the active or head-electrode, and the reference or ear-electrode. The head electrode consists of a flat coil of silver wire, 1 cm. in diam., lying on the under side of a small piece of sponge rubber which was held in place on the head by means of an elastic head band. Contact between the *S*'s skin and the silver coil was provided by means of saline electrode paste (Redux-Sanborn Electrocardiogram Co.). The rubber sponge over the electrode bulges down around the edges of the silver coil and thus provides a pocket within which is held the electrode and paste covering it. The ear-electrode consisted of a small rectangle of pure silver foil, bent over in a U-shape so as to make contact with both sides of the ear-lobe, and held in place by a spring ear-clip. Contact between the silver-surface and the skin was here too provided by means of the electrode paste. An electrode of this sort was attached to the lobes of both ears and connected together by wire leads to jointly provide the reference electrode.

The elastic head gear used for holding the active electrodes in place consisted of an elastic band attached around the circumference of the head (corresponding to a latitude-meridian of a globe) and two bands at right angles to the first (which corresponded to longitude-meridians). The length of all these bands may be adjusted to fit heads of different sizes, and the position of the 'longitude-bands' adjusted so an electrode may be located readily at any position on the head. The tension of the various bands was such as hold the electrodes stably in place without being great enough to be uncomfortable to the subject.

(2) Amplifier. The amplifier consisted of 5 stages, resistance-capacity coupled. A circuit diagram is shown in Fig. 1. The first stage is of the push-pull, balanced type, similar to that described by Matthews,[16] though with somewhat different circuit-constants. It was built as an independent unit, enclosed with its own batteries in a shielded metal case. It may be referred to as the pre-amplifier. The next three stages form a second unit, contained in a shielded metal case. This unit may be referred to as the voltage amplifier. The final, or power stage, forms a third physical unit, with separate batteries. Connections between the separate units are made by means of shielded microphone cable, and telephone plugs and jacks.

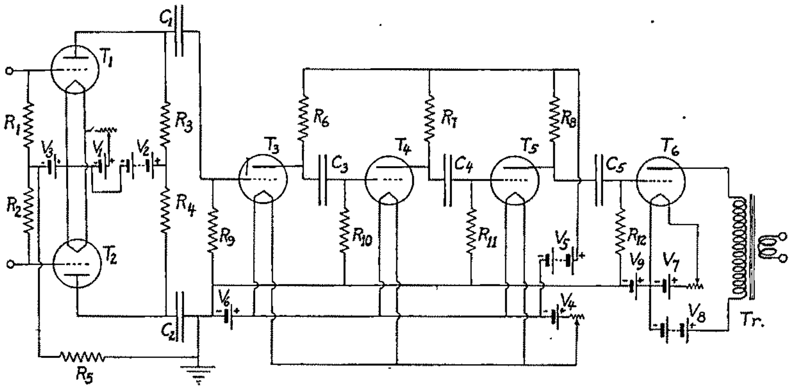

FIG. 1. CIRCUIT DIAGRAM OF AMPLIFIER

$R_1 = R_2 =$ 100,000 ohms
$R_3 = R_4 =$ 10,000 ohms
$R_6 = R_7 = R_8 =$ 150,000 ohms
$R_9 =$ 250,000 ohms
$R_5 = R_{10} = R_{11} = R_{12} =$ 1 megohm
$C_1 = C_2 =$ 4 μFd

$C_3 = C_4 = C_5 =$ 0.1 μFd
$V_1 =$ 2 volts (storage cell)
$V_2 =$ 67 volts
$V_3 = V_6 =$ 1.5 volts
$V_4 = V_7 =$ 6 volts (storage battery)
$V_5 =$ 225 volts

$V_8 =$ 135 volts
$V_9 =$ 9 volts
T_1, T_2 : W. E. 264 B
T_3, T_4, T_5 : W. E. 102 G
T_6 : 112 A
Tr.: Transformer G. R. Co. No. 541 C

An essential requirement of the amplifier apart from the provision of sufficient sensitivity and a satisfactory frequency-response curve was that it be free from disturbances which might introduce artifacts in the records. The disturbances to be excluded were of three kinds: (a) induction effects, or disturbances arising from electrical sources external to the amplifier; (b) microphonic effects, or disturbances produced by sound waves striking the amplifier tubes; and (c) internal noise-effects, or disturbances arising within the amplifier itself.

(a) Induction effects. Induction effects were eliminated through the use of adequate shielding for *S* and amplifier. The most serious source of disturbance in this category was the 60 ~ house current. A 'Faraday cage' provided shielding for *S*. It was constructed by lining all sides of *S*'s room with galvanized iron mesh. Since this amount of shielding was not found adequate to exclude all traces of the 60 ~ current from the records when *S* was connected to the amplifier (due to the relatively

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[16] B. H. C. Matthews, A special purpose amplifier, *J. Physiol.*, 81, 1934, 28 P-29 P.

large capacity of *S* to ground), a second, smaller enclosure was built of fine copper mesh at one end of the room. This smaller enclosure was large enough to contain a cot for *S* and certain necessary apparatus. Additional shielding for the pre-amplifier and voltage amplifier units was provided by the metal containers in which they were built. These two units were located just outside of the small copper-lined enclosure containing *S* (in order that the leads from *S* might be kept short). All metal shields were connected together and grounded at the same point.

(b) Microphonic effects. The Western Electric 264B tubes of the pre-amplifier stage are relatively non-microphonic. As an added precaution, however, the bases on which these tubes were mounted were suspended from overhead brackets by means of springs. More complete measures were found necessary to shield the Western Electric 102G tubes used in the voltage amplifier. These tubes, though of the low noise type are moderately microphonic. The measures taken consisted in placing the voltage amplifier inside a galvanized iron box lined on the inside with a 1-in. thick layer of air-acoustic,[17] a sound absorbing material, and in suspending this box with the amplifier by means of springs from the brackets supporting it. The relatively large mass of the box gave it, in conjunction with the supporting springs, a low natural period of vibration (about one a sec.). Mechanical vibrations of higher frequency which were transmitted to it tended accordingly to be damped out. Tests showed that speech of moderate intensity in the immediate vicinity of the amplifier failed to produce microphonic effects.

(c) Internal noise. Various factors within the amplifier tend to produce small fluctuations in voltage which are referred to collectively as the noise-level of the amplifier. This noise-level sets the lower limit of useful amplification since signals which do not exceed it will be obscured by the fluctuations in voltage represented by the noise-level. The following measures were used to keep the noise-level low.[18] (1) Low-noise tubes were used in the pre-amplifier and voltage stages. These tubes are designed to reduce the factors in the tube (shot-effect, ionization, etc.) which may contribute to its noise-level. (2) The tubes in the first stage were operated at a relatively low plate-voltage. (3) A relatively low input resistance of 200,000 ohms was used to keep fluctuations due to the 'thermal effect' low, inasmuch as the magnitude of the 'thermal effect' is proportional to the input resistance.[19] (4) Coupling condensers and plate and grid leak resistors were selected so as to minimize noise-production that may arise in these components. The ordinary type of paper condenser was used only in the last stage, mica or oil-impregnated paper condensers being used in the earlier stages. Wire-wound resistors (non-inductive) were used in the pre-amplifier and voltage stages, since other types are likely to be noisy. The metallized type was found satisfactory for the last stage. (5) Care was taken to provide well-soldered joints for all connections within the amplifier. (6) The storage batteries which supplied the filament-current were kept well-charged, and the dry cells used

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[17] This material was obtained from Johns-Manville Co. A layer 1-in. thick is reported as having a sound absorption coefficient ranging from one of 0.46 for a 128 cycle tone to a coefficient of 0.73 for a 2048 cycle tone (Bulletin No. VI of the Acoustical Materials Association).

[18] For a discussion of factors contributing to the noise-level of amplifiers, and procedures for reducing this level, see J. B. Johnson and F. B. Llewellyn, Limits to amplification, *Bell Syst. Tech. J.*, 14, 1935, 85-96.

[19] *Ibid.*, 85-88.

as B batteries were discarded when fall in output voltage indicated an increase in internal resistance. The best check on the dry batteries, was, however, a functional one. An increase in noise-level which was found not to be due to other factors could be eliminated by the substitution of fresh batteries. (7) In general, care with the early stages was most important inasmuch as disturbances arising at these stages are successively amplified by all later stages.

(3) Oscillograph. A Westinghouse PA bi-filar type oscillograph was used for recording. The unit used contains 3 galvanometer or 'vibrator' units, so that 3 records may be obtained simultaneously. One vibrator was used to provide a time-line, a second (standard type vibrator) was used to provide the records analyzed in this study, and the third vibrator (high sensitive vibrator) unit was used in conjunction with a second amplifier of modified design to record the very low frequency phenomena that may be present in the electro-encephalogram. In the records analyzed in this study, wave-frequencies below the alpha frequency range tended to be filtered out, as a consequence of the frequency-response curve of the amplifier, thus making it easier to analyze the records for different properties of the alpha rhythm. Records were made upon bromide paper (Eastman recording paper No. 697 or No. 797) moving at a rate of 22 mm. per sec.

The oscillograph is equipped with a polygon of mirrors and a viewing screen which permits the wave form of the phenomenon being studied to be observed visually without interfering with the taking of photographic records. This viewing attachment permits the nature of the potential fluctuations being picked up by the electrodes to be observed before a photographic record is taken. Artifacts such as an increased noise level, muscle potentials, or 60 ~ induction effects could, consequently, be detected, if present, and eliminated before a record was taken.

(4) Accessory devices: (a) Photocell oscillator.[20] A photo-cell oscillator was constructed for determining the frequency-response curve of the amplifier, that is, its relative sensitivity at different frequencies of input signal. In this type of oscillator, the form of the output current is regulated by the variations produced in the amount of light entering a slit in front of a photo-electric cell. A rotating disk placed between a light source and the slit can be cut in any desired shape and thus permit the form of the output current of the oscillator to be controlled. The frequency of the current fluctuations may be regulated by the frequency at which the disk is rotated. The use of a D.C. shunt-motor permitted this frequency to be readily adjusted. A gain control on the oscillator unit provided for adjustment of the output voltage.

The merits of this type of oscillator is that it makes available sinusoidal alternating currents or voltages in the low frequency range (for example, between 0 and 60 ~ a sec.) and that the amplitude of such alternating currents is independent of the frequency of the current fluctuations. It thus permits measurements of the relative sensitivity of the amplifier for the different frequencies in the low-frequency range (for example, between 5 ~ and 15 ~ a sec.) with which we are concerned in the present study.

(b) Attenuator. In order to reduce the output voltage of the oscillator to values of the range found in the electro-encephalogram, an attenuator was inserted be-

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[20] We are greatly indebted to Mr. Albert A. Grass, resident engineer at the Harvard Physiological Laboratory, for the design of this oscillator.

tween the output of the oscillator and the input terminals of the amplifier. Inasmuch as the attenuator was to feed into a high resistance of 200,000 ohms, it could be constructed on the simple voltage divider principle. One of the attenuator units used was made with precision resistors with resistance ratings correct to within 1 per cent. With a 1.5-v. dry cell used as a source of voltage, this attenuator provided D.C. output voltages of 6, 30, 60, 300, and 600 microvolts (µv.). These values were determined by calculation. With the dry cell voltage assumed to be constant, errors in the calculated values of output voltage may be assumed to be no larger than that of the resistors. Another attenuator unit was used to provide a larger number of voltage steps in the low voltage range. Resistors of the inexpensive metallized type were used to provide the larger resistance values in this unit. The accuracy of the output voltages obtained was checked, through use of amplifier and oscillograph, against the outputs from the attenuator constructed with precision resistors. Any differences were within the limits of error with which deflections could be read on the oscillograph-record. Output voltages available with this unit, for 1.5 v. input, were 5, 10, 20, 50, 100, and 1000 µv. With each of the attenuator units, the different attenuation settings could be made readily by means of a selector switch.

The calibration of the output of the attenuator units in terms of the microvolt values specified above was based on the assumption of a 1.5-v. D.C. source. In using the *oscillator* with the attenuators to provide alternating voltages of equivalent magnitudes, it was necessary for its gain control to be adjusted so that the amplitude of the alternating voltage output of oscillator was 1.5 v. This adjustment was made by connecting the 1.5-v. battery to attenuator and amplifier-oscillograph system, and determining the deflection produced by D.C. pulses, for a given attenuator setting. The oscillator was now substituted for the 1.5-v. battery, and its gain control adjusted until the same deflection (from center line to peak) was produced on the oscillograph. This setting of the gain control could then be assumed to correspond to an output voltage of 1.5 v. The use of this method assumes that the frequency of the oscillator signal used lies in the region of maximum sensitivity on the frequency-response curve. That is to say, the amplifier must be equally sensitive to the D.C. pulses and the A.C. frequency provided by the oscillator. Otherwise equivalence of deflection would not imply equivalence of output voltage. The frequency used for the oscillator signal was 30 ~, a value lying well within the frequency region of maximum response of the amplifier, as shown by the frequency-response curve. As a further check on the equivalence of sensitivity of the amplifier to the 30-~ sinusoidal frequency and to rectangular pulses, a disk was made which permitted the oscillator to produce rectangular pulses instead of sinusoidal pulses. With all other conditions the same, the deflections produced on the oscillograph by these two types of oscillator-signal was found to be equal, within limits of errors of measurement. In using oscillator and attenuator to obtain a frequency-response curve for amplifier, it was, however, not necessary for the absolute value of voltage of alternating current signal to be known. All that was necessary was that the same voltage signal be used at all frequencies.

(c) Filter. A few *Ss*, although apparently lying quietly showed fairly persistent muscle potentials. These potentials were due presumably to a failure of *S* to relax sufficiently the muscles about the neck and jaws. Muscle potentials are of relatively high frequency, of the order of 40 or 50 waves a second. Though there is no

danger of their being confused with the alpha waves, which are much lower in frequency, they tend to obscure the alpha waves and render their analysis more difficult. A 'muscle-potential filter' was, therefore, used with some *Ss* to attenuate frequencies in the range between about 25 and 65 ~ a sec. The filter used consisted of a condenser and choke coil in series connecting the two leads just before the power stage. At a value of 42 H for the inductance and 0.13 μFd. for the capacitance, a frequency of 47 ~ was attenuated maximally, with frequencies on either side attenuated in lesser amounts corresponding to a slightly asymmetrical damped resonance curve. Thus, if, with filter inserted, transmission of the amplifier at 10 ~ is designated as 100%; transmission at 27 ~ is 68%; at 37~, 47%; at 47 ~, 37%; at 57 ~, 44%; and at 67 ~, 64%. In the alpha frequency range between 7 ~ and 14 ~, there was practically no change other than some decrease in absolute sensitivity, *e.g.*, at 10 ~ a change from 9.6 μv. per mm. to 12.7 μv. per mm.

(5) Performance of amplifier-oscillograph system. The performance of the amplifier-oscillograph system may be summarized in terms of the absolute level of sensitivity, the form of the frequency-response curve, time constant, linearity of response, and noise-level.

(a) Sensitivity. The standard sensitivity used corresponded to a 1-mm. deflection on oscillograph for 9.6 μv. input, at a sine-wave frequency of 10 ~ a sec. When the muscle-potential filter was used, the general level of sensitivity was somewhat lower, a 12.7 μv. input being required per mm. deflection at 10 ~.

(b) Frequency response. The frequency response-curve of the amplifier shows that as the frequency decreases from 100 ~ per sec., the curve is flat down to a frequency just below 20 ~, at which point the sensitivity begins to fall off slightly. The slope of the curve in the alpha frequency range with which we are concerned in the present study is indicated by the following figures. The sensitivities given are in terms of microvolts input required per mm. of deflection: at 20 ~ and above, 9 μv.; at 15 ~, 9.2 μv.; at 14 ~, 9.2 μv.; at 13 ~, 9.3 μv.; at 12 ~, 9.3 μv.; at 11 ~, 9.4 μv.; at 10~, 9.6 μv.; at 9 ~, 9.8 μv.; at 8~, 10.3 μv.; at 7 ~, 11.3 μv.; and at 6 ~, 12.7 μv.

(c) Time-constant. The time-constant of the amplifier (time required for deflection due to a direct current pulse to fall to 37% of its value) is 0.012 sec.

(d) Voltage response. The deflections of the amplifier-oscillograph system were linear with respect to input voltage up to the maximal input voltage used in the tests, above 300 μv. Tests were made both with D.C. pulses, and with A.C. sinusoidal signals and rectangular pulses produced by the oscillator. Deflections were measured from trough to crest. The amplifier was thus linear in response well beyond the range of input voltages indicated by any of the records, the maximum shown by any record being 108 μv.

(e) Noise-level. The noise-level of the amplifier was regularly determined with input terminals short circuited. At the standard sensitivity specified above, the base-line is practically a straight line, only a slight roughness in this line representing the noise-level. Records taken with a more sensitive oscillograph vibrator showed that the maximum amplitude of fluctuations present in the noise level was about 5 μv.

To determine the noise-level with an imput impedance equivalent to that present when an *S* was included in the circuit, a pair of the silver-coil electrodes were attached to *S* on the skin over the tibia, at a separation of 5 cm. This region was selected as representing the region of the body surface most likely to be free of muscle potentials or other kinds of bio-electric potentials. The D.C. resistance between electrodes was 20,000 ohms, a value higher than the maximum shown by any of the *Ss* during the experiment with electrodes connected to the head. The base-line obtained under these conditions was not distinguishable from that obtained with amplifier input short-circuited. The noise-level obtained with input short-circuited may therefore be regarded as representing the level of internal disturbance present in the amplifier when the *S* is included in the input circuit.

(C) Technique. The standard sequence of procedures carried out with each *S* was the following: (1) preliminary tests of amplifier performance; (2) attachment of electrodes to *S*; (3) measurement of inter-electrode resistance; (4) showing of preparatory motion pictures to facilitate adjustment of *S* to recording situation, and other procedures for preparation of *S*; (5) taking of records; (6) final showing of motion pictures; (7) measurement of inter-electrode resistance and electrode locations; and (8) final tests of amplifier performance. Details concerning these procedures are given below.

(1) Tests of amplifier-performance. To provide evidence of proper performance of the apparatus during the taking of all E.E.G. records, certain standard tests were made at the beginning and end of each recording period. These tests consisted in obtaining record of the noise level of the amplifier, and in testing amplifier for sensitivity with a 50 µv. signal. Two types of standard signal were used, a D.C. pulse and a sinusoidal 10-~ A.C.-voltage from oscillator.

(2) Location of electrodes. Active electrodes were applied at each of three regions of the head: over the occiput, the motor area, and the anterior frontal area, slightly to the right of the median plane. The results given in this paper are limited to the records obtained over the occiput. The standards adopted relative to the location of occipital electrode were based on two considerations: Adrian and Yamagiwa's report on the region of maximal alpha activity at the occiput,[21] and our expectation of a variation in size of the head of the subjects in the experimental group.

Adrian and Yamagiwa have reported that the locus of maximal alpha activity at the occiput extends over an area and is not limited to a specific point. This area is said to extend approximately 2 in. in a cephalic direction from the inion, and 2 to 2½ in. laterally in both directions from the medial plane. It may be inferred that an electrode located at any point within this area will be equally effective in the detection of the occipital alpha rhythm. It seemed likely, however, that the absolute limits of this area of greatest activity would vary somewhat with the size of the head. The occipital electrode was, accordingly, not located at a fixed distance from the inion and from the mid-line of all *Ss*. An effort was made instead to adjust its location somewhat in accordance with apparent differences in the size of the head, without allowing it to fall outside the approximate limits given by Adrian as bounding the area of greatest alpha activity.

To permit subsequent statistical determination of the rôle of electrode position on the results, measurements were made at the end of each recording period of head dimensions and electrode location. These locations could be determined from the white spots left on the scalp by the electrode paste, the center of the spots being taken to represent electrode-position. To facilitate measurements, two coördinate axes on the surface of the head were assumed: an x-coördinate axis represented by lines parallel to the median plane, and a y-coördinate axis, represented by lines perpendicular to the x-axis.

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[21] E. D. Adrian and K. Yamagiwa, The origin of the Berger rhythm, *Brain*, 58, 1935, 323-351.

The following measurements of head-dimensions were taken (with tape measure): (1) distance from nasion to inion (nasion-inion distance); (2) the lateral distance across the vertex from left to right auditory meatus; and (3) the circumference of the head. Two measurements were sufficient to indicate position of any electrode: the distance from nasion to electrode along x-coördinate axis (nasion distance), and the distance from median plane to electrode along a y-coördinate axis (lateral distance).

On the basis of these measurements, each electrode-position was represented for purposes of comparison in terms of "absolute inion distance," "relative inion distance," "absolute lateral distance," and "relative lateral distance." These terms are defined as follows: (1) *absolute inion distance* equals the distance from inion to electrode—it was determined by subtracting nasion-distance of the electrode from the nasion-inion distance; (2) The *relative inion distance* equals ratio of inion distance to the nasion-inion distance; (3) the *absolute lateral distance* equals distance of electrode from medial plane; (4) the relative *lateral distance* equals the ratio of absolute lateral distance to one half the lateral distance between the two auditory meatuses. Statistical data on the relation of these aspects of electrode position to E.E.G. properties will be considered in the section on results.

(3) Inter-electrode resistance. Before attachment of electrodes, steps were taken to reduce the resistance of the skin at the proposed place of application. A little alcohol was used first to remove some of the skin-oils, and then a bit of electrode paste was rubbed into the skin with the tip of the finger. An extra dab of electrode paste was placed on the electrode before it was brought into contact with the skin.

The D.C. resistance between each of the active electrodes and the reference electrodes was measured by connecting in series with *S* a 1.5-v. dry cell and a milliammeter (1.5-ma. range), and sending a brief pulse of current through the circuit. Resistance could be taken as equal to voltage divided by current, on the basis of Ohm's law. The strength of current flowing was great enough to be easily read on the meter but usually insufficient to disturb *S* by stimulating cutaneous sense-organs.

The resistance varied from *S* to *S*, but was rarely greater than 10,000 ohms. In such cases in which the resistance was greater than that value, the electrodes were removed, and the place of application prepared by additional rubbing with electrode paste in an effort to reduce the resistance further. After the E.E.G. records had been taken, the inter-electrode resistance was again measured. The possible effects of differences in inter-electrode resistance on properties of the E.E.G. will be considered in the section on results.

(4) Subject-conditions during recording. In order to minimize special effects in the E.E.G. records which might be produced by varied physiological or psychological states in the subjects, an effort was made to establish standard conditions with respect

to muscular relaxation, elimination of visual stimulation, the removal of emotional excitement, and the avoidance of states of sleep or drowsiness.

(a) Muscle potentials. Muscular relaxation was considered desirable in order that the brain potential record might not be obscured by muscle potentials spreading from nearby muscle groups. Muscle potentials, exhibiting frequencies of the order of 50 to 60 fluctuations a sec., are in no danger of being mistaken for the much lower frequency alpha waves. They may, however, exceed the brain potentials in amplitude and thus interfere with their detection on the records. The only muscle groups found troublesome in this respect were those of the neck and the jaws. To facilitate relaxation of these muscles, the *S* was instructed to lie quietly on his back on a cot and to avoid mouth-movements during the recording period. If muscle potentials were still present, as observed in the viewing screen of the oscillograph, the *S* was observed for any apparent muscular tensions and instructed appropriately for the relaxation of particular muscles. In the case of a few *S*s, for whom residual traces of muscle potentials were observed even after these measures were taken, records were taken with the 'muscle-potential filter' inserted in the amplifier before the power stage.

(b) Visual stimulation. Elimination of visual stimulation was effected by having the subject-room completely darkened during the taking of a record. Visual stimuli tend to bring about elimination or reduction in amplitude of the alpha rhythm, but the effect is not equally marked in all individuals. It was felt desirable, therefore, to obtain records of the various *S*s when effects of visual stimuli were excluded. With individuals of a high enough mental age, visual stimuli can be most easily excluded by instructions to *S* to close his eyes. But among the lower grades of mentally deficient individuals used in this study, such instruction could not be relied on. A completely dark room was therefore used during the taking of records with all *S*s.

(c) Emotional excitement. There has been some evidence that during states of emotional excitement, a given individual will not necessarily show the same type of brain-potential record that he will when such emotional states are absent.[22] It was therefore felt necessary in obtaining our basal records to take precautions to avoid states of emotional excitement on the part of the *S*s, as represented, for example, by feelings of apprehension.

Factors which tended to prevent such feelings of apprehension arising in connection with the experimental situation were the fact that individuals in both institutions were generally well accustomed to going to the laboratory for tests of various kinds, and the fact that they had become acquainted with the *E* through visits of the latter to the various cottages. In preliminary studies, it was found, however, that some of the *S*s became somewhat apprehensive when the room was darkened. To provide a situation which would make it seem natural to *S* to lie down in the dark, provision was made for projecting motion pictures on a screen placed above the lower end of the cot, at an angle such that the pictures could be seen most comfortably when the *S* was lying down. When the *S* was brought from his cottage by *E,* he was told he was being taken to the laboratory to see moving-pictures. Consequently, when he was led into the experimental room, he would generally lie down on the cot without objection when told to do so in order to better see the motion pictures. After a 3-min. showing of a "Mickey Mouse" film, the projector was turned off and *S* told that if he would rest quietly for a short time, there would be more pictures later. If any body-parts seemed to be in uncomfortable positions or under excessive tension, a little adjustment by *E,* together with appropriate instructions, was usually effective in bringing about a more easy position. The E.E.G. records were then taken, and, at the end of the recording period, there would be another 3-min. showing of the film.

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[22] H. Berger, Über das Elektrenkephalogram des Menschen, VI, *Arch. f. Psychiat. u. Neurol.*, 99, 1933, 555-574; Recent studies of interest indicating an influence of emotional activity on the E.E.G. have been reported by Hoagland and co-workers (H. Hoagland D. E. Cameron, and M. A. Rubin, Emotion in man as tested by the delta index of the electroencephalogram, *J. Gen. Psychol.*, 19, 1938, 227-245, 247-261).

This procedure proved to be very effective in eliminating any externally observable signs of apprehension or unrest on the part of *S* in the experimental situation. It was a common occurrence for an individual, on being brought back to his cottage, to ask when he could come again to see the moving-pictures, or give other indications of having enjoyed himself. On subsequent visits of *E* to a given cottage, it frequently happened that individuals who had not yet served as *S* would ask to be taken to see the pictures. It seems quite safe to conclude that the records obtained were not distorted by effects produced by emotional unrest due to the experimental situation.

(d) Sleep. Investigations by a number of workers have demonstrated that as an *S* gradually falls asleep, distinct changes may appear in the electro-encephalogram. In the earliest stages of sleep, the change consists in a dropping out of the alpha waves, and there are signs that some diminution in amount of alpha activity occurs in even the preliminary drowsy stages of sleep.[23]

It would seem desirable, therefore, to have some assurance that the *Ss* were not asleep during the taking of the records. We know of no way of obtaining an index of the degree of drowsiness of an individual, in the absence of an introspective report. The procedure used in obtaining the standard sequence of records for a given *S* was, however, of a sort to interfere with his falling asleep, and permitted the experimenter to observe whether there were any signs of *S* becoming sleepy. The various samples of electrical activity, obtained with *S*'s room dark, lasted each no more than 60 sec. After these intervals, a moderate light was turned on in *S*'s room, and *E* would enter for adjustment of the electrode switch which connected the electrodes attached to different regions of the head. During this switching period, *S* was talked to and observed, and signs of being awake elicited. The records obtained may therefore be assumed to be representative of the waking state of the various *Ss*.

(5) Recording procedure. The Westinghouse oscillograph on which records were taken was located in a second dark room immediately adjoining *S*'s room. Through the use of the viewing screen, *E* was able to determine the nature of the electrical

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[23] H. Berger, Über das Elektrenkephalogram des Menschen, III, *Arch. f. Psychiat. u. Nervenkr.,* 94, 1931, 16-61; A. L. Loomis, E. N. Harvey, and G. A. Hobart, Cerebral states during sleep as studied by human brain potentials, *J. Exper. Psychol.,* 21, 1937, 127-144; H. Blake and R. W. Gerard, Brain potentials during sleep, *Amer. J. Physiol.,* 119, 1937, 692-703.

activity being picked up by the electrodes. If muscle potentials were present, an effort was made to get *S* to relax more completely before a photographic record was taken. After each record, a light was turned on in *S*'s room, the electrode switch adjusted, and another record taken from an electrode located over another region of the cortex. The sequence of these records was: occiput, motor, frontal, and occiput. The first 3 records were taken with *S* in the dark during the entire period. In the course of the last record, after about 10 sec. had elapsed, a stimulus-light was turned on for 1 sec., and then after another 10 sec. interval, turned on for 1 sec. again. If a considerable amount of muscle potential was observed in the viewing screen during any record, it was repeated, after *S* had been observed and instructed to lie quietly. A careful protocol was kept of the sequence of records to eliminate all chances of error in their subsequent identification after development. The maximum duration of any record was 60 sec.

(D) Analysis of records. The E.E.G. records forming the basis for the present report were those obtained with active electrode over the right occiput. Details concerning location of electrode is given in the section on technique. The properties of the records which were measured were *alpha index, alpha wave frequency,* and *average alpha wave amplitude.*

For the purposes of this study, alpha waves were defined as those waves whose individual durations fell between limits of one-seventh and one-fourteenth of a second, the corresponding frequency range for such waves occurring in continuous sequences being 7 to 14 a sec. Waves falling within this duration or frequency-range were not, however, used for determining the alpha wave properties specified above unless they had an amplitude of 15 μv. or more, and occurred in sequences of 5 or more waves. Any such sequence was designated as a regular sequence.

The three properties of the alpha waves measured (alpha index, alpha wave frequency, and average alpha amplitude) may be defined as follows: Alpha index equals the percentage of a standard sample occupied by regular sequences of alpha waves; alpha wave frequency equals the number of alpha waves per second occurring within regular sequences; and average alpha wave amplitude is an estimate of the average amplitude of alpha waves occurring within regular sequences.

As a preliminary to the determination of these characteristics of the alpha waves, a standard 'readable' sample of 40 sec. duration (86 cm. with paper speed of 2.2 cm. per sec.) was selected. This selection was made after the record had first been surveyed and the limits of any sections containing artifacts marked on the record. Then the first 40 sec. of record exclusive of the intervals containing artifacts were regarded as a standard sample. The type of phenomenon which was rejected, as representing possible artifacts, were relatively high frequency waves occurring at frequencies of the order of 40 to 50 a sec., which were interpreted as muscle potentials, and single waves of long duration such as could be produced by movement of the head against the electrode. The standard sample was now examined and crayon or pencil marks made to indicate the limiting points of all regular sequences.

Alpha index could now be easily determined by measuring the total length of the standard record occupied by regular sequences, and dividing by the length of the standard sample.[24] *Alpha frequency* was measured by counting the total number of waves occurring in regular sequences, and dividing this number by the total time occupied by all regular sequences. To determine *average alpha wave amplitude,* a sampling procedure was adopted, since it would have been excessively time-consuming to measure all alpha waves. This procedure was as follows: the 40 sec. of standard record was divided, by crayon marks, into 8 smaller units of 5 sec. each. As an index of the average amplitude of alpha waves, the plan envisaged was to measure in mm. the amplitude of the first 10 alpha waves in each of these eight 5 sec. sub-units, and to use the computed average amplitude of this sample of 80 waves as an index of the average amplitude of alpha waves throughout the entire standard record. This procedure was carried out in principle, but a number of supplementary rules were adopted for the following special situations. (1) If the beginning of a 5-sec. sub-unit fell in the middle of a 'regular sequence,' the waves used for measurement were those starting at that point, and not those at the beginning of the regular sequence. (2) If a total of 10 alpha waves (in a regular sequence) was not found in any 5-sec. sub-unit, then enough additional waves to make up the total of 10 were taken from the next following sub-unit containing alpha waves. The measurements for the next group of 10 waves representing this following sub-unit was then begun with the alpha wave following the last one measured, rather than with the first alpha waves in the sub-unit, since these would have already been measured. (3) In cases in which there was less than a total of 80 alpha waves in regular sequences throughout the standard record, the amplitude of all alpha waves was measured. In such cases, the average obtained was based on fewer than 80 waves. The average value obtained for the amplitude on the basis of this sampling procedure was converted from mm. to microvolts on the basis of the amplifier-calibration made at the time of a given *S*'s record. Although the sensitivity remained practically constant throughout the course of the study, very slight changes occurred, due probably to slight progressive changes in the condition of the batteries. A further correction was made in the recorded average amplitude, in accordance with the average frequency of the alpha waves found for a given *S*, adjustment being made to correspond to the slight differences in sensitivity of the amplifier at different frequencies specified in the section on the frequency-response curve.

(E) Statistical procedures: (1) Continuous variables. For determining the dependence of two variables on each other and for excluding the influence of a third variable, use was made, when the variables involved were continuous in character, of total and partial product-moment correlations. The statistical significance of

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[24] In previous papers of the author, the term pattern was used to designate the percentage of time the alpha rhythm is present. In the present paper the term alpha index, proposed by Davis (H. Davis, The electroencephalogram, *Tabulae Biologicae,* 17, part 2, 1938, 126), is used instead. The criteria used by Davis in the computation of the alpha index differ somewhat from the criteria used in the present study. Davis' calculation of the "per cent time alpha" is based on sequences of alpha waves at least 7μv. in amplitude and occurring in sequences of 3 or more waves. The "per cent time alpha" reported in this and previous studies of the author is based on consideration of alpha waves 15μv. or more in amplitude and occurring in sequences of 5 or more regular waves.

these correlations was determined on the basis of the methods proposed by Fisher.[25] Fisher has pointed out that when the population for which a given correlation has been computed is relatively small, the determination of reliability on the basis of probable error is not valid, since the *r*'s will not be distributed normally. As a substitute, he has furnished tables which specify the size of *r*'s which might arise in uncorrelated populations, corresponding to different levels of probability and for different degrees of freedom of the population. He proposes as a criterion of significance the 0.05 level of probability. Thus, an obtained *r* is regarded as significant if it is greater than an *r* for which there is a probability of 0.05 of it arising in an uncorrelated population. In the case of obtained correlations which were found to meet this criterion of significance, the probability of the correlation arising in an uncorrelated population was computed more precisely on the basis of Fisher's procedures for transforming a given product-moment *r* into its corresponding *z* value.[26]

(2) Non-continuous variable. One of the variables considered in the present study, sex, is non-continuous in character. Determination of its relation to the alpha rhythm and correction for its effects, when found, could not therefore be made by way of product moment correlations. To determine whether sex showed any significant relation to properties of the alpha rhythm, two other methods, both described by Fisher, were used: (a) the *t*-method, for determining the significance of the difference between two means; and (b) the method for analysis of variance. Since certain differences from the procedures conventionally used were involved in the use of these methods in the present study, a brief description of the steps followed is necessary.

Obtaining the difference of the means for the two sexes, and testing the significance of this difference provides a direct method for testing the influence of sex on any given property of the alpha rhythm. It was not feasible, however, to carry out this procedure with the total experimental group as a single unit, in view of the need of excluding the possible effects of mental age. The female group in the present study contains a larger proportion of *S*s of the lower mental age-levels, whereas the male group contains a relatively larger proportion of *S*s of the higher mental ages; moreover, evidence to be presented later indicates a tendency for certain properties of the alpha rhythm to change with mental age. Consequently, in determining the influence of sex alone upon the alpha rhythm it was necessary to exclude possible effects of variations in M.A. To balance the effects of M.A.-level in the two sex groups, the practice was followed of comparing the means for the two sexes in each of the five M.A.-groups separately. The differences thus obtained for the various M.A.-groups were weighted in proportion to the number of *S*s in the group, and combined to provide a final average difference to represent the total group of *S*s. Differences due to M.A. were thus excluded from the final result. An estimate of the standard error of this difference was obtained by pooling the variances provided by the two sexes in the different M.A.-groups in the manner described by Fisher for the pooling of variances from different groups.[27] The ratio *t* of the average difference to the standard error of the difference could then be tested for significance by means of Fisher's table of *t*, in the usual manner. For *t* to be regarded as significant it was considered necessary, in accordance with the practice proposed by Fisher, for the probability P corresponding to *t* to be less than 0.05.

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[25] R. A. Fisher, *Statistical Methods for Research Workers*, 2nd ed., 1928. (The most recent edition is the 7th, which appeared in 1938.)

[26] Fisher, *op cit.,* 163-177.

[27] Fisher, *op. cit.,* 108.

A second method used was the method for "analysis of variance" described by Fisher. The term variance, it will be recalled, refers to the square of the standard deviation of a distribution of measures. The effect of a given factor, such as sex, on the dependent variable (the E.E.G. property under consideration) is determined by comparing the part of the total variance (of the E.E.G. measure) due to differences between the sexes with the part due to variations in the measurements within any sex group. A statistic *z* is computed which equals the $\log_e$ of the standard deviation due to sex variations minus the $\log_e$ of the standard deviation due to variations within each sex group. This statistic *z* can be tested for significance by means of tables of *z* given in Fisher's book.[28] The procedure used in the present study to exclude the influence of a third variable, such as M.A. consisted in dealing with each M.A.-group as a separate unit. The sums of the deviations squared obtained for the separate M.A.-groups are all pooled to provide the final statistics representing the total group, on the basis of which the variances due to sex and to errors of observation are computed. Since the variations due to M.A. do not enter into any of the statistics, the influence of M.A. may be regarded as excluded from the final result. Fisher points out that the "analysis of variance method" may be regarded as equivalent to the "*t*-method" when the independent variable has but two classes, as is the case for sex. Both methods have, nevertheless, been used in this study so that they may provide a check on each other. When the independent variable contains more than two classes, the method for comparison of means cannot be used. The method for analysis of variance can, on the contrary, be used with any number of classes.

The application of the methods described above to the mongolian group indicated significant effects of sex on alpha wave frequency and alpha wave amplitude. It therefore became necessary to exclude the possible influence of sex from the correlations obtained between alpha wave frequency and M.A., and between wave-amplitude and M.A. The familiar method of correcting such correlations by the obtaining of partial correlations, corrected for the third variable, could obviously not be used, since the influence of sex could not be represented by means of coefficients of correlation.

The following procedure was adopted for excluding the influence of sex from the correlation-coefficients in question. In connection with a given alpha wave property, for example, frequency, separate coefficients of correlation between the alpha-wave property and mental age were obtained for *each of the two sexes.* These *r*'s were transformed into their corresponding *z* values on the basis of tables given in Fisher. These *z* values were then compared to determine whether they were significantly different. These tests indicated that the *z* values obtained could be regarded as representative of like-correlated populations. The *z* values corresponding to the two sex groups were then combined or averaged, and the resultant *z*-value for the composite group converted back into its equivalent *r*. The significance of the composite *z*-value, and consequently of the equivalent *r* was determined from tables.[29] The procedure just described thus provides a way for correcting for the effects of non-equal proportions of the two sexes in the different mental-age groups.

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[28] Fisher, *op. cit.*, 177.

[29] Fisher, *op. cit.*, 163-171.

II. RESULTS

(A) Dependence of properties of the alpha rhythm on Binet M.A.: raw correlations. The empirical relations obtaining between the properties of the alpha rhythm and Binet mental age were determined by means of product-moment coefficients of correlation. The coefficient of correlation obtained between alpha index and mental age was +0.316. The correlation obtained between alpha wave frequency and mental age was −0.291. The correlation obtained between alpha wave amplitude and mental age was +0.114. The size of the correlation required for statistical significance lies between 0.2875 and 0.2732, corresponding to 50 and 45 degrees of freedom respectively.[30] On the basis of this criterion, the correlations of M.A. with alpha index, and with frequency are significant whereas that between M.A. and amplitude is not significant. These estimates of significance are to be considered only provisional pending consideration of the possible rôle that variables other than M.A. may have played in these relationships. In the discussion that follows the correlations specified above between properties of the alpha rhythm and M.A. will be referred to as the raw correlations.

In Fig. 2, the dependence of the three properties of the alpha rhythm on mental age is represented graphically. Separate curves are given for the two sexes and, in addition, a third, a composite curve, which represents the total group of *S*s, without consideration of sex. The extent to which sex should be taken into account in estimating the dependence of the alpha rhythm on mental age will be considered below. Points are also plotted in each graph to indicate the average values obtained for each property in a group of mentally normal adults made up of 15 males and 15 females.

For each of the properties of the alpha rhythm considered, the y-coördinates of the points on the curve are the mean values of the property in question for each of the mental age-groups. With respect to the x-coördinate axis, it should be noted that the M.A. class intervals represented are not the same as those used in computing correlations. In the computation of correlations, half-year M.A.-intervals were used. In the computation of the mean values represented in the curves in Fig. 2, a fewer number of mental age class-intervals was used in order to reduce the effect of sampling errors in any given group and thus permit the graphs to show more effectively any general trends in the data.

(B) The effect of other variables on the raw correlations. In Section I were specified the measures taken to control, by way of experimental conditions, or in the selection of *S*s, certain factors which might have an effect on the alpha rhythm, independently of mental age and which might therefore have proven sources of error in the determination of the relation

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[30] Fisher (Table V A), *op. cit.*, 176.

between the properties of the alpha rhythm and mental age. In the present section, the nature of some of these potential sources of error is examined, and an attempt made to determine whether or not they may be regarded as excluded from our results. The factors to be considered here are electrode resistance, electrode location, life age, and sex. These factors may be referred to, for the sake of brevity, as subsidiary variables. Our problem in connection with each of these variables is the same. It is necessary (1) to determine whether any of them has a significant influence on the properties of the alpha rhythm, and (2) to correct for this influence when it is found to be present. Each of the properties of the alpha rhythm may be considered in turn.

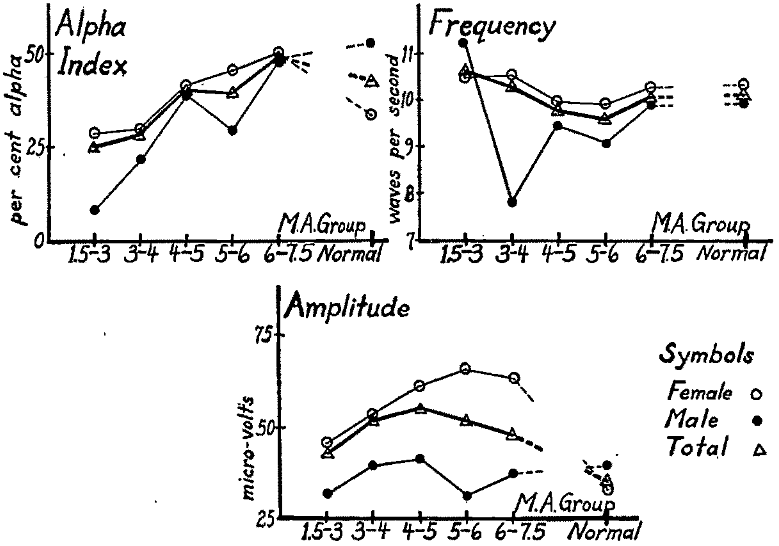

FIG. 2. PROPERTIES (MEAN VALUES) OF OCCIPITAL ALPHA RHYTHM AS A FUNCTION OF BINET MENTAL AGE-LEVEL

(1) Alpha index and mental age. An examination of the relations between alpha index and each of the four subsidiary variables shows that none of them exercises a significant influence on alpha index. No adjustment is necessary, therefore, of the initial correlation of +0.316 obtained between alpha index and M.A. This correlation is greater than the value of 0.2875 required for significance. The probability of a correlation of this size arising by chance in an uncorrelated population is 0.03, or only 3 chances in 100. The correlation between alpha index and Binet M.A. may therefore be regarded as significant, and not dependent on any con-

comitant variations of electrode-resistance, electrode-location, life-age or sex. The detailed evidence is considered below.

(a) Electrode resistance. As indicated in the section on technique, a uniform procedure was followed in the application of electrodes, and measures were taken to assure good contacts of electrodes with the skin. Nevertheless, a certain range of variation in inter-electrode D.C. resistance was found among the various *S*s of the group. It is necessary to inquire whether these variations could have influenced the correlation obtained between M.A. and alpha index, and to make corrections if they are found necessary.

Theoretically, an increase in inter-electrode resistance would have as a consequence a reduction in the voltage changes occurring at the input of the amplifier. The consequent reduction in the amplitude of the recorded waves might conceivably lead to a decrease in alpha index if waves of low amplitude were no longer counted on account of falling beneath the required minimum. The following calculations show, however, that even for the extreme values of resistance found among the *S*s, the effect on the amplitude would be so small as to be negligible.

The average resistance found for all *S*s in the group was 8480 ohms, with a range between 1170 ohms and 16,875 ohms. The maximum resistance, 16,875 ohms, was thus low relative to the input resistance of the amplifier, which was 200,000 ohms. An increase in inter-electrode resistance even as great as that between the two extremes of 1170 ohms and 16,875 ohms, could produce but a relatively small change in the amplitude of the recorded waves. Assuming a constant e.m.f. originating in the *S*s, a change in resistance from 1170 ohms to 16,875 ohms could lead to a reduction in the amplitude of the recorded waves of but 7.3%. Accordingly, for alpha waves 1.5 mm. in amplitude from trough to peak, waves which just exceed the minimum required voltage of 15 µv., the reduction in amplitude would be less than 0.11 mm., a quite negligible difference. It seems improbable therefore that variations in electrode resistance could have had any appreciable effect on the alpha index.

These calculations are made on the assumption that the change in potential difference at the input of the amplifier will be proportional to the total resistance of the circuit consisting of the input resistance of the amplifier and the inter-electrode resistance in series. The calculations may be made on the basis of the D.C. measurements of resistance since the impedance of the input circuit to alternating current as low as 10 ~ will be but negligibly different from the D.C. resistance.

Inspection of the correlation coefficient actually obtained between alpha index and electrode resistance for the total group of *S*s shows that electrode resistance had in fact no significant influence on the alpha index. The raw correlation between alpha index and electrode resistance was +0.008. The partial correlation between these 2 variables, corrected for variations in M.A. was —0.007. The correlations are thus extremely small and of no significance. (If, nevertheless, the correlation between M.A. and alpha index is corrected for electrode resistance, the change produced is from a correlation of 0.316 to a partial correlation of 0.324, an increase of 0.008.)

(b) Electrode position. As indicated in the section on technique, no effort was made to apply the active electrode at a constant distance from the inion and from the midline, but merely at the same approximate position. It is, therefore, necessary to inquire whether the slight variations which occurred in the position of the electrode exercised

any influences on the alpha indexes obtained. The position of the electrode may be characterized in terms of the absolute inion-distance, the relative inion-distance, absolute lateral distance, and relative lateral distance. The definitions of these terms have already been given on p. 516.

For the total group of *Ss*, the average absolute inion-distance was 1¼ in., with extreme values of 1 in. and 2 in. from the inion. The average absolute lateral distance was ⅞ in., with extreme values of ½ in. and 1⅜ in. from the median plane. As already noted, this electrode-region falls within the area specified by Adrian as representing the area of maximal alpha activity at the occiput. It is not to be expected, therefore, that the variations in electrode placement which occurred will show any significant relation to variations in the alpha index, in so far as alpha index may be dependent on the amplitude of the waves recorded.

An examination of the correlations obtained between alpha index and electrode position fails in fact to show any significant influence of electrode position within the limits involved. In the following list of correlations the first one given refers in each instance to the total correlation obtained; the second one to the partial correlation, corrected for M.A.

(1) Between alpha index and *absolute* inion-distance: —.13 and —.10;
(2) Between alpha index and *relative* inion-distance: —.13 and —.09;
(3) Between alpha index and *absolute* lateral distance: +.06 and +.16;
(4) Between alpha index and *relative* lateral distance: +.04 and +.18

The correlations are all well below the value of 0.287 required for significance. If, despite this evidence that variations in electrode position had no significant effect, the correlation of +0.316 obtained between *alpha index* and *M.A.* is corrected for electrode position, the following results are obtained. Corrected for *absolute* inion-distance, the partial correlation between alpha index and M.A. becomes +0.306; corrected for *relative* inion-distance, it becomes +0.303; corrected for *absolute* lateral distance, it becomes 0.347 and corrected for *relative* lateral distance, it becomes +0.328. The changes thus produced in *r* are relatively small, the partial *r*'s ranging between extremes of +0.303 and +0.347. All of these values are appreciably above the *r* of +0.287 required for significance. The presence of a significant correlation between alpha index and Binet mental age cannot, therefore, be attributed to the slight variations in electrode position. It seems appropriate, however, to regard the raw correlation coefficient of +0.316, in view of the evidence cited above that electrode position exercised no significant effect, as the most satisfactory value for representing the correlation between alpha index and M.A.

(c) Chronological age. As already noted, all *Ss* in the experimental group were older than 16 yr. of age chronologically, and thus well above the age at which any influence of life age on alpha rhythm has been reported. It is nevertheless of interest to determine whether variations in life age above the limit specified exercised any significant effect on the alpha index.

The product-moment correlation obtained between alpha index and chronological age was +0.008. The partial correlation obtained, when corrected for M.A., was —0.006. The correlations are thus extremely small, and of no significance. Variations in life age cannot, therefore, be considered a factor in the significant correlation obtained between alpha index and mental age.

(d) Sex. It was not found feasible, as pointed out on p. 507, to maintain the same proportion of the two sexes in the different M.A. groups. It is necessary, therefore,

to determine whether or not there are, for the present group of *Ss*, significant differences in the properties of the alpha rhythm of the two sexes and whether any statistical correction is required for differences in the proportions of the sexes represented at the different M.A.-levels.

As mentioned in the section on statistical method, two procedures were used for determining the influence of sex on the properties of the alpha rhythm, the method based on comparison of means, and the method based on analysis of variance. The following results were obtained from comparison of the mean alpha indexes of the male and female groups. The average difference in alpha index between males and females for the total experimental group is 11.01, the standard error of the difference is 8.24. The critical ratio t is 1.35. The minimum ratio required for significance even where the number of cases is large is 2. Since the obtained t was less than this, the obtained difference cannot be regarded as significant.

The application of the method for analysis of variance gave the following results. The parts of the variance compared was the part due to differences *between* the two sexes and the part due to variations *within* the two sex groups. The influence of M.A. variations was excluded by the procedure already described. The value for z obtained was 0.0899. The minimum z required for significance is 0.4311. The result thus agrees with that obtained above, in showing that sex exercises no significant influence on the alpha index, in the mongolian group. It is, therefore, not necessary for the correlation between alpha index and M.A. to be corrected for the effect of sex.

(2) Alpha wave frequency and mental age. When the relation between alpha wave frequency and the 4 subsidiary variables (*supra* 523) was examined, sex was found to be the only factor related significantly to alpha wave frequency. It is, therefore, necessary for the raw correlation of —0.291 obtained between alpha wave frequency and mental age to be corrected so as to exclude the possible influence of unequal proportions of the two sexes at different M.A. levels. The other subsidiary variables need not be taken into account further in view of the evidence just mentioned that they had no significant influence on alpha wave frequency. Possible effects due to sex may be excluded from the correlation between frequency and mental age by obtaining separate correlations for the two sexes, and averaging them by the method described by Fisher. The result of this procedure is to change the correlation coefficient between alpha frequency and mental age from one of —0.291 to one of —0.213. The latter coefficient may be regarded as more accurately representing the empirical dependence of alpha wave frequency on mental age. This coefficient is *not,* however, statistically significant since it is smaller than the minimum coefficient of 0.2875 required for significance. The chances of a correlation as large as 0.213 arising by chance in an uncorrelated population are 15 in 100 (as determined through the use of Fischer's z coefficients) ; whereas for r to be significant, the chances should not be greater than 5 in 100. It may be concluded, therefore, that there is no adequate evidence of a real association between alpha wave frequency and Binet

mental age. The detailed results supporting the above summary follow.

(a) Electrode resistance. The correlation obtained between alpha wave frequency and electrode resistance was —0.263; the partial correlation corrected for variations in M.A. was —0.205. Neither of these correlations are significant inasmuch as the minimum correlation required for significance lies between 0.2875 and 0.2732, corresponding to 45 and 50 degrees of freedom, respectively. The number of degrees of freedom in our sample is 48 in the case of the total correlation, and 47 in the case of the partial correlation. On the basis of the criterion of significance adopted, it may be concluded that variations in electrode resistance had no significant influence on alpha wave frequency.

(b) Electrode position. The correlations obtained between the various components of electrode position and alpha wave frequency were as follows:

Between frequency and absolute inion distance	—.097 and	—.142
Between frequency and relative inion distance	—.045 and	—.090
Between frequency and absolute lateral distance	—.074 and	—.170
Between frequency and relative lateral distance	+.007 and	—.100

In each case, the correlation first given is the total correlation; the second is the partial correlation, corrected for variations in M.A. None of the correlations given is significant since they all fall well below the criterion of significance specified above. It may be concluded that variations in alpha wave frequency were independent of any variations which were present in absolute or relative electrode position.

(c) Chronological age. The correlation obtained between alpha wave frequency and chronological age was +0.153; the partial correlation between these two variables was +0.174, corrected for variations in M.A. Neither of these correlations are significant. It may therefore be concluded that there is no significant association between variations in alpha wave frequency and chronological age, for a range between 16 and 56 yr.

(d) Sex. The data resulting from the comparison of means for the two sexes (with the influence of M.A. excluded) were as follows. The average difference in alpha wave frequency for the two sexes was 0.764 waves per sec. with a standard error of 0.34. The sign of the difference corresponds to a higher frequency for the female group. In the determination of this difference it was found that the female group showed the higher mean frequency for every one of the five M.A.-groups considered except the lowest. The ratio t of the average difference to its standard error is 2.24. The number of degrees of freedom represented is 40. Fisher's t-table indicates that for 30 degrees of freedom, the t required for significance is 2.042. On this basis the average difference found of 0.764 may be regarded as significant.

The application of the 'analysis of variance' method, with the influence of M.A. excluded, gives the following data. The value of z obtained was +0.9101. The value of z required for significance is 0.4648 for the 0.05 level of significance, and 0.654 for the 0.01 level of significance. The z obtained must therefore be regarded as significant and indicates a real effect of sex upon the frequency of the alpha waves in the mongolian group.

The two statistical tests applied thus agree in indicating a significant influence of sex on alpha-frequency in the mongolian group, and points to the need of excluding the possible effects of sex in the determination of the relations between frequency and mental age. To exclude the influence of sex from the correlation between alpha wave frequency and mental age, we may obtain separate correlations between these latter two variables for each of the two sexes, and average them in the manner described by Fisher.

The steps in this procedure are as follows: The correlations obtained between wave frequency and M.A. are —0.198 for the female group, and —0.261 for the male group. These values are converted into their corresponding z values by means of Fisher's Table V B, and the difference between the z's tested for significance. The difference between the z values was found to be 0.053 with a standard error of 0.348. The difference is not significant and the two r's may be regarded as derived from equally correlated populations. It is, therefore, justifiable to average them.

The (weighted) average value of z was found to be 0.2168 with a standard error of 0.151. The value of r corresponding to this average z is 0.213. The ratio of z to its standard error equals 1.44. Since this ratio is less than two, z cannot be regarded as significant. We may obtain a more precise estimate of the probability of the z obtained arising in an uncorrelated population by the use of a normal probability table. Corresponding to the ratio 1.44 just given, it is found that the chances of a z as large as 0.2168 arising in an uncorrelated population are 15 in 100. The chances are, therefore, 15 in 100 that an r as large in absolute magnitude as 0.213 could arise by random sampling in an uncorrelated population. This value is appreciably greater than the value of 0.05 allowed by our criterion of significance. We are not justified, then, in inferring a significant relation between alpha wave frequency and mental age.

(3) Alpha wave amplitude and mental age. An examination of the relation of the four 'subsidiary variables' and alpha wave amplitude reveals the fact that significant differences in wave-amplitude are associated with differences in sex in the mongolian group. It is therefore necessary in this instance, too, to correct the correlation of alpha wave-property and mental age to exclude possible errors due to the sex variable. The other three variables considered, electrode resistance, electrode position, and life age, show no significant correlation with alpha wave amplitude and therefore make no corrections necessary. When potential errors due to sex are excluded, by obtaining correlations for the two sexes separately and combining them, the correlation between wave amplitude and mental age is changed from a value of +0.114 to one of +0.313. By the use of Fisher's z-coefficient it is found that the probability of an r of this size arising by chance in an uncorrelated population is 0.031. The probabilities of an r as large as 0.313 occurring by chance are thus less than the 0.05 value adopted as a criterion of significance. It may be concluded, therefore, that alpha wave amplitude shows a significant positive correlation with mental age; that is to say, wave amplitude tends to increase with mental age level. The detailed results follow.

(a) Electrode resistance and wave-amplitude. The correlation obtained between electrode-resistance and wave-amplitude was —0.183; the partial correlation corrected for variations in M.A. was —0.22. These values are both below the minimum value of 0.287 required for significance. It may be concluded that electrode resistance has no significant influence on alpha wave-amplitude.

(b) Electrode position and wave-amplitude. The correlations obtained between the various components of electrode position and alpha wave-amplitude were as follows:

Between amplitude and absolute inion distance	—.115 and	—.105
Between amplitude and relative inion distance	—.064 and	—.049
Between amplitude and absolute lateral distance	+.005 and	+.040
Between amplitude and relative lateral distance	+.095 and	+.142

The correlation given first in each line is the total correlation, that given second is the partial correlation, corrected for M.A. All of these correlation coefficients are well below the minimum value of 0.287 required for significance. It may be concluded that variations in electrode-position exercised no significant effect on alpha wave-amplitude.

(c) Chronological age. The correlation obtained between wave amplitude and chronological age was —0.044; the partial correlation, corrected for M.A., was —0.049. Both of these correlations are extremely small, and obviously not significant. It may be concluded that variations in chronological age within the limits involved in this study were not associated with variations in alpha wave amplitude.

(d) Sex and alpha wave-amplitude. The investigation of the influence of sex on wave amplitude carried out by way of the comparison of mean values in the two sexes gave the following results: average difference between the two sexes (female minus male), with influence of mental age eliminated, is +20.20; the standard error of this difference is 7.69; the ratio t of the difference to its standard error is 2.6; the number of degrees of freedom involved is 40. The value of t required for the 0.05 level of significance for 30 degrees of freedom is 2.042; t is thus well above this value and must regarded as significant. These results justify the conclusion that there is a significant tendency in the mongolian group for the females to show alpha waves of greater amplitude than the males. It is of interest to note that the mean value of the amplitude was greater for the females than the males in each of the five M.A.-groups.

The application of the 'analysis of variance' method in order to determine the influence of sex on wave-amplitude gave the following results. The value of z obtained was +0.5099; the value of z required for significance for the 0.05 level of significance was 0.4648. The value of z obtained is higher than this value and consequently must be regarded as significant. The results of this test thus agree with the other in indicating that sex exercises a significant influence on alpha wave amplitude.

It accordingly becomes necessary to correct the value of the correlation coefficient obtained between wave-amplitude and mental age, in order to eliminate effects due to unbalanced proportions of the sexes at different M.A.-levels. The procedure used previously of obtaining separate coefficients of correlation for each of the sexes and averaging them may be used. The data obtained were as follows. The first step is to obtain coefficients of correlations for the two sexes separately and to determine whether they may be regarded as arising from equally correlated populations. The coefficients of correlation obtained between alpha wave amplitude and M.A. was +0.400 for the females, and +0.044 for the males. The corresponding z values were 0.4183 and 0.0433 respectively. The difference 0.375 was found to have a standard error equal to 0.348; the ratio is accordingly 1.07. The value is below the minimum critical ratio of 2 required for significance. The result is thus compatible with the hypothesis that the r's for the two sexes were derived from equally correlated populations.

The next step is to combine the z values corresponding to the two sexes. The two z-values are weighted in proportion to their variances and averaged. The result is an average z for the total group equal to 0.324 with a standard error of 0.150. The ratio of z to its standard error is 2.15. Since this ratio is greater than 2, z and its corresponding r (which equals 0.313) may be regarded as significant. Or we may determine more precisely the level of significance of z through the use of a normal probability table. The table shows that a deviate which is 2.15 times its standard error has a probability of being exceeded of but 0.0316. This value is smaller than our 0.05 criterion of significance. It may be concluded, therefore, that the average value of r, equal to 0.313, which we have obtained to represent the total group, is significantly greater than zero, since the chances of its arising by random sampling in an uncorrelated population are but slightly more than 3 in 100. There is thus evidence of a real association between alpha wave amplitude and mental age, when the effect of an uneven proportion of the two sexes at different mental age levels is eliminated.

It is of interest to consider in more general terms why the elimination of possible errors due to unequal proportions of the sexes has *increased* the size of the correlation between amplitude and M.A. The initial correlation of 0.114 obtained for the total experimental group represents a population in which there are relatively more females at the lower M.A.-levels, and relatively fewer at the upper M.A.-levels. Since the females tend to show greater alpha wave amplitudes than the males, the effect of such disproportions will be to raise the level of regression line (of amplitude on M.A.), at the lower mental age-levels and lower it at the higher levels. The slope of the line is thus less than if there were the same proportions of females at all M.A.-levels. Consequently when we eliminate the influence of such disproportions by obtaining correlations for each sex group separately, the result will be an increase in the positive slope of the regression line and in the corresponding coefficient of correlation.

A similar type of reasoning applies in accounting for the *reduction* which takes place in the correlation coefficient between alpha wave frequency and M.A. when the sex factor is controlled. In this case we are dealing with a negative correlation and a regression line of negative slope. Results cited earlier showed that the female group tended to show higher alpha frequencies than the male groups. The excessive proportion of females at the lower M.A.-levels should, therefore, have the effect of raising the level of the lower end of the regression line, that is to say, of increasing the negative slope of the line. Consequently when the effect of a disproportion in the sexes at different M.A.-levels is eliminated by dealing with each sex-group separately, the regression line will acquire a flatter slope and the coefficient of correlation will become smaller in absolute magnitude.

III. SUMMARY AND CONCLUSIONS

The present study represents one of a series undertaken to determine whether there are significant correlations between the electro-encephalogram and intelligence level. The feebleminded offer important methodological advantages in the investigation of this question, since they provide the possibility of a wide range of variation in intelligence level, and permit independent control of mental age and chronological variables. Properties of the electro-encephalogram examined were the alpha index, alpha wave frequency, and alpha wave amplitude of the occipital electro-encephalogram. The experimental group was limited to *Ss* of the mongolian type of mental deficiency. Intelligence level was measured by means of the 1916 form of the Stanford-Binet Scale, supplemented by the Kuhlman-Binet Scale for test levels under 3 yr.

Considerable attention was given to subsidiary conditions which might have led to changes in the electro-encephalogram independently of mental age effects, or which might have obscured the characteristics of any record. The conditions of this sort taken into account were instrumental artifacts, muscle potentials, conditions in *S* during recording, such as states of sleep, emotional excitement, and visual stimulation, variations in electrode position, inter-electrode resistance, chronological age-variations, and the proportions of the two sexes at different mental age-levels. Procedures for control of such factors occurred by way of selection of *Ss*, control of experimental conditions, techniques of record-analysis, and statistical corrections in the treatment of data.

The final results obtained after steps were taken to exclude possible errors due to such factors are represented by product-moment correlations between mental age-level and alpha index, alpha wave frequency, and alpha wave amplitude. Statistically significant correlations were obtained of mental age-level with alpha index and alpha wave amplitude, but not with alpha wave frequency. The criteria of statistical significance used were those of Fisher.

The finding of the significant correlations just mentioned points to the existence of common physiological factors underlying certain properties of the electro-encephalogram and mental age-level in the mongolian type of mental deficiency, and opens the way to the use of the electro-encephalogram in the discovery of the nature of these factors. Before these results can be regarded as definitely established, however, they must be confirmed by further, more extensive investigations, particularly in order to determine the extent to which errors of sampling may have been involved in the selection of the experimental group. More extensive investigations to this end are in progress.

AN EXPERIMENTAL STUDY OF REVERSALS IN READING

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During the past three decades considerable clinical and experimental attention has been devoted to children with average or superior intelligence who have had difficulty in learning to read under ordinary school instruction.[1] A characteristic noted in such children has been the frequent reading of words, letters, or sentences in a reversed or right-to-left direction. Implicit in many of these studies is the assumption that children making reversals actually observe the reading material in a right-left direction. Theories have been advanced to account for the presumed backward visual observation pattern.[2] Experimental evidence to show that the visual impressions leading to the reversal-responses were formed in the right-left direction, however, has been lacking.

The aim of the study reported here was to investigate reading behavior

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[*] Accepted for publication September 15, 1938. This paper is the first of three covering a dissertation presented in partial fulfillment of the requirements for the degree of Doctor of Philosophy at Yale University in 1935. The study was done under the supervision of Professor Walter R. Miles and Dr. Catherine C. Miles to whom the writer is greatly indebted.

[1] Extensive bibliographies and summaries of the literature on reading disability have been compiled by A. I. Gates (*The Improvement of Reading,* 1927, 1-440), W. S. Gray (*Summary of Investigations Relating to Reading,* 1925, 1-275; Summary of reading investigations, *J. Educ. Res.,* 26, 1933, 401-424), and Joseph Jastak (Interferences in reading, *Psychol. Bull.,* 31, 1934, 244-272). Experimental studies on reversals have been reported by W. F. Dearborn (The nature of special abilities and disabilities, *School and Soc.,* 31, 1930, 632-636), L. G. Fildes (Psychological inquiry into the nature of the condition known as congenital word blindness, *Brain,* 44, 1921, 286-307), A. I. Gates and C. C. Bennett (Reversal tendencies in reading, *Teach. Coll. Bur. Publ., Col. Univ.,* 1926, 1-23), Gertrude Hildreth (Success of young children in number and letter construction, *Child Develop.,* 3, 1932, 1-14; Reversals in reading and writing, *J. Educ. Psychol.,* 25, 1934, 1-20), Alexander Mintz (A study of indications of unstable unilateral cerebral dominance, reading disability and mental deficiency, *Psychol. Bull.,* 30, 1933, 565-566), Marion Monroe (Methods for diagnosis and treatment of cases of reading disability, *Genet. Psychol. Monog.,* 4, 1928, 335-456; *Children Who Cannot Read,* 1932, 1-205), Lorene Teagarden (Tendency to reversal in reading, *J. Educ. Res.,* 27, 1933, 81-97), Clifford Woody and A. J. Phillips (The effects of handedness on reversals in reading, *J. Educ. Res.,* 27, 1934, 651-662), and others. Many clinical observations of the phenomenon have also been reported.

[2] W. F. Dearborn, Teaching reading to non-readers, *Elem. School J.,* 30, 1929, 266-269; Structural factors which condition special disabilities in reading, *Proc. Amer. Assn., Ment. Def.,* 57, 1933, 268-283; The causes of "word-blindness," *Science,* 80, (no. 2084), 1934, 12-14. S. T. Orton, Specific reading disability—strephosymbolia, *J. Amer. Med. Assoc.,* 90, 1928, 1095-1099; The three levels of cortical elaboration in relation to certain psychiatric symptoms, *Amer. J. Psychiat.,* 8, 1929, 647-659.

involved in making reversals. The reading responses of children inferior in reading ability were observed under carefully arranged experimental conditions and then compared with those of average readers obtained under the same conditions.[3] Six experiments were conducted. Four were concerned with the effect on word-reversals (I) of controlling direction of word-exposure, (II) of eliminating reactive eye movements, (III) of accentuating word-form, and (IV) of mirroring the material. The remaining experiments were concerned with (V) the direction of reading single letters and rows of letters, and (VI) the direction of reproducing geometric designs.

METHOD AND PROCEDURE

Subjects. Two groups of *Ss* were selected for the experiments: a control group, composed of *Ss* showing average achievement in reading and arithmetic computation (reading-age and arithmetic-age approximating chronological age closely enough to yield quotients between 90 and 110), and an experimental group, composed of *Ss* possessing average achievement in arithmetic computation but a retardation in reading-age of at least two years below chronological age. The groups were further limited by selecting *Ss* who were within arbitrary ranges of certain variables with which reading achievement has been demonstrated to accord fairly closely: (1) average intelligence; (2) chronological age between 8 yr. 11 mo. and 9 yr. 11 mo.; (3) attendance in school for three complete years beyond kindergarten; (4) absence of speech disturbances; and (5) membership in unilingual, American-born families of middle social and economic status. Finally, the *Ss* of the experimental group were paired with those of the control group on mental and chronological ages.

The study was carried out in five grade schools in Weathersfield, a commuters' suburb of Hartford, Connecticut, inhabited chiefly by families of middle social class, and in Ivy school of New Haven, Connecticut, which is attended chiefly by children from middle social class homes. All children who were in the fourth grade or below and who complied with the chronological age requirement and the arbitrary limits listed above were given one form of the New Stanford Achievement Paragraph Meaning Test and Arithmetic Computation Test. The children who approximately fulfilled the achievement requirements were then given the Haggerty Sigma I Reading Test and a second form of the New Stanford Paragraph Meaning Test and Arithmetic Computation Test.

The children who were indicated by the above mentioned tests as having a special reading difficulty of the degree defined for this study were given the Stanford Binet Intelligence Test. Those individuals who now met the requirement of average intelligence and who confirmed the manifestation of reading difficulty during an individual examination on the Gates Pronunciation Test and a third form of the Stanford Achievement Test were enlisted in the experimental group.

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[3] In subsequent articles the two groups of *Ss* used in the present study of reversals will be compared with respect to certain other factors which have been considered by some investigators as typical of, or causal in, reading disability and reversal-errors; hand and eye dominance, audition, vision, verbal associations, and emotional adjustment.

The control group was selected in an equally rigorous manner. Stanford Binet tests were administered to children from the total group surveyed whose achievement test scores met the criterion of average adequacy and whose chronological ages could be paired with those of the experimental *S*s. Individuals were selected whose mental ages matched those of the experimental *S*s.

Of the 119 boys and 99 girls tested in the original survey, 21 of the former and 4 of the latter proved to have a reading difficulty of the degree defined for this study. Eighteen of the boys and 2 of the girls conformed with the other criteria also. In the interests of homogeneity the present report has been confined to the results obtained from studying the boys.

The grade placements and number of grade repetitions for the 18 experimental and 18 control boys are given in Table I. All *S*s had attended school 3 years beyond kindergarten; all, consequently, had been exposed to reading instruction for an equal number of years. The majority of the control *S*s were in the fourth grade; the majority of the experimental ones, having repeated a grade, were in the third grade.

TABLE I

CLASSIFICATION OF THE SS ACCORDING TO GRADE-PLACEMENT AND NUMBER OF GRADES REPEATED

Ss	Grade placement			Number grades repeated		
	2	3	4	0	1	2
Experimental	1	15	2	2	15	1
Control	0	2	16	16	2	0

The average achievement-ages and grades for the two groups of *S*s on the selection-tests are given in Table II.[4] The statistical significance of the differences between the reading-ages of the two groups indicates that the groups were definitely of different samples in the reading processes measured.[5] The average arithmetic-ages and quotients for both groups were within an average achievement-range. The significance of the difference in arithmetic-scores may be higher than the true difference in arithmetical ability because the majority of the control *S*s, as compared with very few of the experimental ones, had been introduced to division in the fourth grade and because some confusion in carrying out the printed instructions of the arithmetic tests occurred in the experimental group despite the experimenter's attempt to avoid this difficulty.

Table III shows the chronological ages, mental ages, and intelligence quotients obtained on a corrected scoring of the Stanford Binet Intelligence Test for the paired *S*s. In order to arrive at a rating of intelligence which would accurately represent the ability of the experimental *S*s who are penalized on certain test items by the possession of reading handicaps, a correction was applied by omitting from a revised scoring of the Binet those items which such *Ss*, as opposed to control *Ss*, have been found to fail rather consistently.[6] These items include reading and report, dissected sentences, vocabulary, designs, and repetition of digits backwards. The procedure followed was that of giving the complete Binet and scoring it in the usual manner. Then the correction in scoring was applied for both groups of *Ss* by omitting the above enumerated items up through the last age-level at which three or more items were passed. The number of months represented at each age-level were redistributed among the items remaining after the omission of the penalizing tests. This procedure proved to leave the average rating made by the control group on the uncorrected form almost unaltered. The uncorrected Binet yielded an average intelligence rating for the experimental group which was 3.61 IQ points lower than that of the control group. The difference was reduced to 1.05 IQ points on the corrected scale.[7]

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[4] In order to compute average achievement-ages for the reading handicap group, the procedure of assigning an age-score of 6 yr. to *S*s who did not score at all on the tests was used since the average chronological age for school entrance, and hence for practically no achievement at all, was six years in this community.

[5] Student's correction was utilized in computing the standard errors of the means. A critical ratio of 2.89 was considered significant. It is Fisher's value of t for 17 degrees of freedom, or n-1 in this study.

TABLE II

MEAN ACHIEVEMENT AGES, GRADES, AND QUOTIENTS OBTAINED BY THE EXPERIMENTAL AND CONTROL GROUPS ON TESTS FOR SELECTION

Tests		Groups of Ss						D/σ_d
		Experimental group			Control group			
		age	grade	quotient	age	grade	quotient	
Reading:								
Stanford Achievement Paragraph Meaning*	M	6.74	1.98	72.11	9.59	3.79	102.94	14.62
	σm	.15	.12	1.59	.13	.10	1.75	
Gates Pronunciation	M	6.75	1.33	72.22	8.97	3.27	95.67	10.83
	σm	.03	.03	.76	.20	.19	2.46	
Haggerty Sigma I	M	7.19		76.56	9.64		103.39	10.38
	σm	.19		1.95	.15		1.72	
Arithmetic:								
Stanford Achievement Computation†	M	8.94	3.30	95.61	9.78	3.96	105.11	4.86
	σm	.14	.09	1.36	.11	.09	1.25	

* Based on the average of scores on 3 forms (V, X, Y) of the test.
† Based on the average of scores on 2 forms (V, X) of the test.

Apparatus. A falling-screen exposure-apparatus, modeled after the Dodge falling-plate apparatus,[8] was used for the presentation of the visual material (Fig. 1). The principle of the apparatus is that of exposing stationary material when a window in the falling-screen coincides with one in a front-screen behind which is the printed material. The apparatus was designed with several considerations in view: it had to be portable, adjustable in height, and adjustable in speed of fall so that it could be used for both slow and fast exposures. When the apparatus was to be used for slow exposures, the falling-screen was attached to a piston which moved in an oil filled cylinder. When the oil control was not attached, the instrument could be used for rapid exposures.

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[6] L. S. Wolfe and C. C. Miles (unpublished study on the influence of reading disability on the Stanford Binet intelligence rating).

[7] Donald Durrell (The effect of special disability in reading on performance on the Stanford Revision of the Binet-Simon tests, unpublished M.A. Thesis, University of Iowa, 1927) found an average increase of 4.75 IQ-points for *Ss* with reading disability of much wider C.A.- and M.A.-ranges by using a somewhat different correctional scheme on the Binet scores.

[8] Raymond Dodge and T. S. Cline, The angle-velosity of eye movement, *Psychol. Rev.*, 8, 1901, 145-147.

TABLE III

Chronological Ages and Corrected Stanford Binet Mental Ages and Intelligence Quotients for Every *S* in the Experimental and the Control Groups

(The *S*s of the two groups which were paired are represented by the same numbers.)

Subject pairs	C.A.*		Stanford Binet corrected			
			experimental		control	
	experimental	control	M.A.	IQ	M.A.	IQ
1	9-3	9-1	9-10	106	10-0	110
2	9-5	9-5	10-0	106	10-2	108
3	8-11	8-11	9-4	105	9-4	105
4	9-8	9-7	10-4	107	10-3	107
5	9-3	9-3	9-8	105	10-6	114
6	9-10	9-10	10-6	107	9-7	97
7	9-1	9-1	9-9	107	9-4	103
8	9-4	9-4	9-4	101	9-8	104
9	9-5	9-7	9-2	97	9-1	95
10	8-11	8-10	8-9	98	8-10	100
11	9-3	9-3	8-7	93	8-11	96
12	9-0	9-0	8-8	96	9-0	100
13	9-3	9-1	8-7	93	8-9	96
14	9-11	9-9	8-10	89	9-0	92
15	9-5	9-5	9-7	102	10-0	106
16	9-11	9-10	10-4	104	9-11	101
17	9-2	9-2	9-0	98	9-4	102
18	9-5	9-5	10-1	107	9-10	104
Mean	9.36	9.32	9.47	101.17	9.53	102.22
σ_m	.07	.07	.151	1.35	.127	1.36
D/σ_d	.56			.30		

* Months were expressed in tenths of years in order to describe the groups in convenient units.

Both the supporting and the falling-screen frames were constructed of brass. The former measured 72 cm. by 31 cm. over all; the sides were 1 cm. square with a groove of 3 mm. in which the guides moved. The cross piece measured 2 cm. in width; the bottom piece, 2.5 cm. by 2.5 cm.

The falling-frame measured 28 cm. by 41 cm. over all; the width of the pieces was 3 cm. Attached to the falling-frame were four hard rubber lugs which extended 3.5 mm. beyond the frame and fitted into the grooved guides of the supporting-frame. The falling-frame was pulled up by means of a cord and a brass pulley attached to the center top of the supporting-frame. On the back of the falling-frame was a 1 cm. extension into which could be placed a bristol board screen containing an exposure window. A 31 cm. by 18 cm. aluminum flap for holding the reading-material was attached to the center back of the supporting-frame by two flat springs.

The oil cylinder, measuring 50 cm. in length, was clamped into the bottom of the supporting-frame. The plunger of the oil cylinder was secured to the center bottom

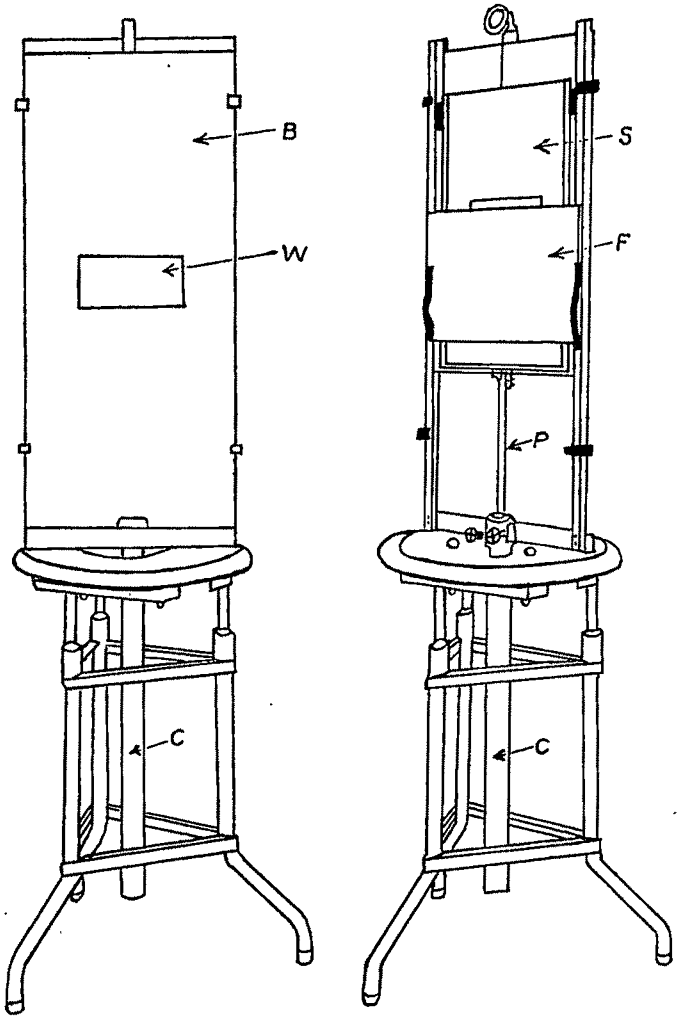

Fig. 1. Front and Back View of Exposure-Apparatus
B, bristol board front piece; W, exposure window; C, oil-filled cylinder; P, piston; F, flap for holding exposure material; S, falling-screen.

of the falling-screen by a screw attachment. Two screws determined the rate of flow of the oil and, consequently, the speed of the fall. The cylinder was filled with kerosene, the viscosity of which allows the fluid to be little affected by temperature changes.

The front piece of the apparatus, containing an 8 cm. by 15 cm. window, was dull neutral gray bristol board; it was fastened to the supporting-frame of the apparatus by four small brass clamps. Dull finish, white bristol board was used for the screen which carried the window in the falling-frame. Since the distance between the moving window and the material exposed was only 6.3 mm., the same as between the moving window and the front window, no detectable shadows resulted.

The apparatus was mounted on a stable Ever-Hold Automatic Adjustable Stool with three iron legs. The oil cylinder of the apparatus extended through the top of the stool and was held by a brass collar fitted with finger set screws. Two means of adjusting the apparatus so that the exposure window was on a level with *S*'s eyes were possible: adjustment of the height of the stool and of the height of the cylinder in the stool.

The apparatus was used only in well lighted rooms. It was placed facing the source of light and one meter from *S*. This placement proved satisfactory in that shadows on the reading material were not detectable.

Two kinds of exposure-cards, 21 cm. by 30.5 cm., were used: dull finish, white bristol board and glassine, a transparent paper. Lower case letters were used for all the reading material. The different kinds of printing used will be described in connection with the various experiments.

The words used in the experiments were chosen from a word list for primary grades compiled by Gates.[9] Words having the following structural forms were selected: (1) words which could be read meaningfully in a right-left direction (*e.g. was* and *top*) and which, therefore, could give opportunity for *complete reversals* in response; (2) words which could be read by interchanging the letters within the words (*e.g. who* and *bread*), giving opportunity for *partial reversals;* and (3) words which would not be meaningful if reversed unless they were mutilated in some way. Each non-reversible word, chosen as a control for a reversible word, was selected so that it was similar to the reversible one in Gates' rank (indicating the importance and probable frequency of the words in primary texts) and in word-form, *i.e.* in the number of and position within the word of ascending and descending determining letters and of indifferent letters. The final list of 50 words consisted of 15 completely reversible words with 15 controls for them and 10 partially reversible words with 10 controls for them. These 50 words were printed both in a right-left direction and left-right direction, making a total of 100 stimulus-words.

Procedure. Since the study necessitated the presentation of the same list of words a number of times, at least 4 days were allowed to elapse between successive presentations of the list by different exposure-methods in order to prevent the *Ss* from developing stereotyped responses. They were told at the beginning of the repeated presentations, "Today you are going to see some more words." The total time required for the presentation of all of the reading material was approximately 6 hr. per *S*. On any one day no *S* worked over one hour and during this time he was given rest-periods between successive 15-min. work-periods.

The responses of the *Ss* to the reading material exposed were made orally. This procedure had the advantage of (1) permitting adequate explanation, when necessary, in response to questioning by the examiner, (2) not complicating the responses by motor factors required in writing responses, and (3) eliminating chance elements which might result through any use of a multiple-choice response-method.

Mimeographed word-sheets listing the exposure-material were used by the experimenter in recording *Ss*' responses. Whenever the responses were an exact reproduction of the exposed material, either in a left-right or right-left direction, recording consisted in drawing arrows indicating the direction of the response. All other responses and remarks were given full written records.

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[9] A. I. Gates, A reading vocabulary for primary grades, *Teach. Coll. Bur. Publ., Col. Univ.,* 1926. 1-23.

The scoring system used was that of counting, as recorded, the frequencies of (1) reversal-errors, (2) all other response-errors combined, (3) "don't know" responses, and (4) correct responses. The responses other than reversals were recorded in order to indicate what happened to the latter in comparison with the former under the various experimental conditions imposed. Reversal-responses for words were classified under two categories: complete reversals and partial reversals. A complete reversal consisted in a right-left directional response either of the entire stimulus-word, *e.g. saw* for *was,* or of the word with a single letter substitution which made possible a meaningful reversal, *e.g. toe* for *eat.* Right-left responses to control-words, including those printed backwards, were also counted as complete reversals. A partial reversal consisted of a change in the order of the letters within a word, *e.g. bread* for *beard.*

I. EFFECT OF DIRECTION OF WORD-EXPOSURE

The object of Expermient I was to determine the effect of direction of exposure of words upon the frequencies of right-left word-responses and letter-arrangement responses.

Method and procedure. Three different methods of exposure were used: every word (1) was presented as a whole; (2) was presented letter-by-letter in a left-to-right direction; and (3) was presented letter-by-letter in a right-to-left direction. The first method was used in order to obtain responses which could be used as a standard with which to compare reading responses made under the various experimental conditions in the present and subsequent experiments which introduce various controlled factors.

Two different exposure-windows in the falling-screen were used in presenting the material. The window for the whole-word presentations was a horizontal slit, 5 cm. by 15 cm. The one for the letter-by-letter exposures was a diagonal slit, 17 cm. long and 0.7 cm. wide. This slit was the hypotenuse of a right isosceles triangle, the base of which was parallel to the bottom of the screen. In falling, this angular slit exposed only one complete letter at a time. By inverting this screen, both the left-right and right-left letter-by-letter exposures were accomplished. The falling-plate, carrying the screen, was attached to the oil control which was adjusted for a constant, predetermined rate of fall. The exposure-period allowed by the horizontal window for whole-word presentations was 2.17 sec.; that by the diagonal slit for directional presentation was 0.32 sec. per letter.

The order of presentation of the words was arranged so that the highest ranking words (according to Gates' list) were alternated with the lowest ranking ones. Reversible and non-reversible words were arranged in random fashion. The reversed form of any word was not presented at the sitting during which the original form was shown. The entire series of words was presented first by the whole-word exposure-method (hereafter referred to as the standard method). Then, alternations of 10 right-left with 10 left-right letter-by-letter exposures followed until the complete list of words had been presented in both directions. This alternation, it was hoped, would prevent a directional set from being induced by the experimental procedure or the possibility of *S*'s catching on to the fact that some of the control-words were printed backwards. Half of the *S*s in each group began this part of the work with the right-left and half with the left-right exposure-method.

Instructions. The instructions were designed to provide as a set the giving of word-responses. For the whole-word exposures the instructions were: "Now you are going to see some words through the window; watch very carefully and tell me the words as quickly as you can. Let us see how many you can get right." Two practice trials were given. For the controlled-direction exposures, *S* was told, "Now you will see some more words, but this time you will see only one letter at a time. Watch carefully and tell me the words as quickly as you can." Two practice trials were given.

In order to determine the correspondence between reversed word-responses and the order of the letters *S* perceived, and also to determine the direction of letter-groups perceived when no words were reported, *S* was asked, "What letters did you see?" The letters of all the correct responses were not requested since it was desired to prevent undue interference with the set to give word-responses. Yet it was desirable to include some correct words in the questioning to allay the suspicion of further questioning on the incorrect word-responses only.

Results. The results are presented in three divisions: (1) word-responses; (2) letter-responses to the words which were reversed; and (3) letter-responses to the words which the *S*s did not recognize.

(1) Word-responses: (a) Inter-group comparisons. Table IV indicates for each method of exposure the average number of complete and partial reversals and of other responses made by each group of *S*s for the different word-categories and for all words combined. The number of words reversed by the experimental group exceeded that made by the control in every word-category by amounts producing critical ratios between 1.20 and 6.47. When all the classes of words were combined,[10] the differences between the groups in reversal-errors are all statistically significant, the critical ratios falling between 3.87 and 8.18. The experimental group also made a significantly greater number of other errors and "don't know" responses than did the control group.

All *S*s of the experimental group reversed some words under each condition of word-exposure. This was not true for the control *S*s. Two of the experimental *S*s deviated considerably from the remainder of the group in the number of reversals, one making relatively many and one making relatively few under the three methods of exposure. Three of the control *S*s made relatively many reversals in comparison with the rest of the controls.

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[10] For all combinations, the original data were regrouped and the means and variabilities determined directly therefrom.

TABLE IV

Mean Number of Responses to Different Types of Words Under Three Different Methods of Slow Exposure for the Experimental and the Control Groups

(CR=complete reversal; PR=partial reversal; AE=all other errors; DK="do not know" responses; Cor.=correct responses.)

Words	Responses	Methods of exposure and groups of Ss														
		Whole-word exposure					Left-right, letter by letter					Right-left, letter by letter				
		exper.		control		D/σ_d	exper.		control		D/σ_d	exper.		control		D/σ_d
		M	σ_m	M	σ_m		M	σ_m	M	σ_m		M	σ_m	M	σ_m	
Completely-reversible (n=30)	CR	2.39	.36	.06	.05	6.47	2.33	.36	.22	.15	5.41	3.50	.91	—	—	3.85
	PR	.22	.10	—	—	2.20	.17	.09	—	—	1.89	.39	.14	—	—	2.79
	AE	4.17	.86	.39	.04	4.40	3.06	.60	.17	.09	4.74	2.78	.54	.72	.23	3.49
	DK	7.00	2.10	.78	.09	2.96	8.28	1.50	.39	.12	5.26	7.67	1.58	.61	.23	4.41
	Cor.	16.44	.99	29.39	.24	12.70	16.17	1.39	29.22	.29	9.19	15.67	1.28	28.67	.15	10.08
Partially-reversible (n=20)	CR	—	—	—	—	—	—	—	—	—	—	.06	.05	—	—	1.20
	PR	1.61	.25	.56	.12	3.75	1.78	.29	.50	.12	4.13	2.00	.36	.78	.13	3.21
	AE	2.67	.58	.17	.09	4.24	2.72	.41	.11	.08	6.21	3.44	.60	.61	.27	4.29
	DK	5.94	1.08	.22	.13	5.25	6.06	1.03	.44	.17	5.40	5.78	1.10	.28	.11	4.95
	Cor.	9.78	.96	19.06	.24	9.37	8.89	1.09	18.83	.24	8.88	9.06	1.01	18.33	.37	8.58
Control, printed left-right (n=25)	CR	.06	.05	—	—	1.20	.17	.09	—	—	1.89	.39	.18	—	—	2.17
	PR	.06	.05	—	—	1.20	—	—	—	—	—	.06	.05	—	—	1.20
	AE	3.89	.66	.06	.05	5.80	2.67	.44	.11	.08	5.69	3.11	.62	.50	.22	3.95
	DK	6.17	1.16	.11	.08	5.22	6.83	1.19	.44	.25	5.24	7.11	1.12	.17	.09	6.20
	Cor.	14.83	1.30	24.83	.16	7.63	15.33	1.21	24.44	.27	7.35	14.06	1.27	24.17	.27	7.78
Control printed right-left (n=25)	CR	5.61	.56	1.06	.53	5.91	5.28	.57	.89	.38	6.36	6.67	.79	1.78	.57	5.04
	PR	.28	.11	—	—	2.55	.17	.09	—	—	1.89	.11	.08	—	—	1.38
	AE	2.06	.45	.94	.30	2.07	3.06	.73	.94	.19	2.83	1.83	.49	1.50	.30	.58
	DK	13.17	1.02	7.61	1.69	2.82	11.72	1.23	7.89	1.50	1.97	12.39	1.09	7.83	1.58	2.83
	Cor.	3.78	.75	14.83	1.73	5.85	5.22	.69	15.28	1.67	5.56	4.17	.63	13.89	1.74	5.25
Total words (n=100)	CR	8.06	.73	1.11	.44	8.18	7.78	.72	1.11	.49	7.67	10.61	1.69	1.78	.57	4.96
	PR	2.17	.35	.56	.12	4.35	2.11	.33	.50	.12	4.60	2.56	.44	.78	.13	3.87
	AE	12.78	2.24	1.56	.43	4.92	11.56	1.52	1.33	.24	6.64	11.16	1.81	3.33	.70	4.04
	DK	32.72	4.20	8.61	1.66	5.33	32.89	4.12	9.17	1.72	5.32	32.94	4.42	8.94	1.78	4.76
	Cor.	44.28	3.76	88.17	2.05	10.25	45.67	3.47	87.89	1.96	10.58	42.94	3.12	85.17	2.45	10.64

(b) Intra-group comparisons. A comparison of the reversals made by the experimental *S*s under the standard exposure-method with those made by them under the right-left and left-right exposures indicates that the frequency of their reversal-responses was little influenced by the direction in which the words were viewed. Table V reveals these differences in critical ratio units, none of which is statistically significant.[11] Forced viewing of the words in a left-right direction decreased complete and partial reversals (as compared with the standard condition) by only 0.47 and 0.21 critical ratio units, respectively. Right-left viewing of the words increased reversals by 1.18 to 1.88 critical ratio units over those made under the standard condition and left-right exposure-condition.

TABLE V

COMPARISON IN CRITICAL RATIO UNITS OF THE NUMBER OF REVERSALS MADE UNDER THREE DIFFERENT METHODS OF SLOW EXPOSURE FOR THE EXPERIMENTAL AND THE CONTROL GROUPS

Exposure methods	Groups of Ss and types of reversals			
	experimental		control	
	complete	partial	complete	partial
Whole and left-right	.47	.21	—	.60
Whole and right-left	1.88	1.28	2.39	2.00
Left-right and right-left	1.83	1.18	1.60	2.00

That controlling direction of word-exposure had only a minor effect upon the almost negligible number of reversals made by the control group under the standard condition is shown in Table V. The differences, expressed in critical ratio units, between the reversals made by this group under the different exposure conditions are not statistically significant.

In short, the effect of direction of word-exposure upon reversals was small in the case of both the experimental and the control groups. Forced viewing of the words in the custom-determined left-right direction or in the opposite direction left the experimental group making about as many reversals and the control group making about as few reversals as each one made when direction of viewing the words was voluntary.

In order to investigate the possibility that reversals might bear a relationship to the *S*s' relative familiarity with the two forms of a reversible word, a tabulation was made to show how frequently every word used in this experiment was reversed. As an indication of probable familiarity, in terms of frequency of appearance of these words in primary texts,

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[11] The usual correction for correlated measures was made in the computation of the critical ratios for intra-group comparisons.

Gates' ranks of the words were used.[12] Admittedly this procedure could yield only an approximation to the frequency with which the *S*s in the present experiment had been exposed to the various words. It was not feasible, however, to count the number of times every word had appeared in the various books which the *S*s had actually used in school.

TABLE VI A

RELATION OF GATES' RANKS OF THE WORDS AND OF THE RATIO OF RANKS OF THE TWO FORMS OF EACH WORD TO THE NUMBER OF REVERSALS MADE FOR THESE WORDS UNDER THREE DIFFERENT SLOW EXPOSURE-CONDITIONS BY THE *S*s OF THE EXPERIMENTAL GROUP

Gates' rank (2)	Reversible words	Gates' rank (1)	Exposure manners and no. of reversals made			Reversible words	Gates' rank (2)	Exposure manners and no. of reversals made		
Gates' rank (1)	complete reversibles		whole-word	left-right	right-left	complete reversibles		whole-word	left-right	right-left
1.1	on	8	2	2	4	no	9	5	6	6
1.2	saw	64	8	7	8	was	77			
1.2	pat	730		1	1	tap	901		3	2
1.2	rats	350			1*	star	432		1	
1.5	lap	964			2	pal	1500+	1	1	2
2.4	pin	615				nip	1500+	3	1	4
2.4	pets	377			2, 1*	step	885	1		1
3.2	oh	279	5	7	7	ho	889	7	6	6
4.0	pan	367				nap	1500+	3		2
5.2	nuts	282				stun	1500+			
7.2	but	206			1	tub	1500+	4	2	6
9.8	ten	152			1	net	1500+		1	1
10.9	top	105			1	pot	1141	3	4	3
16.5	stop	67			2*	pots	1141	4*	3*	2, 3*
20.2	not	74	1			ton	1500+			1
	Sums		16	17	31			31	28	39
	Partial reversibles					Partial reversibles				
1.3	den	760			1†	end	970			
1.5	pear	923			1	reap	1500+			
2.0	read	146		1		dear	302		1	
2.1	ant	707	1	2		tan	1500+		1	1
2.4	left	523	2	1	3	felt	1259	4	2	2
3.4	meat	435				team	1500+	1	1	1
6.8	who	62	2	3	3	how	425	5	6	7
7.6	are	34				ear	258		2	2
8.0	bread	158				beard	1259	12	12	14
25.9	eat	50	2		2	tea	1296			
	Sums		7	7	10			22	25	27

* Partial reversals.
† Complete reversals.

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[12] The 1500 words in Gates' list are arranged in order of their importance for primary grades, frequency of appearance in representative primary texts being a factor in determining the ranks. The most frequent or important word has a rank of one; the least frequent, a rank of 1500.

The number of reversals made by the experimental group for each of the reversible words is given in Table VI A. The words in these tables are arranged in the ascending order of the ratios of the ranks of the two forms of each word and with the lower ranking, or more frequent, of the two forms appearing in the second column. It will be seen that when the ratios were greater than about three, *i.e.* when the words of a pair were disparate in rank, the experimental group reversed more of the higher ranking, or less frequent, words of the pairs, *i.e.* presumably the less well-known words of the pairs. The total number of reversals of the higher ranking words of the pairs exceeded those for the lower ranking ones under all conditions of word-exposure. This relationship did not hold, however, for all individual word-pairs. Apparently, two factors were in operation: the more difficult and unfamiliar reversible words

TABLE VI B

Relation of Gates' Ranks of the Control Words to the Number of Reversals Made Under Three Different Conditions of Slow Exposure for the Experimental Group

Control words	Gates' rank	Exposure and no. reversals			Control words	Exposure and no. reversals		
		whole-word	left-right	right-left		whole-word	left-right	right-left
is	3		1		si	15	12	15
man	28				nam	16	15	16
run	40			1	nur	7	8	10
red	49				der		1	1
she	53				ehs	7, 1*	8	12
into	135				otni	8, 4*	9, 3*	11, 2*
coat	157				taoc	0	1	
puts	168	1*		1*	stup	0		2
hat	185				tah		1	2
too	194				oot	14	12	13
ride	209			1	edir		4	3
drink	252			1	knird	2		3
leg	278				gel	6	5	5
late	340				etal	2	1	2
if	344				fi	14	12	14
set	351				tes			1
pie	390				eip	1		3
sat	394				tas	1		1
must	401				tsum		1	1
ship	469				pihs	2		
pen	525			1	nep	2	2	2
toe	642			1	eot	3		2
year	788				raey	1		2
yet	908				tey			
tag	949		2	2	gat			11
Sums		1	3	8		106	98	123

* Partial reversals; all others were complete reversals.

tended to be reported in their more familiar form (*e.g. top* for *pot*), and the majority of reversible words whose ranks would indicate that they are fairly similar in importance in primary texts were reversed in both forms. Hildredth has suggested that when practice has been about equal and simultaneous on both words of a reversal-pair, an "opposites-association bond" is formed and the child is thereby confused.[13] A confusion of this kind may account for the present findings.

The total number of times the experimental *S*s read every control word in a right-left direction is presented in Table VI B. When the control words were printed backwards, or in a right-left direction, the *S*s tended to read in a right-left direction more of the words with low Gates ranks than with high ones.

The foregoing word-analysis, preliminary as it is, suggests that words have different "reversal-values" for children of the reading level represented by the experimental *S*s, one determinant of which may be familiarity with the words as a function of the frequency of their appearance in primary texts.

Reversals made by the control *S*s were too few to warrant an analysis of them on the basis of word-rank.

(2) *Letter-responses to words reversed.* During preliminary experimentation there were some indications that word-responses did not always agree with the letter-arrangements which *S* apparently observed, or thought he was reporting. Consequently, it seemed desirable as a further step in analyzing the reversal phenomenon to determine the extent of agreement between the reversing of words and the letter-arrangements reported for those words.

Table VII gives the percentage of reversed word-responses whose letter-groups were also given in a right-left direction by the experimental and control groups. Both groups of *S*s showed a somewhat low correspondence between the reversal of words and the reversal of the letters of those words. The control group showed slightly more agreement than did the experimental, but the differences were statistically insignificant.

The rather low consistency between reversing words and the letters of those words is a result difficult to interpret because of the influence which the set to give words and the actual report of the words preceding the report of letters may have had on the direction of the letter-groups reported. The greater consistency of the control *Ss* may have been due to their resorting to spelling the words which they had reported, an act which they could perform to a more successful extent than could the experimental *Ss* who were known to be backward in spelling as well as in reading achievement.

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[13] Gertrude Hildreth, Reversals in reading and writing, *J. Educ. Psychol.*, 25, 1934, 14.

(3) Letter-responses to the words unknown to S. Since the letter-responses treated in the preceding section were given following word-responses and might have been influenced thereby, the *Ss* were also asked to report what letters they saw after each word-exposure for which they could give no word-response. The frequencies with which the letter-groups were given in the various directions (right-left, left-right or partial) were tabulated and expressed for each group and every exposure-condition in percentages of the total number of letter-groups given. The results are presented in Table VIII.

TABLE VII

NUMBER AND PERCENTAGE OF LETTER-GROUPS REVERSED OUT OF THE TOTAL NUMBER OF WORDS REVERSED

(CR=complete reversal; PR=partial reversal; TR=total number of all reversals.)

Methods of exposure	Reversal responses	Groups of Ss and responses: experimental word no.	experimental letter no.	experimental letter %	control word no.	control letter no.	control letter %	D/σ_d
Whole-word	CR	145	59	45	20	8	40	1.14
	PR	39	6	15	10	5	50	
	TR	184	65	35	30	13	43	
Left-right	CR	140	47	34	20	10	50	1.75
	PR	38	9	24	9	4	44	
	TR	178	56	31	29	14	48	
Right-left	CR	191	85	45	32	20	63	2.00
	PR	46	9	20	14	6	43	
	TR	237	94	40	46	26	56	

(a) Inter-group comparisons. The experimental group exceeded the control group by statistically significant amounts in letter-groups reported in a right-left direction for the standard and right-left exposure-methods. No difference occurred for the left-right exposure condition.

(b) Intra-group comparisons. In the case of the experimental groups, right-left exposure, as compared with the standard exposure, significantly increased the proportion of letter-groups given in a reversed direction (complete and partial reversals combined) while the left-right exposure had no effect at all. A comparison of the proportion of letter-groups reported in a partial and complete right-left direction with the proportion of words reversed reveals that the latter greatly exceed the former. Of the

total number of stimulus-words reported as words, 14%, 13%, and 18% were reversed (all words combined) under the conditions of standard, left-right and right-left exposures, respectively. The corresponding proportions for letter-arrangements were 3%, 3%, and 9%.

The control *S*s reversed no letter-groups at all when they were exposed by the standard method. Both of the other two methods of exposure resulted in a few reversals, those for the left-right exposure method slightly exceeding those for the right-left method. No differences resulted for this group of *S*s between the proportion of letter-groups reversed in this experiment and the proportion of word-responses given in a reversed direction; they were both small.

TABLE VIII

NUMBER AND PERCENTAGE OF LETTER-GROUPS REVERSED OUT OF THE TOTAL NUMBER OF LETTER-COMBINATIONS

(Cor.=correct responses; CR=complete reversals; PR=partial reversals; TR=total number of all reversals.)

		Groups of *S*s				
Methods of exposure	Response type	experimental		control		D/σ_d
		no.	%	no.	%	
Whole-word	Cor.	506	97	156	100	3.75
	CR	10	1	—	—	3.33
	PR	7	2	—	—	2.50
	TR	17	3	—	—	3.75
Left-right	Cor.	499	97	142	97	.00
	CR	6	1	2	1	.00
	PR	9	2	3	2	.00
	TR	15	3	5	3	.00
Right-left	Cor.	464	91	133	98	3.89
	CR	32	6	1	1	3.57
	PR	12	3	1	1	1.82
	TR	44	9	2	2	3.89

The very few letter-group reversals of the control group in this experiment as compared with the number of reversals following reversed word-responses suggests that giving word-responses (in a reversed direction) prior to letter responses influenced the direction of giving the letters. This influence, as has already been suggested, may have been exerted through the *S*s' attempts to spell the words which they had given.

II. EFFECT OF ELIMINATION OF REACTIVE EYE MOVEMENTS

This experiment was designed to determine what influence eliminating reactive eye movements would have upon reversals. The reading material consisted of the same 100 stimulus-words which were used in Experiment I.

Apparatus. The falling-screen of the exposure-apparatus was detached from the oil control and allowed to fall freely upon 4-in. pieces of sponge rubber wrapped around the lower side-pieces of the supporting-frame. A horizontal slit, 8 cm. by 12 cm., formed the window of the falling-plate and gave an exposure of 50 ms. A cross marked upon the center of the exposure screen was fixated by *S* before the exposure of every word. The apparatus was placed one meter from the *S;* the visual angle subtended by the longest word at this distance measured 2°.

TABLE IX

MEAN NUMBER OF RESPONSES TO DIFFERENT TYPES OF WORDS EXPOSED UNDER CONDITIONS OF RAPID EXPOSURE

(CR=complete reversal; PR=partial reversal; AE=all other errors; DK="do not know" responses; Cor.=correct responses.)

Words	Responses	Groups of Ss				D/σ_d
		experimental		control		
		M	σ_m	M	σ_m	
Completely-reversible (n=30)	CR	1.28	.19	.06	.05	6.10
	PR	.11	.08	—	—	1.38
	AE	6.00	.82	2.17	.41	4.16
	DK	11.50	1.32	.44	.17	8.32
	Cor.	11.11	1.27	27.33	.49	11.93
Partially-reversible (n=20)	CR	.06	.05	—	—	1.20
	PR	.78	.25	.61	.14	.81
	AE	4.22	.53	1.39	.49	3.93
	DK	8.33	.97	.28	.13	8.21
	Cor.	6.61	.79	17.72	.53	11.69
Control printed left-right (n=25)	CR	.33	.28	—	—	1.18
	PR	—	—	.06	.05	1.20
	AE	4.67	.57	2.11	.35	3.82
	DK	8.00	1.01	.50	.17	7.35
	Cor.	12.00	1.21	22.33	.45	8.01
Control printed right-left (n=25)	CR	2.61	.35	.94	.28	3.71
	PR	.06	.05	—	—	1.20
	AE	5.11	.98	4.56	.59	.48
	DK	13.89	1.21	5.39	1.10	5.41
	Cor.	3.33	.61	14.11	1.44	6.91
Total (n=100)	CR	4.28	.59	1.00	.28	5.05
	PR	.94	.26	.67	.16	.87
	AE	20.00	2.12	10.22	1.30	3.93
	DK	41.72	3.79	6.67	1.31	8.74
	Cor.	33.06	3.51	81.44	2.57	11.12

Instructions. The instructions were as follows: "You are going to see some words for a very short time; when I say 'ready,' you look at the cross in the window and then you will see a word there. Be sure not to move your eyes until after you have seen the word, or you might not see it. Let us see how many words you can get." Four practice trials were given.

Results. (a) Inter-group comparisons. The responses made by each group of *S*s and the differences between the groups for the word-categories taken both separately and together are presented in Table IX. For all the word-categories combined, it will be seen that the experimental group significantly exceeded the control group in complete reversals but not in partial reversals. The former group also made a significantly greater number of other errors and 'don't know' responses than did the latter.

(b) Intra-group comparisons. A comparison of the number of reversals made under the conditions of this experiment with those made on the same words during the standard exposure (Experiment I: whole-word, slow exposure) was made for each group of *S*s to determine the effect of elimination of reactive eye movements with materials of the kind used here. The results in critical ratio units are given in Table X.[14]

TABLE X

INTRA-GROUP COMPARISONS IN CRITICAL RATIO UNITS OF REVERSALS MADE DURING RAPID AND SLOW EXPOSURE OF SAME WORDS

Words	Reversal	Groups of Ss	
		experimental	control
Completely-reversible (n=30)	Complete	2.92	.00
	Partial	1.38	.00
Partially-reversible (n=20)	Complete	1.10	.00
	Partial	2.27	.33
Control, printed left-right (n=25)	Complete	.93	.00
	Partial	1.10	1.10
Control, printed right-left (n=25)	Complete	5.08	.22
	Partial	1.69	.00
Total (n=100)	Complete	5.56	.22
	Partial	3.25	.69

In every word-category except one, the experimental group reversed more words during the standard exposure than during the rapid exposure which eliminated reactive eye movements. For the total list of 100 words the differences with respect to both partial and complete reversals were significant.

Although the number of reversals made by the experimental group during the rapid exposure was significantly less than was made by it during slow exposure, it should be noted that the total number of incorrect (including "don't know") responses was not decreased. Examination and comparison of the original records revealed that the reduction in reversals was due to a compensatory increase in the number of more serious errors or of complete failures probably because of the shortened time for recognition. Thus, elimination of reactive eye movements cannot be said to have decreased reversals in terms of converting them into correct responses.

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[14] Intra-group correlations employed in the critical ratio computations ranged from + 0.05 — .163 to + 0.49 ± .124 for all words combined and from — 0.18 ± .158 to + 0.64 ± .097 for the separate word categories.

The direction of the control *Ss'* reading was little affected by elimination of reactive eye movements. The differences were neither consistent in direction nor statistically significant.

III. EFFECT OF ACCENTUATION OF WORD-FORM

In the third experiment an investigation was made of the suggestion that lack of attention to word-form, *i.e.* the visual appearance of a word, produces reversals and that reversal-errors disappear when methods are used which demand a high degree of immediate attention to word-form.

Procedure. Thirty words (10 reversible words printed in both directions, *e.g. was* and *saw;* 10 control words printed in a left-right direction) were selected for use in this experiment from the list of 100 words used in Experiments I and II. The words were printed in outline by a commercial artist. Exploratory experimentation determined the extent to which the separate letters had to be mutilated by outlining to cause 9-yr.-old children considerable difficulty in reading them in isolation.

A check of the difficulty encountered by the *Ss* was made by recording their responses to 10 representative isolated letters printed in outline. The experimental *Ss* failed on the average to identify 34% of the letters; the control group, 32%. The isolated letter 'a,' for example, was almost always called 'c i.' Apparently both groups experienced sufficient difficulty in reading the separate letters to necessitate giving extra attention to word-form in reading the words.

The words were exposed on the falling-screen apparatus. The same exposure-window and exposure-time per word were used here as in Experiment I for the standard exposure.

Results: (a) Inter-group comparisons. The responses to the words in outline print are given in Table XI. It can be seen that the experimental group exceeded the control by statistically significant amounts on both complete and partial reversals.

The differences between the two groups of *Ss* when the responses to the same 30 words in the present experiment were tabulated for the experiments using fast and slow whole-word exposures of the words in Goudy print are also shown in Table XI. The differences between the experimental and control groups were always in the same direction and were statistically significant in every instance except one (partial reversals made to the words in Goudy print, fast exposure).

(b) Intra-group comparisons. Intra-group comparisons of the frequencies of reversals made under the conditions of the present experiment and

those made in the experiments using Goudy print fast and slow exposures, *i.e.* when reactive eye movements were and were not eliminated and when word-form was probably not accentuated, appear in terms of critical ratios in Table XII. Accentuation of word-form, as produced in this study, had no significant effect on the number of reversals made with the same series of words by either group of *S*s. The tendency on the part of the group handicapped in reading to make reversal-errors and of the control group to refrain from making them was persistent despite the various experimentally controlled factors which have thus far been studied.

TABLE XI

RESPONSES TO DIFFERENT TYPES OF PRINTING UNDER DIFFERENT EXPOSURE-RATES

(CR=complete reversal; PR=partial reversal; AE=all other errors; DK="do not know" responses; Cor.=correct responses.)

Printing and exposure	Responses	Groups of Ss: experimental M	experimental σ_m	control M	control σ_m	D/σ_d
Outline, slow	CR	1.06	.19	.17	.09	4.24
	PR	.61	.12	—	—	5.08
	AE	6.50	.97	1.56	.40	4.70
	DK	5.78	1.43	.22	.13	4.12
	Cor.	16.06	1.28	28.06	.45	8.82
Goudy, slow	CR	.83	.23	—	—	3.61
	PR	.56	.14	—	—	4.00
	AE	5.17	.94	.22	.13	5.21
	DK	5.94	1.29	.17	.09	4.47
	Cor.	17.50	1.59	29.61	.19	7.57
Goudy, rapid	CR	.83	.21	.05	.05	3.55
	PR	.33	.14	.05	.05	1.87
	AE	5.28	.62	1.72	.41	4.81
	DK	9.28	1.01	.33	.14	8.77
	Cor.	14.28	1.19	27.83	.53	10.42

TABLE XII

COMPARISON IN CRITICAL RATIO UNITS OF REVERSALS MADE UNDER DIFFERENT CONDITIONS OF PRINTING AND EXPOSURE-TIME

Printing and exposure	Reversal	Groups of Ss: experimental	control
Goudy, slow and rapid	Complete	.00	1.09
	Partial	1.53	1.00
Goudy, slow and outline, slow	Complete	.96	1.89
	Partial	.23	.00
Goudy, rapid and outline, slow	Complete	1.05	1.20
	Partial	1.56	1.00

IV. Effect of Mirrored Printing

Whether or not mirroring words would alter reading direction was investigated in this part of the study.

Procedure. The 100 words used in the first two experiments were mirrored for the present purpose by inverting laterally sheets of glassine, a transparent paper, upon which the words had been printed, and pasting them on bristol board exposure-cards. The mirroring, of course, reversed not only the lateral direction of the word but also the orientation of the letters themselves within the word. The type and spacings were exactly like those used in the experiments employing ordinary Goudy print. The instructions, exposure-period, and means of exposure were identical with those for the standard method in Experiment I. The *Ss*' responses were recorded in terms of the directions in which the words were read, *i.e.* from right-to-left or from left-to-right.

Results: (a) Inter-group comparisons. Table XIII shows the average number of words of the various categories read in each direction by the experimental and control groups. (The control-words are designated in the table according to the direction in which they could be read meaningfully, *i.e.* in terms of letter order, after they were mirrored. Control words printed right-left, for example, could be meaningfully read only in a right-left direction unless *S* mutilated the words or read them as nonsense syllables). For all groups of words the control *Ss* exceeded the experimental ones in the number of words reported in a left-right direction, the significance of the differences for 4 out of 5 word-groups being between 2.46 and 3.13 critical ratio-units. The control group also read more words in a right-left direction than did the experimental group except in the

TABLE XIII

Mean Number of Responses to Mirrored Words

Words	Direction of responses	Groups of Ss				D/σ_d
		experimental		control		
		M	σ_m	M	σ_m	
Completely-reversible (n=30)	L-R	8.06	1.00	12.06	.80	3.13
	R-L	3.00	.49	3.89	1.09	.75
Partially-reversible (n=20)	L-R	2.39	.46	3.00	.41	1.00
	R-L	2.28	.42	3.83	1.09	1.32
Control, printed right-left (n=25)	L-R	.83	.35	2.50	.58	2.46
	R-L	6.78	.65	6.39	1.39	.25
Control, printed left-right (n=25)	L-R	9.56	.72	12.50	.85	2.65
	R-L	.39	.23	.89	.36	1.16
Total (n=100)	L-R	20.83	2.44	30.06	1.90	2.99
	R-L	12.44	1.33	14.83	3.34	.67

case of control words printed right-left, but none of these differences was statistically significant.

The results of this experiment were also expressed for each group of *S*s in terms of the proportion of the number of mirrored words read in a left-right or right-left direction to the total number of words read. As Table XIV reveals, the experimental group surpassed the control group in the proportion of words read in a right-left direction for the whole list of words and for two out of the four separate word-categories. Only one of these differences, however, was significant; namely, the one for control words printed in a right-left direction.

TABLE XIV

NUMBER AND PERCENTAGE OF MIRRORED WORDS READ IN DIFFERENT DIRECTIONS

Words	Direction of responses	Groups of *S*s						D/σ_d
		experimental			control			
		no.	%	σ_p	no.	%	σ_p	
Completely-reversible	L-R	145	73	.031	217	76	.025	.75
	R-L	54	27	.031	70	24	.025	.75
Partially-reversible	L-R	43	51	.055	54	44	.045	.99
	R-L	41	49	.055	69	56	.045	.99
Control, right-left	L-R	15	11	.026	45	28	.036	3.86
	R-L	122	89	.026	115	72	.036	3.86
Control, left-right	L-R	172	96	.015	225	93	.016	1.43
	R-L	7	4	.015	16	7	.016	1.43
Total	L-R	375	63	.020	541	67	.016	1.54
	R-L	224	37	.020	270	33	.016	1.54

(b) Intra-group comparisons. To determine the effect of mirroring words on the reading responses of both groups of *S*s, a direct comparison of the results of the present experiment (Table XIII) may be made with those obtained in the first experiment (Table IV) when the same words in ordinary printing were exposed under identical conditions (standard or whole-word exposure). Mirroring the words decreased the number of left-right responses for both groups of *S*s as compared with the number of such responses given to the non-mirrored words. The extent of the decrease was greater for the control than for the experimental group. The right-left responses were increased by mirroring to a significant extent for the former group but to only a small extent for the latter one. Mirroring the words had, thus, far less effect on the responses of the experimental group than on the control group. The right-left responses of the experimental group persisted to about the same extent despite the decrease in the total number of words read. Mirrored printing, while

increasing significantly the number of right-left responses of the control group, decreased the total number of words read by that group to a greater extent than for the experimental group.

In an attempt to throw some light on the meaning of these results a word-analysis on the basis of frequency of response-direction was made. For the experimental group the ratio of the left-right to the right-left reports for the completely reversible words printed so that the letters comprising the words with the lower Gates' ranks were from left-to-right was 6.45; that when the lower ranking word was printed right-left was 1.41. The corresponding figures for the control group were 2.46 and 2.18. Apparently, then, one factor which was operating in the case of the experimental *S*s was their familiarity with certain words. They may have had a tendency to make words that they knew out of the letter-groups shown and, when these groups were printed right-left, a response classifiable as a reversal occurred. Relative familiarity with the various reversible words apparently was not related to the responses of the control group. Analysis of control words on the basis of the relationship of rank with direction of reading-response was unrevealing of any trends.

A probable explanation of the results of this experiment will be presented in the Discussion; the control *S*s apparently had a set to correct for the mirroring.

V. DIRECTION OF LETTER-READING

This experiment aimed to determine (1) the tendency to reverse lower case single letters and (2) the direction of reading horizontal rows of letters.

Procedure. Pursuant of the first aim 20 letters printed in 24 point bold Goudy type were presented in both a mirrored and an unmirrored fashion.[15] The letters were printed five in a column on bristol board cards and were exposed in succession for 0.65 sec. each by a horizontal slit, 1.5 cm. by 1.5 cm., in the falling-screen. *S* was asked to name every letter as soon as it was exposed.

The second part of the experiment dealt with the direction of reading rows of 'indifferent' lower case letters, *i.e.* letters which had no ascending or descending parts which might cause some to stand out more than others and thereby influence the direction of the reading. Six letters printed in 24 point bold Goudy type with 24 point spacings, both mirrored and unmirrored, were arranged in three different orders.[16] Each grouping of letters was exposed by means of the same window and with the same exposure-period as were used in the first experiment for the standard presentations. *S* was told, "Now you are going to see some letters through the window;

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[15] b, a, p, n, s, d, c, z, q, t, n, y, u, o, m, v, w, e, g, f.

[16] avciox; xoicva, civxoa.

name them for me as quickly as you can." No preëxposure fixation-point was used for obvious reasons.

Results. The mean number of lateral reversals of the singly presented unmirrored letters was 1.28 and 0.22 for the experimental and control groups, respectively. The significance of the difference was 3.58. For the same letters mirrored, the mean number read in right-left orientation was 17.22 for the experimental and 16.17 for the control group with a critical ratio of 1.84.

No significant differences were obtained between the two groups in the frequency of reading rows of letters from right to left. When the letters were unmirrored, the mean number of rows read in a right-left direction was 0.11 for each group of *Ss*. The experimental group read on the average 0.50 rows of mirrored letters from right to left; the control group, 0.39. The resulting critical ratio was 0.33.

VI. Reproduction of Designs

That individuals having reading disabilities may be inferior in form memory has been suggested by some experimenters. In order to compare the *Ss* in this study on form memory, especially on direction of reproducing forms, geometric designs unfamiliar to the *Ss* were used.

Procedure. Ten Seashore geometrical designs were presented to the *Ss*.[17] Every design was drawn on a bristol board card and was exposed on the apparatus under the same conditions as obtained in Experiment II (rapid exposure). *S* was given paper and pencil and was instructed to draw what he saw after each exposure.

Results. The experimental group (Table XV) exceeded the control group in the frequency of making the following responses: (1) horizontal inversions; (2) vertical inversions; (3) horizontal and vertical inversions combined in one response; (4) all errors other than inversions;

TABLE XV

Mean Number of Different Types of Reproduction of 10 Seashore Designs

Responses	Groups of *Ss* experimental		control		D/σ_d
	M	σ_m	M	σ_m	
Horizontal inversion	.50	.17	.22	.10	1.44
Vertical inversion	.06	.01	.11	.07	.66
Horizontal & vertical	.33	.14	.06	.05	1.79
All other errors	3.11	.74	2.61	.36	.60
Total	4.00	.58	3.00	.37	1.47

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[17] C. E. Seashore, *Elementary Experiments in Psychology,* 1908, 131.

and (5) all errors combined. The control group made more vertical inversions than did the experimental group. None of the differences, however, was statistically significant.

DISCUSSION

The assumption that reversals result from right-to-left visual observation of words was not upheld by the results of this study. In Experiment I, forced viewing of words in a right-left direction produced only a statistically insignificant increase in the number of reversal-responses made by both the experimental and the control groups, whereas forced left-right observation resulted in an insignificant decrease in reversals of the former group and had no effect upon the latter one. Experiment II, which allowed viewing of the whole word while preventing reactive eye movements likewise did not eliminate the experimental group's reversals. The persistence of the experimental group in making relatively many reversals and of the control group in making relatively few despite control of direction of observation and despite elimination of reactive eye movements is evidence, then, that some causal factor or factors other than direction of visual impression as such must be sought to account for reversals.

An explanation of reversals in terms of lack of attention to word-form also appears inadequate. The particular means of insuring increased attention to word-form used in this study (outline printing; Experiment III)[18] produced no material alteration in the experimental group's reversals nor in the control group's relative freedom from such responses.

Experiment IV did not support the expectation that the proportion of right-left reports of the experimental group would be substantially increased by mirroring the words. Only a small increase occurred. The control group, on the one hand, showed a significant increase in right-left reading with the result that this condition of word-exposure was the only one in which the experimental group did not significantly exceed the control in the number of right-left word-responses.

An interpretation of this increase in the right-left responses of the control group and the little change in the responses of the experimental group can only be conjectured at this time. It seems possible that in spite of the number of years of exposure to reading materials the experimental

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[18] If the tachistoscopic exposure in Experiment II accentuated word-form at all, it certainly did not do so to the extent that the outline printing of Experiment III did, for the *Ss* could almost invariably report all the letters of the stimuli shown in the former but not in the latter experiment.

Ss were still in a stage of reading in which responses to letters may be made regardless of their orientation or position in the word. Even the severe alteration produced by mirroring the words did not change their proportion of right-left responses. The control group, on the other hand, being in a more advanced stage of reading, presumably should have been responding not only to the letters but also to the position of the letters within the words. With the letter orientation reversed or mirrored, the more mature reaction may have been that of reading the words in a corresponding direction. In other words, it is possible that the control group, recognizing (because of its greater familiarity with the words) that the words were printed backwards, may have had a set to read them accordingly, *i.e.* to correct for the mirroring. Certain observations made during the experiment substantiated this view. The number of control *Ss* who expressed difficulty with the mirrored words exceeded the number of experimental *Ss* who made similar report. Likewise, many more of the former made such remarks as: "these words are backwards," which was of course correct; and "I think thay are up-side-down," which at least indicated recognition of a peculiarity. These individuals attempted to read the words in a right-left direction. One boy asked which way he should read them and, being given no satisfactory reply, stated, "I guess I should read them backwards." He proceeded to do so.

It should be stated that the results of this experiment probably are not directly comparable with those obtained by other investigators who used mirrored material consisting of paragraphs, who worked on *Ss* more alike in reading age, and who took measurements in time rather than errors.[19] The reading material of the present experiment consisted, it will be remembered, in short words only, which were undoubtedly very familiar to the control *Ss*.

The possibility that some learning or stereotyping of responses may have occurred during the repeated presentations of the words should perhaps be mentioned. As has been previously stated, however, at least four days were allowed to elapse between successive presentations in order to minimize such a result. Evidence of learning was not apparent.

In consideration of the general results of the six experiments here reported, the writer wishes to propose that the reading behavior noted in this study may be accounted for in terms of characteristic stages in the process of learning to read, stages which are regularly reached and passed more or less quickly by groups made up of individuals such as our control and experimental *S*s. As habitual methods of attacking words are established, a series of stages is defined. In our control group the school-taught habit of systematically beginning in the custom-determined left-right direction, and thereby always attending to the position of letters within a word as well as to the orientation of the letters, has been more or less adequately set up. The experimental group has not yet reached this stage, with the result that its members frequently respond in terms of partial recognition and association cues which cause errors of various kinds including the so-called reversals. According to this interpretation we would expect to find reversal-errors in an early stage of the learning of average children whose reading progress has followed the usual course at the usual rate.[20] The child with a reading handicap may remain abnormally long at this normal learning stage because of a complex of undesirable characteristics such as those found for the present experimental group, or because of some basic factor or factors which are as a whole or in part responsible for these characteristics.

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[19] Marion Monroe, Method for diagnosis and treatment of cases of reading disability, *Genet. Psychol. Monog.*, 4, 1928, 335-456.

Descriptions of the present *S*s with respect to behavioral factors which many workers have investigated in reading disability indicate that as a cross-section of children of average intelligence and from the general school populations retarded with respect to reading, the experimental group of this study may fairly be considered typical.[21] Because of the nature and requirements of this investigation, no actual non-reader incapable of recognizing any words was included.

SUMMARY

As an attack on the nature of the reading behavior involved in making reversals of individual words, measurements were made of the effect of six experimental procedures upon the frequency and consistency of reversal-errors. The first four experiments provided (I) control of direction of word-exposure, (II) elimination of reactive eye movements, (III) accentuation of word-form, and (IV) mirrored printing of the material. The two further experiments pertained to (V) the frequency of reading single letters in reversed orientation and of reading horizontal rows of letters in a right-left direction, and (VI) the frequency of reproducing certain geometric designs in orientations differing from the stimulus presentations. School children, boys, with and without reading handicaps (experimental and control groups, respectively, with 18 in each group) served as *S*s.

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[20] Several studies in the literature on reading, writing, or copying support this point of view although in general they were performed on children representing wide ranges of abilities and were, in the main, cross sectional (L. G. Fildes, *op. cit.;* Gertrude Hildreth, Success of young children in number and letter construction, *Child Develop.,* 3, 1932, 1-14; Reversals in reading and writing, *J. Educ. Psychol.,* 25, 1934, 1-20. Marion Monroe's study of poor readers and normal readers of the same average reading grade should be mentioned in this connection (Methods for diagnosis and treatment of reading disability, *Genet. Psychol. Monog.,* 4, 1928, 335-456.). While both of her groups of *S*s made reversals and while such errors decreased as the reading age of the groups increased, the poor readers exceeded the normal ones in the frequency of reversals. It seems possible, however, that the reversal frequencies of a control and an experimental group of the same reading age may be found to be fairly comparable in reversals if the groups are selected from school populations and are strictly equated in IQ.

[21] The results will be presented in subsequent papers.

A falling-screen exposure-apparatus was used for the presentation of the materials. Speed of fall of the screen was controlled by means of a piston, moving in an oil filled cylinder, which could be attached to, or disconnected from, the screen.

The following results were obtained.

(1) Control of direction of word-exposure. The experimental group significantly exceeded the control group in the number of partial and complete reversals under all three methods of word-exposure, *i.e.* whole-word (standard) exposure, left-right letter-by-letter exposure, and right-left letter-by-letter exposure. Forced viewing of the words in a right-left direction produced only a slight and statistically insignificant increase in the number of reversals made by the experimental and control groups, where forced left-right observations resulted in an insignificant decrease in reversal-responses of the former group and had no effect upon the latter one.

An analysis of the individual words with respect to frequencies of reversals indicated that the *S*s' probable familiarity with the words was related to the frequency with which given words were reversed.

When the *S*s were asked to report the letters they had seen for the words they reversed or did not know, the following results were obtained. The experimental group significantly exceeded the control in the proportion of letter-groups reversed for the words they did not know. The control *S*s (apparently because they attempted to spell the words they had given) exceeded the experimental, though not significantly, in the proportion of letter-groups given in a right-left direction following reversed word-responses. Neither group, however, reported as many letter-groups in a right-left direction as it did words.

(2) Elimination of reactive eye movements. Elimination of reactive eye movements left the experimental group still significantly exceeding the control group in the frequency of complete reversals of words. This exposure-method decreased significantly the number of reversals made by the experimental group as compared with the number it made when reactive eye movements were permitted in viewing the same words (standard exposure condition, Experiment I) but had no effect upon the control group. Analysis indicated, however, that many of the words which the experimental group reversed during the standard method of exposure evoked other more serious errors or complete failures during the fast exposures which eliminated reactive eye movements. So elimination of reactive eye movements did not decrease reversals by converting them into correct responses.

(3) Accentuation of word-form. When word-form was accentuated by outline printing, the experimental group again significantly exceeded the control in the number of complete and partial reversals. Intra-group comparisons of the number of reversals of the words in this experiment with those made under the conditions of the foregoing experiments showed that none of the variations in exposure produced significant differences for either group of *S*s.

(4) Mirrored materials. Mirroring the words had little effect upon the frequency of right-left reports made by the experimental group while it increased those made by the control group as compared with the number it made under the other experimental conditions of this study. That this result is not contradictory to the other results of the study is presented in the Discussion (p. 557); there were certain indication that the control group was set for backwards words and proceeded to attack them accordingly.

(5) Letters. The experimental group significantly exceeded the control group in lateral inversions of separate letters in ordinary print and surpassed it also in the number of mirrored letters read in a right-left direction although this difference was not significant. Practically no difference between the groups occurred in the direction of reading rows of letters.

(6) Geometric designs. The experimental group exceeded the control group in the number of horizontal inversions and double inversions (horizontal and vertical) of geometric designs while the control group surpassed the experimental one in the number of vertical inversions. None of the differences was significant, however.

This study, in short, indicated that reversals of words were remarkably persistent for the *S*s with reading disability despite the experimental control of factors which have heretofore been suggested as causes of reversals. The control group's maturity of reading response was evident throughout.

It is proposed that the reading behavior displayed in this study may be accounted for in terms of characteristic stages in the process of learning to read. The control group was in the stage in which the habit of systematically attacking words in a left-to-right direction had been fairly well established. The experimental group had not yet reached this stage, with the result that it was responding to partial recognition and association cues which cause various errors including the so-called reversals.

DISCRIMINATION WITHOUT AWARENESS

By JAMES GRIER MILLER, Harvard University

Most of the important concepts of clinical psychology have not yet been found susceptible of laboratory investigation, or at least have not been subjected to it. Nevertheless such undertakings are likely to be most valuable in lessening the gap between the approaches of the clinic and of the laboratory to human behavior. The notion of unconscious behavior is of utmost importance in clinical psychology, and it has from time to time demanded the attention of laboratory investigators. Sparsely distributed throughout the literature, investigations are to be found which have some more-or-less distant bearing on this question.[1]

HISTORY

The experimental work on this problem can be grouped under the following headings: (1) subliminal perception; (2) subliminal learning and conditioned response; (3) the effect of attitude on psychophysical measures; (4) unreported factors in problem solving; (5) liminal measures of attention and distraction; (6) reportable effects of subliminal neural stimulation; (7) the rôle of subliminal stimulation of neurones in inhibition and facilitation, including increase of sensitivity; and (8) the effect of the manner of response on the limen.

It is impossible to consider here the history of the investigations in these several fields. We can, however, summarize a few of the important researches dealing with the first problem listed: *subliminal perception.* In 1863 Suslowa reported his work with the esthesiometer.[2] He found that under electrical stimulation esthesiometer sensitivity was lowered, even when the current was subliminal. Peirce and Jastrow, in 1884, reported experiments with weights less than one jnd. apart.[3] They discovered that judgments of which was heavier were more often right than wrong, though made with no confidence whatsoever.

In 1898, Sidis reported four series of experiments.[4] In the first series he was *S*, using only his right eye, which was amblyopic. Nevertheless he guessed correctly in 71.5% of the cases whether a character presented was letter or figure, so that his results were 21.5% above chance. In the second series, he guessed both whether the object was letter or figure and also what the specific figure was, with successes well above chance. In the third series, he got like results from *S*s with normal vision

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[1] The author is indebted to Professor Edwin G. Boring for his constant advice and assistance. He presented a preliminary report of this research to the Society of Experimental Psychologists on April 7, 1938. The apparatus used in this study was purchased by a grant-in-aid by the Maria E. McMaster Fund for Psychic Research.

[2] M. Suslowa, Veranderungen der Hautgefüle unter dem Einflusse electrischer Reizung, *Zsch. f. Rationelle Med.*, 18, 1863, 155-160.

[3] C. S. Peirce and J. Jastrow, On small differences in sensations, *Mem. Nat. Acad. Sci.*, 3, 1884, 73-83.

[4] B. Sidis, *The Psychology of Suggestion*, 1898, 1-386.

when the cards bearing the characters were held at such a distance that the *Ss* said they could not differentiate the characters. In the final series, proper names on cards held at a like distance were distinguished better than chance. Sidis stated that "all these experiments tend to prove the presence within us of a secondary subwaking self that perceives things which the primary waking self is unable to get at."[5]

In 1900 Dunlap used a modification of the Müller-Lyer illusion in which the angular lines were subliminal shadows.[6] He found that the illusion was present but slight. In 1904 he reported that *Ss* perceived the interruption of inaudible sounds.[7] Titchener and Pyle repeated Dunlap's experiment with the Müller-Lyer illusion, and reported opposite results in 1907.[8] They added further that they could not find any correlation between the degree of imperceptibility of the arrow heads and the strength of the illusion. Manro and Washburn in 1908 made cards of the Müller-Lyer illusion with inked horizontal lines and lightly pencilled arrows.[9] Following Sidis's procedure, the stimuli were held at such a distance that the pencil lines were not visible. Of ten *Ss*, eight got no illusion, and two got some. The authors made the questionable conclusion that these results confirmed those of Titchener and Pyle. At about the same time Stroh, Shaw, and Washburn[10] carried on modifications of Sidis's experiments. With slightly better control they repeated his letter differentiation experiment, and also whispered the letters subliminally. All the results corroborated Sidis and indicated subliminal perception.

Perky's experiment, in 1910, bears an indirect relation to this problem, and also to the procedure reported in this paper.[11] She was trying to discover whether subjective images and objective presentation could be differentiated. She told the *Ss* to imagine certain objects and to project them on a ground glass screen which was in front of them. At the same time, unknown to them, she projected these images on the screen from behind at a supraliminal intensity (the limen was determined at the end of the experiment), but each *S* thought he saw only subjective images. He was receiving visual stimulation above his normal limen, but he was unaware of it.

In 1913 a student of Hollingworth's carried on an experiment using essentially Manro and Washburn's procedure, and found that 17 out of 20 *Ss* got the illusion.[12] The results were interpreted to indicate wide individual differences of susceptibility to the suggestion of illusion. In 1916 de Laski reported on the basis of experiments that: "It seems certain that subliminal separations of the aesthesiometer-points are discriminable. . . . Discrimination appears to be based not upon length (quantitative) but upon form (qualitative); for the percentage of accuracy does not vary with the difference between the two stimuli. Moreover, we do not find any progressive effect of practice."[13] In 1917 Coover performed a well-controlled experiment based on Sidis's procedure and found that subliminal stimuli exert a positive effect on verbal behavior.[14] Friedline, working on de Laski's general problem, obtained results which she held to show an "extreme delicacy of discrimination" between various cutaneous patterns below the level of the two-point esthesiometer limen.[15]

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[5] Sidis, *op. cit.*, 171.

[6] K. Dunlap, Effect of imperceptible shadows upon the judgment of distance, *Psychol. Rev.*, 7, 1900, 435-453.

[7] Dunlap, Some peculiarities of fluctuating and inaudible sounds, *Psychol. Rev.*, 11, 1904, 308-318.

[8] E. B. Titchener and W. H. Pyle, Effect of imperceptible shadows on the judgment of distance, *Proc. Amer. Phil. Soc.*, 46, 1907, 94-109; also this JOURNAL, 18, 1907, 388.

[9] H. M. Manro and M. F. Washburn, Effect of imperceptible lines on judgment of distance, this JOURNAL, 19, 1908, 242.

[10] M. Stroh, A. M. Shaw and M. F. Washburn, A study in guessing, *ibid.*, 243-245.

[11] C. W. Perky, An experimental study of imagination, *ibid.*, 21, 1910, 422-452.

[12] H. L. Hollingworth, *Advertising and Selling*, 1913, 229-232.

In 1931 Bressler constructed a Müller-Lyer illusion with Hering gray papers, making the arrows of grays less than one jnd. different from the background.[16] He reported: "We found that the Müller-Lyer illusion is effective in evoking a response even when the arrows are so faint that they are not visible to the observer. . . . The magnitude of this response varies directly with the strength of the stimulus."[17] Presuming that these results are the most recent on the problem of the Müller-Lyer illusion, the score at present may be figured to stand at approximately three to one against Titchener and Pyle, with one tie.

Recently two experiments in many ways like the one reported in this paper have been carried out. Baker in 1937 used both auditory and visual modalities.[18] In the auditory he had the *Ss* distinguish between subliminal dots and dashes; in the visual, between subliminal perpendicularly crossed lines in the 'plus' and 'multiplication-sign' positions. He summarized his results as follows: "(1) Evidence in both the visual and auditory modalities, from the results of all subjects combined, and in the case of each individual subject, gives strong grounds for the conclusion that conscious verbal behavior is influenced by stimuli which are below the conscious judgment threshold. (2) It follows that the physiological thresholds are not coincident with, but are lower than, the thresholds of conscious judgment. (3) Evidence in both the visual and auditory modalities, and from all of the subjects used, gives strong grounds for the conclusion that the influence of the subliminal stimuli is a function of the intensity of the subliminal stimulus."[19]

The latest work of this sort is that of Williams reported last year.[20] Interested in the effect of subliminal cues upon Rhine's extra-sensory perception, he projected from the rear three figures (circle, triangle, square) upon a ground-glass screen. He presented these figures 594 times to 11 unexperienced *Ss* in a prearranged order, showing each figure an equal number of times. They were projected at an intensity below the limen as determined by the method of limits. He then chose the 4 *Ss* with the highest scores for a second experiment. To each of these he represented 8 series of 27 figures each at different levels of subliminal intensities, and then 108 control figures at zero illumination. He had the *S*s report the degree of confidence with which they guessed what the figure was: 1=seen clearly; 2=with doubt but with something seen; 3=a pure guess with nothing seen. In tabulating the results, only figures below the limens which gave rise to '3' judgments were considered. All the results were well above chance frequency, except in the control experiments with no illumination, when the results were at chance frequency. *E* reported that "in no case could any relationship between the number of hits and the intensity of the stimulation be discovered."[21]

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[13] E. de Laski, Perceptive forms below the level of the two-point limen, this JOURNAL, 27, 1916, 569-571.

[14] J. E. Coover, *Experiments in Psychical Research at Leland Stanford Junior University*, 1917, 1-641.

[15] C. L. Friedline, Discrimination of two cutaneous patterns below the two-point limen, this JOURNAL, 29, 1918, 400-419.

[16] J. Bressler, Illusion in the case of subliminal visual stimulation, *J. Gen. Psychol.*, 5, 1931, 244-250.

[17] Bressler, *op. cit.*, 250.

[18] L. E. Baker, The influence of subliminal stimuli upon verbal behavior, *J. Exper. Psychol.*, 20, 1937, 84-100.

[19] Baker, *op. cit.*, 98-99.

[20] A. C. Williams, Jr., Perception of subliminal visual stimuli, *J. Psychol.*, 6, 1938, 187-199.

TERMINOLOGY AND PROBLEMS

The present experiment has the purpose of investigating several questions of subliminal perception. In order to state these problems carefully, three terms must be defined.

The phrase *verbal report* will be used in this paper to indicate the sheer use of the vocal cords to produce a certain recognizable sound. The sounds which will be employed are *circle, square, plus, star* and *wave*, but they might just as well be *one, two, three, four* and *five*, or *ay, eee, eye, oh* and *ooo*. Before any sounds are used, it must be arranged that they are associated with the five proper geometric figures, and this is assured with *S*s who speak English if the words *circle*, etc., are used. Except for the fact that five sounds produced by the vocal cords are used rather than five keys, 'verbal report' is in all ways the same sort as manual.

Awareness will be indicated by another use of verbal behavior, however. The meanings which the words symbolize and the syntax of the sentences in which the words occur will be considered. When the *S* says "I see a circle" or "I see a wave" or "I see a flicker of light," these will be considered statements of awareness. If he is unwilling to say "I actually see a circle," then he is unaware of a circle.

Discrimination in this experiment will be used to indicate that the *S* is showing characteristic differentiated behavior for one or more of five figures.

The six problems investigated in this experiment are:

(1) Can *S*s who are entirely unaware that they are taking part in an experiment in visual perception nevertheless perceive?

(2) Under these conditions is there a relation between the strength of a purely subliminal stimulus and the correctness of perception?

(3) Are the limen of awareness and the limen of discrimination equivalent?

(4) Does passing above the limen of awareness have any effect on dis-

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[21] Williams, *op. cit.*, 195.

criminatory action? (*I.e.* is discrimination of which the *S* is unaware, merely because he is unaware of it, different from that of which he is aware, as certain clinicians maintain?)

(5) Does the change of set produced by explaining to *S* that he is perceiving alter his limens?

(6) Are there other behavioral results from explaining to *S* that he is perceiving?

APPARATUS AND PROCEDURE

Apparatus. The arrangement of the experimental rooms is diagrammed in Fig. 1. *S* entered Room 2 through the door at the left. The room was painted black and was illuminated by a 25-watt bulb shaded from the eyes of the *S* as he sat at G, which was his position throughout the experiment. Apparently on the wall in front of *S* was an 8 x 10-in. mirror. This, however, was actually not on but in the wall, and was in fact a transparent mirror, which could transmit light from behind although it appeared like a regular mirror from the front. In Room 1, of which the 'naïve' *S* had no knowledge, was a card-exposure apparatus (A) in a soundproof box with a glass window facing *S*. It was made to hold 200 cards, each bearing one of the

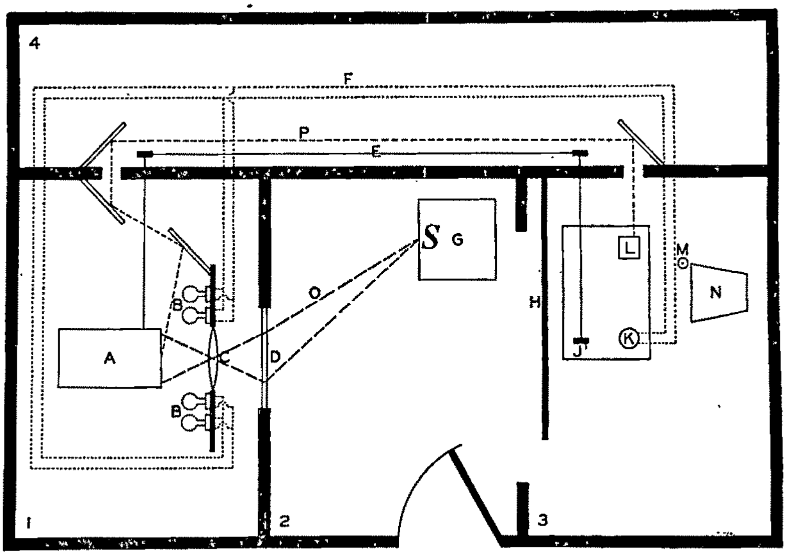

FIG. 1. DIAGRAM OF THE APPARATUS

A, soundproof exposure apparatus; B, electric bulbs; C, lens; D, transparent mirror set in wall; E, pulley system for remote control of exposure apparatus; F, wiring for illumination of cards in exposure apparatus; G, *S*'s chair; H, 4-ft. screen between *E* and *S*; J, control wheel for exposure apparatus; K, variac in illumination circuit; L, end of periscope system; M, signal bell; N, *E*'s chair; O, path of light to *S*; P, path of light through periscope system; 1, 2, 3, 4, separate rooms.

five geometric figures of Rhine's test for extra-sensory perception: circle, plus, square, star, wave. These cards were shuffled in chance order in a shuffling machine before they were put into the machine. *E*, who sat at N, could by the wheel (J) on his control board silently expose these cards one by one. *S* had no knowledge of this set-up, for the control board was hidden from him by the screen (H), over which *E* might, however, see him, and the connections between J and A went through an attic (4) of which *S* had no knowledge.

The light reflected from the card exposed in the machine at any one time was caught in the lens (C) and the image was focussed on a milk glass just behind the transparent mirror (D). If the illumination on the card was high enough, its image, approximately 6 by 6 in., was visible on the mirror. The illumination on the card was supplied by a circle of bulbs (B) around the lens. The illumination could be varied by *E* by means of the variac (K) on his control board. Since, during the experiment, the image on the mirror was not strong enough to be visible, a periscope system was set up which reflected the image of each card through Room 4 to the mirror (L) on *E*'s control board. The path of this light is indicated in Fig. 1 by a short-dashed line (P). The long-dashed lines (O) indicate how the light from the card passed to *S* through the mirror. M represents a bell which *E* rang with his foot to signal *S*.

Subjects. Essentially three different experiments were performed. Experiment A employed 5 naïve *S*s, undergraduates in a beginning course in psychology. They knew nothing of the set-up, and thought, until the experiment was ended, that it had to do with clairvoyance rather than visual perception. Experiment B was identical with Experiment A, except that it was performed on 5 sophisticated *S*s, one professor of Psychology, 2 graduate students in psychology, and 2 seniors in psychology. They had all had some experience as *S*s in visual experiments; one had had 30 hours of such work, and another many times that. In Experiment C, 5 *S*s went through the procedures of both Experiments A and B with certain variations. The Ss were 5 students in a beginning course in psychology who performed the experiment as naïve *S*s, and then repeated it after having had its purposes and the apparatus explained to them.

Experiment A. The following procedure was followed with 5 naïve *S*s, who will be referred to as *A, B, C, D,* and *F.* Several days before the experiment the *S*s were shown a pack of the ESP cards and were told about the experiments in clairvoyance developed by Rhine and his co-workers.[22] They were given several clairvoyance tests, and then requested by *E* to take a more rigorous test in the laboratory.

When the *S*s were taken into Room 2, they were immediately led up to inspect the mirror on the front wall. *E* took ample precaution to see that they had sufficient evidence that it was a real mirror, being certain that they saw their reflections in it and so forth. This, however, was not emphasized so strongly that they had any cause to question that it was a regular mirror. They were then seated in *S*'s chair and given verbal directions by *E* approximately as follows:

"It is a common procedure in clairvoyance experiments for the subject to gaze into a crystal to help him find the answer he wants. This is usually done when the subject tries to picture three-dimensional objects. In this experiment, however, you are going to work with the figures on the flat surfaces of ESP cards. I am asking you, therefore, to stare into the flat surface of this mirror. All during the experiment I wish you to stare into the mirror, and not to move your head or your eyes. You may, however, look at the mirror with any part of your eyes that you wish—either fovea or periphery. When your eyes tire, tell me, and we shall rest.

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[22] C. E. Stuart and J. G. Pratt, *A Handbook for Testing Extra-Sensory Perception*, 1937.

"I shall now go into the room behind you and sit at the table behind the screen. Sitting there I shall expose the cards one by one. When I expose a card I shall ring a signal bell. Please stare at the mirror and try to picture one of the five figures on the mirror as approximately 6 x 6 in. in size, that is about one inch from either side of the mirror and two from the top and bottom. Report the first figure that you picture on the mirror. If you cannot get any cue from the mirror to help you, guess one of the figures, but say that it is a guess.

"Please report also what kind of image you see on the mirror, if you see one; that is, whether it is a memory image, a thought image, an imagination image, an hallucination image, an after-image, or an actual physical image.

"Do not figure that out of every 25 calls you must call 5 of each figure. Though an equal number of each figure will be used, the cards are in a large pack, and it is quite conceivable that chance may turn up a long run of any figure. Please use the following names for the cards: *plus, wave, star, circle, square*."

E was able by watching *S*'s head at all times to tell if it moved away from the mirror, but of course he had to rely on *S*'s report that he was not turning his eyes away from the mirror. The phrasing of the directions naturally led the *S* to think that the cards were being exposed in the room behind his back, especially since *E* carried a pack of the cards in his hand while giving the directions and took them back to the room with him when he began to experiment. Careful inquiry at the end of the experiment indicated that no other possibility had occurred to any of the *Ss*.

The various sorts of possible images suggested were not defined for the *Ss*, except insofar as they had learned definitions of the terms in their course in psychology. The purpose of this question was mainly to discover whether they were at any time able to report an actual perception 'out there,' or anything which might be an actual perception, and also to assess the degree of their certainty that they did not see anything. This is a behavioral phrasing of Perky's problem of objective and subjective images. The explanation that a large pack of cards was being used was given for the purpose of counteracting a frequent feeling of *Ss* that they are calling too many of one figure. They were instructed to use certain names for the figures because in previous experiments *Ss* were found to use *cross* for *plus*, and sometimes after long use of the word it was found that they were looking for an 'x-shaped' figure, or a figure with the lower vertical arm elongated. It was also found that *Ss* tend to use *wavy lines* for *wave*. This emphasizes the wave over the other figures by using two words for it, and the figure is probably overemphasized in the minds of the *Ss* because it is not a common geometrical figure. Each *S* swore himself to secrecy for the duration of the experiments in order that the naïve *Ss* might remain naïve.

The cards were exposed; the bell was rung immediately after each exposure. At first a long run of the cards was made without any illumination at all to be sure that *S* was not gifted with ESP. There were no significant variations from chance in this procedure.

S was then given 5 series of 25 guesses each, at each of the illuminations represented by 80, 90, 100, and 110 volts on the variac. These voltages were employed in random order to eliminate possible fatigue effects from the data. The variac readings were converted into illumination readings by means of a photometer 3 yd. from the mirror, and the final results of the experiment are given in terms of illuminations in foot-candles. These voltages were chosen because preliminary experiments with naïve *Ss* showed that they included the significant range.

After the 500 cards had been exposed, *S* was carefully questioned as to whether he had seen any actual physical images in the mirror. His words were written down and he was asked to sign them. Then *S* was allowed to discover for himself the nature of the mirror by raising the illumination to whatever brightness was necessary for him to realize that he was actually seeing a physical image on the mirror-screen. The illumination at which he first realized this fact was recorded. This illumination was taken as his limen of awareness as a naïve *S*. The whole apparatus and purpose of the experiment was then explained to *S*, and his comments were recorded.

Finally *S*'s limen for each of the five figures was determined by the method of limits. For each of the figures the illumination was increased from zero at a constant slow rate until he reported that he saw it. The illumination was then raised to maximum and decreased until he reported no longer seeing it. This procedure was repeated 5 times for each figure with a rest period before each trial. The average of the 5 ascending and the 5 descending trials was taken as his limen for that figure, and the average for all the figures was taken as his limen of awareness in a sophisticated state.

Experiment B. This procedure was repeated for the five 'sophisticated' *Ss* who understood the purpose of the experiment and the apparatus. They will be called *a, b, c, d,* and *f.* They were given 500 trials each, 125 at each of four voltages, but these were taken as 60, 70, 80, and 90 volts instead of the higher voltages, because preliminary experiments with sophisticated *Ss* indicated that these lower voltages included the significant range for them. After each trial they reported whether they actually saw anything on the mirror or not. For them the limen was determined in the same way.

Altogether, in Experiments A and B, 5000 cards were exposed and the comments on each were recorded. *Ss* were at no time told how well they were doing.

Experiment C. Both Experiments A and B were carried through with five originally naïve *Ss* (*1, 2, 3, 4,* and *5*) in this procedure. Each, however, was given only one set of 25 cards, but at each voltage from 60 to 110 in steps of 10 volts. The limens and the point at which the figure was first seen as a real physical image were noted, as above, and then the apparatus and purpose were explained. The procedure was then repeated for them as sophisticated *Ss*. Fifteen hundred more cards were exposed in this experiment.

RESULTS

The data for Experiments A and B are summarized in Table I. In any case where there was a report that an actual physical image was seen (there was no such report in Experiment A), that case was eliminated from the data, and the remaining cases of the run prorated as if for 25.

Since no datum is included in Table I which was given at a time when the *S* was not perfectly certain he did not see an actual physical image on

TABLE I

Results of Experiments A and B

	S	Number correct out of 125 calls at						Limen of awareness	
		60 v.	70 v.	80 v.	90 v.	100 v.	110 v.	soph. *S*	naïve *S*
Experiment A	*A*	*	*	15	24	27	34	93.2	128
	B	*	*	26	43	69	114	100.6	125
	C	*	*	30	26	33	45	90.8	130
	D	*	*	22	21	26	27	91.3	138
	F	*	*	29	36	49	62	107.3	138
	Av.	*	*	24.4	30.0	40.8	56.4	96.6	132
Experiment	*a*	20	28	32	29	*	*	125.2	*
	b	22	21	28	32	*	*	105.8	*
	c	28	33	43	52	*	*	97.7	*
	d	31	33	26	39	*	*	92.3	*
	f	30	31	40	70	*	*	108.4	*
	Av.	26.2	29.2	33.8	44.4	*	*	105.9	*

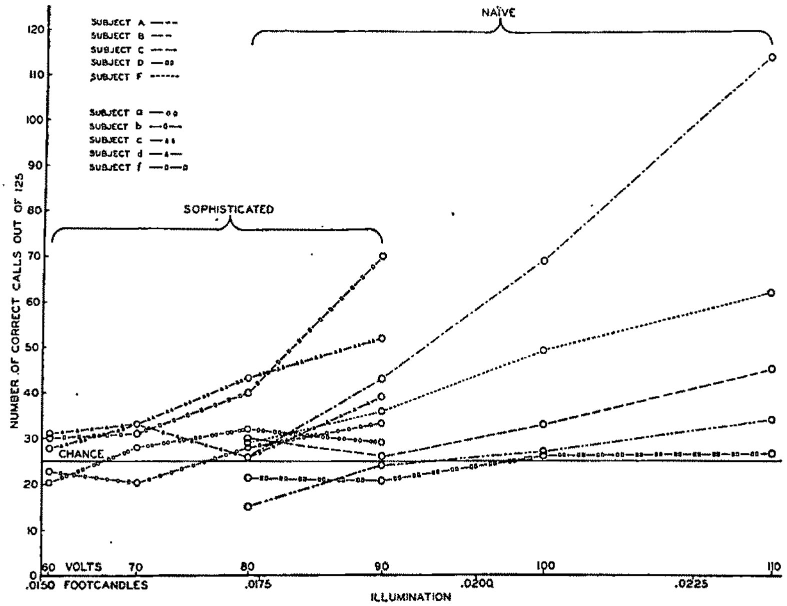

Fig. 2. Individual Scores of *S*s in Experiments A and B.

the mirror, all cases in the table are definitely below the limen of awareness. The illumination intensities at which these curves are becoming asymptotic to the chance line of 25 out of 125 make it obvious why the illuminations used were chosen. The average curves for both groups of *S*s are definite in their trends, with no inversions. The individual scores show 5 inversions totalling 16 points, or an average of 3.2 points. The individual scores for these 10 *S*s are graphed in Fig. 2.

The data of Experiment C are summarized in Table II.

TABLE II

RESULTS OF EXPERIMENT C

	S	Number correct out of 25 calls at					
		60 v.	70 v.	80 v.	90 v.	100 v.	110 v.
Naïve	1	3	3	8	4	6	15
	2	6	10	5	3	7	13
	3	6	7	5	6	3	6
	4	5	3	4	5	9	10
	5	6	5	6	7	5	8
	Total	26	28	28	25	30	52
Sophisticated	1	8	11	6	12	21	21
	2	3	4	6	6	13	7
	3	4	5	12	6	11	15
	4	6	3	6	18	11	20
	5	4	10	6	6	7	12
	Total	25	33	36	48	63	75

In order that truly naïve *S*s might be used, who knew nothing of the experiment or of the type of psychological procedure, and who would be willing to accept the explanations concerning clairvoyance without skepticism, the 5 unpracticed *S*s were chosen. To give the problem as careful a test as possible, however, 5 *S*s with more experience were also used. It seemed improbable that the same *S*s could serve both functions, even with a good deal of practice and training, so separate sets of *S*s were used for Experiments A and B. This procedure might well give rise to the criticisms, however, that an artifact had been introduced, that the limens of two groups would not be comparable, and that their difference would not be significant. In order to make certain of these points, Experiment C was performed as a check, using the same 5 *S*s in both naïve and sophisticated capacities. The results are graphed in Fig. 3 on the same scale with the average results from Experiments A and B, in order to show that Experiment C is significantly comparable with A and B. This result also shows that no artifact was inserted into the results by the choice of different voltages for the two types of *S*s. In Experiment C the *S*s in both naïve and sophisticated capacities were given all voltages from 60 to 110.

It is conceivable that the use of five figures with different limens may have introduced a source of statistical error into the experiment. If it is true that the circle is the best figure,[23] then it is probable that it can be seen at a lower intensity than, say, the wave. There is later evidence contrary to this position of the Gestalt school,[24] and it is interesting to note from Table III in passing that our data corroborate the first position.

TABLE III

Number of Times the Different Figures Were Correctly Reported at the Different Voltages

Figure	Sophisticated Ss				Naïve Ss				Total
	60 v.	70 v.	80 v.	90 v.	80 v.	90 v.	100 v.	110 v.	
Circle	29	26	44	58	32	40	55	72	356
Plus	28	38	32	58	26	35	49	62	328
Square	31	34	36	43	24	31	40	42	281
Star	29	29	39	41	21	30	37	67	293
Wave	14	19	18	22	19	14	23	39	168
Total	131	146	169	222	122	150	204	282	

Although all the figures were presented an equal number of times, it might have been possible that a preponderant number of the figures with higher limens were presented at the lower voltages and those with lower limens at the higher voltages, and that this was the real cause for the rise of correct calls with voltage. One answer to this criticism is that an actual count of the number of each figure presented to each *S* at each voltage shows no significant variation from chance. Another answer is Table III, which

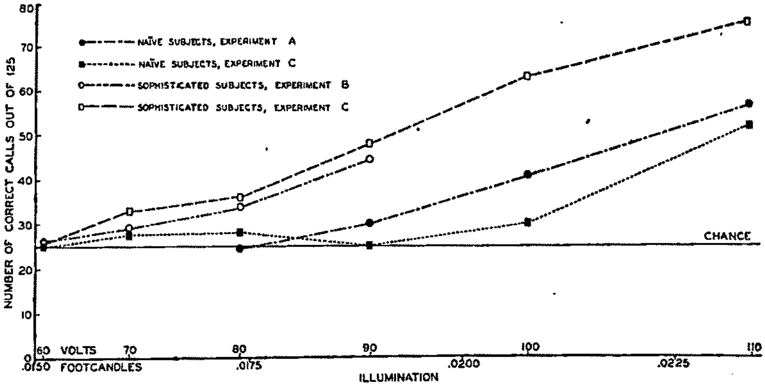

Fig. 3. Average Scores of Naïve *Ss* in Experiment A and of Sophisticated *Ss* in Experiment B, and Total Scores of *Ss* in Experiment C.

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[23] W. Köhler, The problem of form in perception, *Brit. J. Psychol.*, 14, 1924, 268.

[24] H. Helson and E. V. Fehrer, The rôle of form in perception, this Journal, 44, 1932, 79-102.

shows the data for Experiments A and B fractionated for each figure. It indicates that with only 4 inversions, averaging 3.75 points, the number of correct calls for each figure rose with the voltage, and that in all cases curves for sophisticated *S*s were well above those for naïve.

The results yielded two other indications that discrimination can go on without awareness. In Table IV are listed the average limens for each of the five figures for the sophisticated *S*s, and beside them the number of times the figures were called correctly out of all the times presented. The table shows that the higher the limen, the fewer the correct calls—a clear case of visual discrimination, though all *S*s definitely reported in all cases that nothing was visible in the mirror.

TABLE IV

AVERAGE LIMENS FOR EACH OF THE FIGURES FOR THE SOPHISTICATED *S*s TOGETHER WITH NUMBER EACH CORRECTLY REPORTED

Figure	Limen (volts)	Number correct
Circle	94.4	356
Plus	98.7	328
Square*	105.9	281
Star	106.7	293
Wave	118.8	168

* The limen for squares was higher than that for stars in the cases of 40% of the *S*s. The sudden jump to 67 stars at 110 volts in Table III is the cause for the inversion in the last column of Table IV.

We find in Table V another corroboration of the acceptance of unreported visual cues by counting the number of times images were reported projected (by the naïve) or actually seen (by the sophisticated) for each voltage. As the voltage and illumination rose, the numbers reported rose.

TABLE V

NUMBER OF TIMES IMAGES WERE PROJECTED OR ACTUALLY SEEN FOR EVERY VOLTAGE

Naïve *S*s*		Sophisticated *S*s	
Volts	Number projected	Volts	Number seen
80	138	60	23
90	147	70	22
100	158	80	31
110	200	90	42

* Among the naïve *S*s, *D* projected or pictured an image every time, *F*, never; both are omitted because they cancel themselves out.

When deviations from chance are used to indicate the presence of a phenomenon, like discrimination in this case, it is convenient to know what are the probabilities that the results could have come by chance. The chi-square and Pearson's P are conventionally used to indicate this significance. In order to make these computations as certain as possible, they were made only for trials that fulfilled two criteria: (1) they must be below the lowest limen found by the method of limits for any *S*; and (2) they must include both naïve and sophisticated *S*s. The runs at 80 and 90 volts fulfilled these criteria. For 80 volts the chi-square value was 18.95. Since these data have 16 degrees of freedom, this gives a P-value of about 0.3, meaning that 7 cases out of every random 10 would give results nearer the chance line than these. For 90 volts the chi-square value was 107.68, which gives a P-value of less than 0.001, meaning that more than 999 cases out of every random 1000 would be nearer chance than these data.

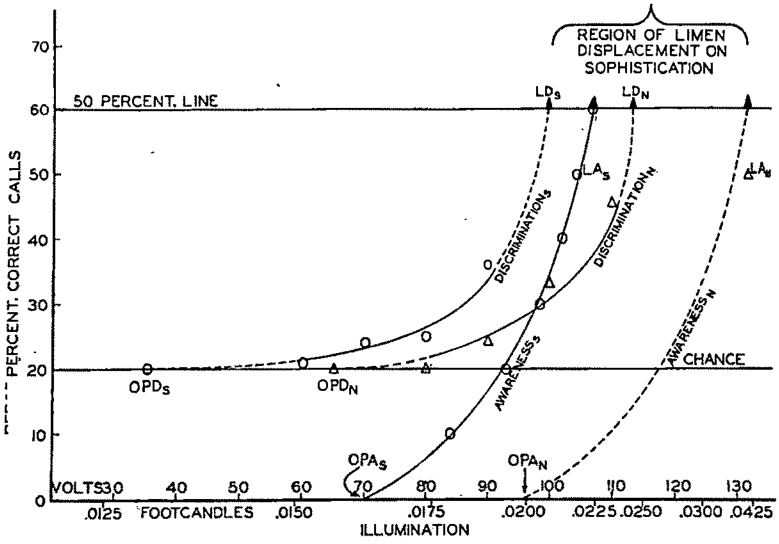

Fig. 4. Comparative Functions of Discrimination and of Awareness for Sophisticated and Naïve *S*s.

The solid lines are derived from the data of this experiment; the dotted lines are extrapolations. OPD$_S$, onset point of discrimination for sophisticated *S*s; OPD$_N$, onset point of discrimination for naïve *S*s; OPA$_S$, onset point of awareness for sophisticated *S*s; OPA$_N$, onset point of awareness for naïve *S*s; LD$_S$, limen of discrimination for sophisticated *S*s; LD$_N$, limen of discrimination for naïve *S*s; LA$_S$, limen of awareness for sophisticated *S*s; LA$_N$, limen of awareness for naïve *S*s. The "50-percent. line" represents 50% of the distance between the chance-line and 100%.

One other part of the protocols is of interest. Every one of the naïve *S*s spoke of his surprise on seeing figures he had been unable to perceive, when the experiment was explained at the end of the procedure. *A* said: "I was positively amazed when the set-up was explained to me." *B*: "I was very surprised when I saw it." *C:* "I was very, very surprised in finding there really was a physical image there." *D:* "I was very much surprised at the set-up." *F:* "I am surprised it [the apparatus] was there." Observers watching the experiment were continually astonished that the naïve *S*s did not see the images at the higher voltages. An experienced sophisticated *S* (*b*), after discussing the experiment with other psychologists, declared: "I could not get that group to feel as surprised as I about the effect of cues that refuse to be available to introspection. Apparently my attitude is due to the fact that I was a subject."

DISCUSSION

Let us consider the significance of the results in terms of Fig. 4. Here average curves for the sophisticated and for the naïve *S*s are plotted, showing the results obtained for the functions of discrimination and of awareness. The two discrimination curves are extrapolated upward and downward beyond the points determined by the data of this experiment, as indicated by the dotted lines. These functions are theoretically asymptotic to the 'chance line' of 20% correct calls, because there can be no positive evidence for discrimination except above this line. Actually the functions are tangent to the line at a certain point which can be approximated by extrapolation. This may be called 'the point of onset' of the function. Extrapolation of the curve of discrimination for naïve *S*s downward on the basis of the data of Experiment C makes it apparent that the point of onset of discrimination for naïve *S*s (OPD_N) is somewhere between 60 and 70 volts. The data do not indicate the point of onset of discrimination for sophisticated *S*s (OPD_S), but downward extrapolation indicates that the region would be about 30 or 40 volts.

The locations of liminal points on these discrimination functions are based on the following assumptions. These curves are essentially the lower halves of two ogives such as represent the results of the method of constant stimuli. They are psychometric functions rising with the illumination. The limen is conventionally taken to be the point where the curve intersects the 50%-line. Since, however, chance would in our experiment give 20% correct calls, we must take the half-way point of the distance between 20% and 100%. This is the 60%-line. The data cannot be extrapolated further to show the upper halves of these two ogives.

The data from the determination of the limens of the sophisticated *S*s by means of the method of limits are the basis of the curve of awareness for sophisticated *S*s. In constructing this curve, use was made of Urban's proof[25] that the results of the method of limits are consistent with the results of the method of constant stimuli, and that they can be plotted similarly; that is to say, the average of the least-perceptible value with the greatest-not-perceptible should be the value as often as not perceptible (50%). In this curve the percentage of the total number of trials in the liminal determinations in which the figures were just seen or just not seen at each illumination was added to the sum of the percentages for all the lower illuminations. The ogive thus obtained indicated that the point of onset of awareness for sophisticated *S*s (OPA_S) was slightly above 70 volts. Only the lower part of this ogive is plotted. The limen of awareness for sophisticated *S*s (LA_S), then, is at 50% correct calls, halfway between 0% and 100%.

Actually in the calling of the cards, all the cases where *S* reported being aware of an actual physical image were omitted from the results tabulated and the curves plotted. Each datum used in determining the locus of the functions of discrimination in Fig. 4, therefore, was in a sense subjected to a liminal judgment by *S*, who reported that he was unaware of it. As the experiment was conducted, then, no part of the function of awareness for either group of *S*s lay at a lower voltage than any part of the function of discrimination for that group. The functions of awareness in Fig. 4 are plotted as they are for the averages of groups of sophisticated and naïve *S*s, and not as they were in the special conditions of the experiment, where the cases were selected in such a manner as to give the problem under consideration the most rigid possible test.

The limen of awareness for naïve *S*s (LA_N) was found by raising the illumination at the end of the experiment until the naïve *S*s first saw an actual physical image. The average for all *S*s was found to be 132 volts. This limen was put at the same level as that for sophisticated *S*s because it is an analogous limen. Admittedly it cannot be carefully determined by the technique used, for it can be tested only once for every naïve *S*, because as soon as it is reached, *S* is *ipso facto* no longer naïve. Admittedly it is subject to error as it gives only the value for the least-perceptible intensity, and does not average it with the value for the greatest-not-perceptible. Moreover, the *S* may be constrained from speaking because he cannot believe an image would be there. (This point may seem to beg the question from a strictly behavioral point of view, because the limen is taken as the point at which *S* speaks, no matter from what causes. But what if as soon as he has spoken he says, as did one *S*, "I was aware that a figure was there before I spoke, but I could not believe it until it got brighter"?) Undoubtedly these factors tend to raise the readings for the limen higher than they should be, but the figures for the determinations of the LA_S indicate that the total effect of all these factors is less than 10 volts.

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[25] F. M. Urban, On the method of just perceptible differences, *Psychol. Rev.*, 14, 1904, 244-253.

The curve of awareness for naïve *S*s is plotted parallel to that for sophisticated *S*s because it is a similar function, and because the experiment gave no data concerning it, except the point of the limen. To allow for the errors considered in the preceding paragraph, the curve at the 50% locus was made to pass through an illumination slightly lower than that determined by this experiment. The point of onset of awareness for naïve *S*s (OPA_N) is found to be about 95 volts.

Fig. 4 illustrates, therefore, how for each group of *S*s the psychometric function for discrimination lies at lower intensities than the psychometric function for awareness.

The figure also shows that the limens and the psychometric functions of both awareness and discrimination are displaced to lower illuminations by sophistication. This shift was not due merely to a sharpening of attention when the naïve *S*s first became aware that actual physical images were appearing on the mirror. The *S*s knew for what shapes they were looking and what size they would be, yet could not 'see' them. Once they had perceived them, the figures could be reported with less attention.

Why this displacement occurred suddenly with the first statement of awareness of an actual physical image is to the writer the most important question of the whole experiment, because its answer indicates something about the nature of unaware behavior and also hints at an operation which is dependent on the passage of acts from unawareness to awareness. The change of limen in the process of sophistication is sudden and complete. It was undoubtedly a change of set, attitude or motivation, because as soon as the first actual physical image had been reported seen, the limen of awareness went down from 132 to 100.5 (for Experiments A and C), and the whole curve was displaced—*S* was sophisticated. *S* could now see what he could not see before, and he was surprised he had been unable to see it before.

This exclamation of surprise may well be a behavioral event caused by the organism's becoming aware of a stimulus which has been acting upon it. Surprise may be an indication that some part of the environment which has been supposed to be of a certain character turns out not to be. Such surprise has been reported before in the literature. Perky, for instance, noted that, when at the end of the experiment the *S* was asked whether he was

" 'quite sure that he had imagined all these things,' the question almost always aroused surprise, and at times indignation."[26] Again she said that, when the images she used were shown to a number of students in the department of psychology and to some psychologically competent visitors, and they were told that the *S*s had not known they were objective physical images, "every one of these observers showed great surprise; most of them 'would, of course, take our word' for the facts, but could hardly credit them; a few remained entirely incredulous—'we asked them to believe too much'."[27] Newhall and Sears, speaking of conditioning finger retraction to visual stimuli near the absolute threshold, say that "a sophisticated subject was surprised to learn that he had made three conditioned responses when he thought his finger had not moved during any of the test trials of the session."[28]

SUMMARY

This experiment has shown: *(1)* That *S* can 'discriminate' intensities too low for him to be 'aware' of them.

(2) That there are at least three perceptual conditions: (1) when *S* cannot discriminate the stimulus; (2) when he can discriminate it but is not aware of it; (3) when he can both discriminate and be aware of it. For this reason we cannot equate awareness and discrimination.

(3) That the correctness of the perception of a subliminal stimulus varies with the strength of the stimulus. This rule holds for both naïve and sophisticated *S*s.

(4) That the range of illumination in which the psychometric function for awareness lies can be affected by attitudinal factors or motivation (in this case, a knowledge of the experimental set-up), but that the psychometric function for discrimination changes correspondingly.

(5) That behavior of surprise brought about by explanations that the *S* is receiving external stimulation is caused by the consequent awareness of discrimination, and may be regarded as an operation indicating awareness of discrimination.

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[26] C. W. Perky, An experimental study of imagination, this JOURNAL, 21, 1910, 431.

[27] Perky, *op. cit.*, 433.

[28] S. M. Newhall and R. R. Sears, Conditioning finger retraction to visual stimuli near the absolute threshold, *Comp. Psychol. Monog.*, 9, 1933, 11.

HIGHER ORDER CONDITIONING

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Pavlov states that conditioned salivary responses based upon food cannot be established beyond the second order (experiments of Frolov), whereas conditioned defense responses based upon shock cannot be established beyond the third order (experiments of Foursikov).[1] Finch and Culler report that conditioned motor responses can be established up to five orders, and presumably to any desired order, by the use of a general energizing stimulus (thoracic shock).[2] These authors attribute Foursikov's inability to obtain conditioning beyond the third order, to failure of maintaining adequate "motivation" in the animal rather than to any inherent difficulty in the problem itself. The thoracic shock which they used (neither an unconditioned stimulus nor a conditioned stimulus for the motor response) provided the incentive or motivation needed to keep the animal responding to the higher order stimuli. They liken the function of the thoracic shock in the dog to the ordinary social incentives in everyday human learning.

Pavlov further states that the second order of the salivary conditioned response is definitely weaker than the first; in defense reactions, the third is weaker than the second, which in turn is weaker than the first. Brogden and Culler,[3] after conditioning animals to four orders by using the same technique as Finch and Culler, measured the relative strength of the orders thus established by the rate of experimental extinction. The first order was found to be about four times as stable as the higher orders. The higher orders (second, third, and fourth) were equal in the resistance to experimental extinction.

The transfer of a response from one stimulus to another indefinitely seems to be a salient characteristic of human voluntary reaction. In view of this and the dissimilarity which was encountered in Pavlov's laboratory between higher order salivary responses and higher order defense responses, it seemed desirable to repeat the work of Finch and Culler and of

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[1] I. P. Pavlov, *Conditioned Reflexes*, 1927, 34.

[2] Glen Finch and Elmer Culler, Higher order conditioning with constant motivation, this JOURNAL, 46, 1934, 596-602.

[3] W. J. Brogden and Elmer Culler, Experimental extinction of higher-order responses, *ibid.*, 47, 1935, 663-669.

Brogden and Culler, using *food* instead of thoracic shock as a constant incentive for the higher orders.

EXPERIMENTAL PROCEDURE

Four stimuli were used: (1) an electric doorbell, B; (2) a column of compressed air directed to the thigh of the left hind limb, A; (3) a 100-watt light, L; (4) a 1000 ~ tone, T. It was planned, in order to offset any inequality of the stimuli, to have every stimulus represent each order in one of four dogs as shown in the following schema:

	Order			
Animal	first	second	third	fourth
No. 1	B	A	L	T
No. 2	A	L	T	B
No. 3	L	T	B	A
No. 4	T	B	A	L

Mongrel dogs, taken from an unselected laboratory stock, were placed in a soundproof, light-shielded test-room where pellets of food were delivered automatically in the food-box at the animal's right. As soon as food was eaten readily, training was started. The first order stimulus (*e.g.* bell) was presented for 2 sec., followed immediately by shock to the left forepaw. Twenty-five such stimulus-combinations were given daily until a score of 92% (23/25) shock-avoiding flexions to the stimulus was reached. At the first of the test-period on the next day, the second order stimulus was given alone to be certain of its indifference. Food was likewise presented to insure its neutrality for flexion. Once these tests were made, the first order and second order stimuli (*e.g.* bell and air) were given in combination (neither shock nor food followed such presentation; shock was given *only* in combination with the first order stimulus). When flexion to this combination reached the 92% level, the second order stimulus (*e.g.* air) was then presented without the first order stimulus (*e.g.* bell), a biscuit of food now being delivered after each flexion.[4] No food was given if flexion failed. Once the second order was established, the third order stimulus and food were tested separately for indifference to flexion before the second and third order stimuli (*e.g.* air and light) were combined. Food was given after each correct response, both to second and third order stimuli alone and in combination. When flexion to the third order stimulus reached the criterion of 92% response, the fourth order stimulus (*e.g.* 1000 ~ tone) and food were tested separately for indifference before training to the fourth order was started. The fourth order was established in the same way as was the third, food following each flexion-response to the stimuli.

If response at any level broke down, training was given at the next test-period on the level just below. For example, suppose an animal were being conditioned to the second order, and response to the second order stimulus began to fail. On the next day, the first and second order stimuli would be presented together. When the 92% criterion to the combination was reached, training would then begin again on the second order. However, if with failure of response to the second order stimulus, there followed failure to the first and second order stimuli in combination, retraining to the first order would be instituted. Once the first order was built up to full strength, training would again proceed as outlined above.

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[4] All tests were carried out when the animals had fasted for 18-20 hr., in order that the condition of hunger might be kept as constant as possible.

In brief, shock was applied only with the first order stimulus. Food was the incentive for the higher orders, being given after each flexion to any higher order stimulus, alone or in combination with another stimulus of the higher order; when the first and second order stimuli were combined *neither* food nor shock was given. The higher order responses were elaborated by combining the stimulus of the next higher order with the stimulus of the order just established, until response occurred to the new stimulus. Both the stimuli and food were tested for indifference for flexion before starting the training to the next higher order. At no time did any of the animals flex the left forelimb when food was given alone. Nor did they respond to any of the stimuli before they were combined with the stimulus of the order just established.

With four orders of conditioning established, each to a response-level of 92%, the relative strength of each order was measured by the rate of experimental extinction. The first order conditioned stimulus was presented without the pedal shock until the criterion of extinction (8% or 2/25) was achieved.[5] The second, third, and fourth orders were successively extinguished to the same level, food being omitted from the test-sequence. After extinction of the four orders, the animals were reconditioned by the original procedure. When retraining was completed, the four orders were once more extinguished; but now the second order was extinguished first, then the third, then the fourth, and lastly the first order. The following scheme shows the training and extinction schedule:

Order of training	Order of extinction			
Original training	1	2	3	4
First retraining	2	3	4	1
Second retraining	3	4	1	2
Third retraining	4	1	2	3

It is clear that each order was extinguished first, second, third, and fourth in each dog.

RESULTS

In the attempt to establish higher order conditioning in four animals (see scheme on page 580), 10 dogs were trained. Of these, only 4 could be conditioned to higher orders. No higher order conditioning was obtained with the stimuli air, light, tone, bell (scheme for Animal No. 2) in 3 animals. Two animals were conditioned to the stimuli light, tone,

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[5] The limits 92% and 8% were used in order to reduce the time involved. If one insists on a perfect score at both ends (100% and 0%) with all animals, the time may be increased by weeks; a given dog may undulate around 23/25 or 24/25 for days without giving a single 25/25. Kelley (*Quart. Amer. Statist. Ass.*, 17, 1921, 743) has well shown the value of this range (7% to 93%) for statistical purposes.

TABLE I

Results from Animals Developing Higher Order Conditioned Responses

Column 1, test-period; Column 2, stimulus-combination; Column 3, ratio of correct responses to total trials for test-period, *e.g.* 24/25 means the animal responded 24 times in 25 trials.

Animal	1	2	3	1	2	3	1	2	3
	1	BA	9/14	24	BA	8/11	42	B	15/15
	2	B	23/25		A	0/3		A	3/10
	3	BA	24/25	25	B	25/25	43	BA	17/17
	4	A	0/2	26	BA	17/18		A	2/8
	5	BA	12/15		A	1/6	44	BA	18/18
	6	B	22/25	27	BA	17/17		A	2/7
	7	B	25/25		A	4/8	45	BA	6/10
	8	BA	25/25	28	BA	16/16		A	0/2
	9	A	0/2		A	2/9	46	B	24/25
	10	BA	25/25	29	BA	17/17	47	BA	18/18
	11	BA	21/21		A	1/8		A	3/7
		A	0/4	30	BA	17/17	48	BA	3/3
	12	BA	16/20		A	1/8		A	9/19
		A	0/5	31	BA	4/10	49	A	6/25
	13	B	22/25		A	0/1	50	A	7/25
No. 1	14	B	25/25	32	B	23/25	51	A	20/25
	15	BA	9/11	33	BA	20/20	52	A	22/25
	16	B	25/25		A	4/5	53	A	24/25
	17	B	5/5	34	BA	2/2	54	A	2/2
		BA	20/20		A	24/25		AL	14/25
	18	BA	19/19	35	A	0/5	55	AL	21/25
		A	0/6	36	BA	16/16	56	AL	24/25
	19	BA	19/19		A	3/9	57	AL	4/4
		A	0/6	37	BA	14/17		L	16/25
	20	BA	13/16		A	2/8	58	L	2/25
		A	0/5	38	BA	6/6	59	AL	13/25
	21	B	24/25		A	11/19	60	AL	23/25
	22	BA	15/15	39	BA	11/11	61	L	23/25
		A	5/10		A	7/14	62	LT	22/25
	23	BA	15/15	40	A	23/25	63	LT	24/25
		A	3/10	41	A	9/23	64	T	25/25
					AL	1/2			
	1	TB	5/11	14	BA	2/2	23	AL	24/24
	2	T	22/25		A	19/25		L	0/1
	3	T	23/25	15	A	23/25	24	AL	25/25
	4	TB	24/25	16	AL	23/25	25	AL	21/21
	5	TB	14/15	17	AL	16/20		L	0/4
		B	10/10		L	0/5	26	AL	21/23
No. 7	6	B	18/25	18	AL	12/16		L	0/2
	7	B	20/25		L	0/7	27	AL	24/24
	8	B	13/25	19	AL	18/20		L	0/1
	9	B	10/25		L	0/5	28	AL	18/22
	10	B	8/25	20	AL	19/25		L	1/3
	11	B	14/25	21	AL	22/23	29	AL	2/2
	12	B	25/25		L	0/2		L	19/23
	13	BA	23/25	22	AL	23/25	30	L	23/25
	1	LT	9/15	13	LT	4/10	27	TB	4/5
	2	L	18/25		T	1/4		B	11/20
	3	L	23/25	14	L	23/25	28	B	8/25
	4	LT	18/24	15	LT	0/5	29	B	19/25
		T	0/1	16	L	23/25	30	B	13/25
	5	L	19/25	17	LT	17/20	31	B	21/25

TABLE I—*Continued*

Animal	1	2	3	1	2	3	1	2	3
	6	L	24/25		T	3/5	32	B	24/25
	7	LT	23/23	18	LT	5/6	33	BA	13/25
		T	3/3		T	14/19	34	BA	8/25
No. 9	8	LT	11/11	19	T	20/25	35	BA	14/25
		T	10/14	20	T	9/25	36	BA	14/25
	9	LT	1/1	21	T	12/25	37	BA	12/18
		T	23/25	22	T	21/25		A	5/7
	10	TB	0/3	23	T	21/25	38	BA	2/2
	11	LT	7/10	24	T	16/25		A	16/25
		T	1/2	25	T	23/25	39	A	16/25
	12	L	23/25	26	TB	21/23	40	A	23/25
					B	2/2			
	1	LT	0/5	21	LT	6/10	42	BA	17/25
	2	L	18/25		T	8/15	43	BA	13/25
	3	L	21/25	22	LT	2/2	44	BA	12/25
	4	L	24/25		T	21/25	45	BA	8/25
	5	LT	25/25	23	LT	5/5	46	BA	9/25
		T	0/1		T	16/20	47	BA	9/25
	6	LT	2/10	24	LT	2/5	48	BA	5/25
	7	L	19/25		T	1/5	49	BA	12/25
	8	L	24/25	25	L	24/25	50	BA	16/25
	9	LT	13/20	26	LT	2/3	51	BA	14/25
		T	4/5		T	10/22	52	BA	16/25
	10	L	21/25	27	T	8/25	53	BA	20/24
	11	L	24/25	28	T	14/25		A	0/1
	12	LT	11/13	29	T	17/25	54	BA	17/24
No. 10		T	6/12	30	T	18/25		A	0/1
	13	LT	14/15	31	T	9/25	55	BA	22/25
		T	8/10	32	T	14/25	56	BA	19/24
	14	LT	2/10	33	T	24/25		A	0/1
	15	L	23/25	34	TB	23/25	57	BA	19/22
	16	LT	9/10	35	TB	2/3		A	0/3
		T	11/15		B	22/25	58	BA	15/15
	17	LT	4/8	36	B	18/25		A	9/10
		T	11/17	37	B	23/25	59	BA	1/2
	18	LT	2/10	38	BA	17/25		A	15/23
		T	5/10	39	BA	3/25	60	A	17/25
	19	L	24/25	40	BA	19/25	61	A	18/25
	20	LT	8/10	41	BA	16/25	62	A	23/25
		T	9/15						

bell, air; the second animal was trained when the first died before the extinction series was completed.

In higher order conditioning (where a constant incentive is used as motivation for the higher orders), the crux lies between the first and second order. Once the response is established to the second order stimulus, training to the successive higher orders is accomplished with relative ease. To pass from the first to the second order, response must first be evoked by the first and second order stimuli combined. This is the first difficulty, for neither shock nor food can follow the presentation of these stimuli; if shock were to follow, the second order stimulus would then become another first order stimulus; if food were given, the first order stimulus would lose its discrete position with regard to shock. Two animals failed here. One responded only 3/45 times to the first and second order stimuli in combination during the 14 days during which this training was attempted. The other responded 12/124 times during a training period of 33 days. Neither animal came near reaching 92% response to the first and second order stimuli, the criterion which had to be attained before the second order stimulus could be given alone. Once the

TABLE II

Days Needed for Extinction

The letter in parenthesis after each score gives the position in which that order was extinguished. For example; under dog no. 1, row 1, column 2, we read, 4(b). This column is headed "air" which in the original conditioning was the second order stimulus, bell being the first. In the first of the series of extinctions, air was extinguished second (b) after the bell and prior to the light and tone; and 4 days (25 trials each) were needed for extinction.

	Order 1	Order 2	Order 3	Order 4
Dog no. 1	bell	air	light	tone
Extinction 1	10(a)	4(b)	1(c)	1(d)
Extinction 2	12(d)	4(a)	2(b)	1(c)
Extinction 3	2(c)	3(d)	3(a)	1(b)
Extinction 4	2(b)	4(c)	1(d)	3(a)
Total	26	15	7	6
Mean	6.50	3.75	1.75	1.50
S.E.$_M$	2.78	0.25	0.48	0.50
Dog no. 7	tone	bell	air	light
Extinction 1	2(a)	3(b)	2(c)	1(d)
Extinction 2	2(d)	3(a)	1(b)	1(c)
Extinction 3	4(c)	2(d)	2(a)	2(b)
Extinction 4	4(b)	3(c)	1(d)	2(a)
Total	12	11	6	6
Mean	3.00	2.75	1.50	1.50
S.E.$_M$	0.58	0.25	0.28	0.28
Dog no. 9	light	tone	bell	air
Extinction 1	13(a)	6(b)	1(c)	1(d)
Extinction 2	9(d)	8(a)	1(b)	1(c)
Total	22	14	2	2
Mean	11	7	1	1
S.E.$_M$	2.00	1.00	0.00	0.00
Dog no. 10	light	tone	bell	air
Extinction 1	16(a)	6(b)	5(c)	2(d)
Extinction 2	5(d)	2(a)	2(b)	1(c)
Extinction 3	6(c)	4(d)	5(a)	1(b)
Extinction 4	7(b)	2(c)	1(d)	2(a)
Total	34	14	13	6
Mean	8.50	3.50	3.25	1.50
S.E.$_M$	2.53	0.96	1.03	0.28

criterion of 92% response to the first and second order stimuli presented together is reached, the second difficulty appears. For the response must now be evoked by the second order stimulus alone and kept alive by the food which is given at the end of each flexion. The difficulty at this stage is twofold. First, there may be failure of response to the second order stimulus. Secondly, the flexion response to the second order stimulus may be replaced by a short-circuiting response to food; at the onset of the stimulus, the animal instead of flexing the forelimb, turns immediately to the food-box in anticipation of food. Three animals failed here. One animal responded to the second order stimulus presented alone only 9/34 times during a training period of 24 days. A second animal responded 9/33 times during a training period of 51 days. The third responded 8/19 times during a training period of 15 days.

Once the second order is built up to the 92% response level, little difficulty is encountered in going on to the third and then the fourth orders. However, one animal (the only one of the three, committed to training with air, light, tone, bell, which showed any evidence of higher order conditioning) attained the criterion of response for the second order after only 6 test-periods, but in the attempt to go on to the third order, the second order response broke down. In 32 days of retraining, the second order response could not again be established, so no further work with this animal was tried:

The results of the four animals in which conditioning to the fourth order was attained are presented in Table I. In column two, B indicates the electric bell-stimulus; A, the air-stimulus; L, the electric light-stimulus; and, T the 1000$\sim$ sound-stimulus. The stimulus-pattern BA indicates that bell and air were presented stmultaneously; AL, that the air and light were presented together and so on. The third column shows the ratio of responses to the number of times the given stimulus-pattern was presented during one test-period.

The 92% response-criterion was reached in 10 days by Animal No. 1, in 6 days by Animal No. 7, in 7 days by Animal No. 9, and in 9 days by Animal No. 10 (not given in Table I). Conditioning to the higher orders proceeded as has been described and the difficulties mentioned above surmounted with more or less ease. The following resume of training gives the total number of days on which training to each order occurred, both for the individual animals and for the group as a whole. Whenever two stimuli were combined, that day of training was counted as training for the higher of the two orders represented.

Animal	Order first	second	third	fourth	total
No. 1	20	42	9	3	74
No. 7	8	10	3	15	36
No. 9	14	17	8	8	47
No. 10	18	23	5	25	71
Total	60	92	25	51	228
Mean	15	23	6.25	13.75	57.00
S.E.$_M$	2.65	6.10	1.37	4.43	9.22

It is obvious that the second order was the most difficult to establish, the first next, then the fourth, whereas the third was the most easily established order. This is contrary to the results obtained by Finch and Culler, and by Brogden and Culler, who found a progressive decrease in the number of days of training from the first to the fourth order. On the other hand the total time required to establish higher order conditioning is about the same, whether shock or food be used as incentive for the higher orders. The difference is in the distribution of the time required to establish the individual orders.

Retraining after each extinction took but a short time (1 or 2 days) and was about the same for all orders, first and higher. Summing all retraining for all dogs, we find that 1 day was required in 24 cases, 2 days in 11 cases, 3 days in 3 cases, and 4 days in but 2 cases. This is similar to the results reported by Brogden and Culler.

In Table II is given the time of extinction, in days, for each order. By summing the scores for each extinction, a measure of the strength of the individual order is obtained. Since each order was extinguished in every position (first, second, third and fourth) the possible cumulative effects of earlier extinctions are roughly factored out.

There is a progressive decrease in the time required for extinction as one goes from the first to the higher orders. The total scores for the three dogs (animal No. 9 is omitted since a complete extinction series was not obtained) are as follows:

Animal	Order first	second	third	fourth	total
No. 1	26	15	7	6	54
No. 7	12	11	6	6	35
No. 10	34	14	13	6	67
Total	72	40	26	18	156
Mean	24	13.33	8.66	6	52
S.E.$_M$	6.45	1.20	2.18	0.00	9.29

The first order does not require a significantly greater number of days for extinction than the second, nor the second than the third, nor the third than the fourth, for in no case is the difference between any two successive orders more than twice its standard error (differences respectively: —10.66, S.E. 6.57; —4.66, S.E. 2.49; —2.66, S.E. 2.18). However, the first order requires a significantly greater number of days for extinction than does the third and fourth (differences resspectively: —15.33 S.E. 6.82; —18.00, S.E. 6.45) and the second order requires a significantly greater number of days for extinction than does the fourth order (difference: —7.33 S.E. 1.20). There is no relationship between the time required to establish any given order and its relative strength as measured by extinction. However, the animal (No. 7) which required the least training to establish the four orders (36 days) also required less time for the extinction series (35 days), whereas Animals No. 1 and No. 10 who required a longer training time, about equal in each case (74 and 72 days respectively), required a longer time for the extinction series, again the time for these two being roughly the same (54 and 67 days respectively).

DISCUSSION

The difficulty in establishing higher order conditioning (with food the incentive for the higher orders) is in direct contradiction to the results reported by Finch and Culler, and by Brogden and Culler. All of the dogs which these experimenters trained (using a thoracic shock as the incentive for the higher orders) became conditioned to the higher orders. Whereas, in this study, only 4 out of 10 dogs could be conditioned to higher orders. This difficulty must be due to the difference in the incentive for the higher orders. It is characteristic of an incentive, classified as a reward, that a given form of behavior must occur before such an incentive can act. On the other hand that type of incentive which can be classified as a punishment acts when there is failure of a given form of behavior. Food can serve as an incentive for the second order response only when the animal has made the appropriate response (forelimb flexion) to the second order stimulus. Even if response does occur to the second order stimulus, food is an incentive only when the animal eats. The thoracic shock is given when flexion to the second order stimulus fails. The shock forces the animal to do something; and since the second order stimulus has been associated with the first order stimulus, that something is forelimb-flexion. The difficulty in training all dogs to higher orders with food the constant incentive is then, in part, due to the action of the incentive, a reward. That this difficulty is not encountered when the thoracic shock is used as an incentive for the higher orders is due to the mode of action of the shock, a punishment. This cannot alone account for all dogs which failed to condition to higher orders. For two dogs failed to respond to the first and second order stimuli in combination. Their failure at this level has nothing whatsoever to do with the incentive for the higher orders. The addition of the second order stimulus to the first order stimulus was sufficient to cause almost complete failure of response. Further retraining of the first order stimulus combined with shock to the forepaw, did not aid in obtaining response to the first and second order stimuli in combination. These two animals differentiated between the first order stimulus alone, which was followed by pedal shock, and the first and second order stimuli which were not followed by shock. On this basis it can be expected that some dogs will fail to form higher order conditioned responses.

Once established, higher order conditioned responses motivated by food are similar to those where thoracic shock is the incentive for the higher orders. For after extinction, any given order is reëstablished very rapidly (within 1 or 2 days in most cases and no longer than 4 days in any case) whether the incentive be food or shock.

The strength of the higher order responses motivated by food and shock differ markedly. Brogden and Culler reported that the first order required 4 times more days for extinction than any one of the higher orders. The higher orders required about an equal number of days for extinction. In this study where food was the incentive for the higher orders, the first order required 1.8 times as many days as the second, 2.76 times as many days as the third, and 4 times as many days as the fourth. The second required 1.53 times as many days as did the third, and 2.22 times as many days as did the fourth. The third required 1.44 times as many days as did the fourth. Neither in higher order responses with thoracic shock the incentive nor in higher order responses with food the incentive was there any relationship between training-time and resistance to extinction. Nor was there any difference in experimental technique between the two, except that in one case, shock to the left thorax followed the second, third, and fourth order stimuli if there were no flexion; in the other case, food followed flexion to the second, third, and fourth order stimuli. The difference in results must then be due to the kind of incentive used for the higher orders. When thoracic shock is used, the three higher orders are equal in their resistance to extinction, requiring one quarter the number of test-days to reach the extinction criterion that the first order requires. When food is used, the three higher orders all require less time than does the first; the fourth requires less time than the third, and third less time than the second.

At first glance there seems to be a similarity between the present results and those reported by the Russian workers. For example: Frolov found that the salivary response to the second order stimulus was of less magnitude and of greater latency than was the salivary response to the first order stimulus; and Foursikov found that the flexion response to the third order stimulus was of less intensity and longer latency than that to the second order stimulus, and the response to the second order stimulus was in turn of less magnitude and of greater latency than the response to the first order stimulus. In the present experiment, response to the fourth order stimulus was less resistant to extinction than to the third order stimulus, response to the third less resistant than to the second, and to the second less than to the first. It must be remembered, however, that in the experiment just reported, response to the stimulus of each order reached the 92% level before each extinction. The magnitude and latency of the response (though not measured) was approximately equal for all orders. In terms of the measures used by Frolov and Foursikov the four orders are, therefore, equal in strength. Since neither Frolov nor Foursikov subjected their animals to experimental extinction, no other comparisons can be made.

In view of the difficulties encountered in producing higher order conditioning in the present case, it would seem wise to review the principles involved in the transfer of a given response from one stimulus to another, without there being a direct association between the secondary stimuli and the original stimulus for the response. Higher order conditioning in the Pavlovian sense offers little to our understanding of the transfer of a response from one stimulus to another. Finch and Culler, by introducing an incentive for the higher orders, made it possible to generalize to the functioning of human behavior. They likened the action of the thoracic shock to that of social incentives. A response can be transferred from one stimulus to another indefinitely, if conditions of social approval or disapproval supply "motivation" for the higher order stimuli. Since, however, higher order conditioned responses could be produced in only 4 out of 10 dogs, when food was used instead of shock as incentive for the higher order responses, the above generalization breaks down. For food may be analogous to social approval, and social approval functions as an incentive for humans as often as does social disapproval. The weakness in higher order conditioning as formulated by Finch and Culler lies in the conditions of transfer from first to second order. The first and second order stimuli are given in combination, but neither shock to the forepaw (the unconditioned stimulus and incentive for the first order) nor food or thoracic shock (the incentive for the second and higher orders) follow. There is no incentive for response to the first and second order stimuli in combination. This stage in higher order conditioning is difficult to pass and it was here that all of the animals in the present study failed. They either differentiated between the first order stimulus, and the first and second in combination, by responding to the first, and not to the first and second, or failed to respond, or respond frequently enough, for the incentive for the second and higher orders to be effective.

Let us suggest an alternative hypothesis to account for the transfer of a response from one stimulus to another, when the secondary stimuli are not directly associated with the original stimulus for the response. The unconditioned stimulus performs a dual rôle in the conditioning process.[6] It determines the form of the response; *e.g.* shock applied to the forepaw produces flexion. It provides the incentive for response. The conditioned stimulus takes over the first of these functions, that of evoking response. The unconditioned stimulus must, however, be present in its reënforcing capacity in order to prevent experimental extinction. But an indifferent stimulus, neither a conditioned nor unconditioned stimulus, can substitute for the incentive-function of the unconditioned stimulus.[7] If forelimb flexion be conditioned to bell, conditioned flexion will be of the same character if shock be omitted and food be given after each successful response. With this completely "conditioned" response, both evoked and maintained by stimuli previously indifferent, it should be easy to develop a second order response. With the conditions bell→flexion→food, no difficulty should be encountered in proceeding to bell+light→flexion→food and then to light→flexion→food. The third order would go, light+air →flexion→food to air→flexion→food, and so on to the fourth and an indefinite number of higher orders. By this procedure, there would be no time when adequate incentive for response was not maintained. Furthermore, in human behavior, where the number of stimuli and situations available either for eliciting or motivating response would be greatly multiplied, successive substitution for both functions of the original or unconditioned stimulus is possible. Not only can there be a transfer of a response from one stimulus, evoking response, to another, but also a transfer from one stimulus, providing incentive, to another. In other words, the transfer of a response from one elicitory stimulus to another would be brought about and accompanied by the substitution of one incentive-situation for another.

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[6] Finch and Culler, Relation of forgetting to experimental extinction, this JOURNAL, 47, 1935, 656-662.

[7] W. J. Brogden, Unconditioned stimulus-substitution in the conditioning process, *ibid.*, 52, 1939, 46-55.

Summary

(1) Higher order conditioned responses, with food the incentive for the higher orders, could be established in only 4 of 10 animals.

(2) In the original training the second order required the greatest number of trials to reach the response-criterion, the first next, then the fourth, whereas the third was the most easily established order.

(3) The first order was the most resistant to extinction, the second next, the third next, and then the fourth.

(4) Retraining after each extinction took but a short time (1 or 2 days) and was about the same for all orders, first and higher.

(5) There is no relationship between the speed of conditioning and the speed of extinction.

(6) The principles of higher order conditioning are examined and an alternative hypothesis is suggested to account for the means of transfer of a response from one stimulus to another, when there is no direct association between the secondary stimuli and the original or unconditioned stimulus for the response.

RETROACTION AND PROACTION IN SERIAL LEARNING

By GERTRUDE RAFFEL SCHMEIDLER, Oradell, N.J.

It is usual in work on retroactive inhibition to present successively for learning two materials and to examine only the effect of the second upon recall of the first. In the total learning situation, however, it has been demonstrated that a prior learning may affect a later one. The present experiment was undertaken to show that the interaction between the first material and the second may be more significant than is either the proactive or the retroactive effect taken alone.[1]

During the course of the research other problems arose, some of which have been studied in detail. The data show that interpolated material or interpolated activity different from the original learning can cause retroactive inhibition. Comparable proactive effects are, moreover, produced by dissimilar material or activity preceding learning. These results appear to be dependent on the difficulty of the material and on the length of the intervals of presentation.

All the experiments used nonsense material presented serially. A series of two units affords the typical condition of retroactive inhibition: learning the first unit is followed by learning a second. As the series increase in length analysis becomes more complicated, but the intrinsic nature of the problem is unchanged. The chief advantage of serial presentation is the flexibility that it offers by the use of short units, approximately equal in difficulty.

PROCEDURE

Learning material. The learning material was of two types: three consonants presented as a unit, or three lines radiating from one central point. The consonant groups were chosen by writing each of the twenty consonants on a separate slip of paper, arranging them in random order, and dividing them arbitrarily into groups of three. Any combination which *E* believed to be too easy was modified or discarded; such a group might consist of an alphabetical sequence or a familiar set of initials. All the letters were used approximately the same number of times. The groups were presented like nonsense syllables, but reported by being spelled rather than pronounced as a unit. In scoring, each correct letter was given one point credit, whether or not its position in the group was correctly recalled. The maximum score for a group was 3.

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[1] This experiment was performed in the Psychological Laboratories of Harvard University, where it profited greatly from the suggestions and criticisms of Dr. Dwight W. Chapman.

The lines presented to *O* were 5 mm. long, each pointed in one of the 12 positions of the numerals on a clock. In preparing the material *E* wrote the numbers 1 to 12 on separate pieces of paper and arranged them haphazard in groups of three, then ruled lines in the positions designated by the numbers. Care was taken in composing a series of such units not to present similar visual patterns close to each other; except for this precaution the material was arranged by chance. *O* reproduced the lines in recall by drawing them; he was always instructed to put each line in one of the twelve positions, and not an intermediate one. A line in the correct position was given a score of 1; two lines in the right relation to each other (with an accurately reproduced angle between them) were also scored 1; if all three lines were in the right relation but were misplaced, they were given 2 points instead of the maximum 3. Scores on the lines were consistently lower than on the consonants.

The material was presented on a small memory drum placed on a table in front of *O*. The consonants were typewritten in capital letters; the lines were drawn clearly in pencil. The members of a series were always shown in immediate succession; each was exposed for 1.5 sec. The consonants were reported verbally; the lines were drawn. In an attempt to make the two learning conditions more similar, *O* was required to repeat a word during the entire exposure period; he would say "Friday," for example, at a uniform rate, perhaps three times a second. This response rapidly became automatized in practice trials so that it was not a distraction in any of the learning series; it prevented a verbal reinforcement of the consonants which would necessarily be impossible for the lines.

Order of recall. When a series has been partially learned *O* may recall it in any order he chooses, or he may be required to recall it in the order in which it was presented. The disadvantage of the first alternative is that the time between presentation and recall is not kept constant for all the units; the ones shown last may benefit because they can be reported soon after they have been seen. The disadvantage of the second alternative is that such instructions tend to make *O* emphasize the first part of the list in learning.[2]

An attempt was made to have *O* distribute his effort equally over the entire list and still to retain a constant interval between exposure and recall, by requiring a report on only one member of each series. For a series of five groups of consonants, for example, the five units were presented successively. After the last had been shown, the memory drum continued to revolve; if the first group was to be reported the numeral 1 appeared in the next space. If the second was to be designated there was one blank space, exposed for 1.5 sec. after the fifth group of consonants, and then a 2; for the third two blank spaces followed by a 3, and so on. Six seconds always elapsed between exposure of a unit and the exposure of the number which indicated that it was to be reported. The numbers were arranged in chance order. In the terminology of retroactive inhibition, we may say that there was 6 sec. interpolated activity for position 1, a 6-sec. rest period for position 5, and intermediate ratios of work and rest for the intermediate positions.

The second method of recall was the simplest possible one. As soon as the list had been shown, *O* reported all he could remember in any order he chose. For the series of mixed consonants and lines, he could draw the lines and then report the consonants, report the consonants and then draw the lines, or do both at once. There was no limitation on the length of time used for recall. With either method, each list was shown only once.

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[2] G. Raffel, Two determinants of the effect of primacy, this JOURNAL, 48, 1936, 654-657.

For 8 series, totaling 44 lists, 4 *O*s used one method of recall and 4 *O*s another. Differences among the series followed the same pattern for both methods of recall. For the purposes of this experiment, therefore, the two methods were equally useful; and the experiment was continued with the second and more convenient method.

Tachistoscopic exposure. An exposure period of 1.5 sec., as used here, is much briefer than is customary in studies of retroactive inhibition. It was considered desirable, however, to explore even further in the direction of short-time intervals, using the shortest possible time intervals that permit successive presentation of units. Hylan has shown that, when two or more units are exposed tachistoscopically, they are perceived successively even though they had been shown side by side in the tachistoscope window.[3] When *O* is instructed to read the stimulus-card from left to right, he perceives first the unit on the extreme left, then the second from the left, and so on. This procedure fulfills the requirements of serial presentation, and was therefore employed.

The material used earlier was simplified slightly here: two consonants instead of three, and two lines radiating from a central point were used as units. There were 4 series, each of which contained 10 exposure-cards; on each card were two units placed beside each other. The consonants were printed in gummed letters; the lines were drawn in India ink, and were approximately the same height, width, and blackness as the letters.

M and W were excluded from the consonants because of their heaviness; the remaining eighteen were all used four or five times apiece. The lines were presented as before in the 12 positions of the hands of a clock; separations of 180° were not used. This allowed 5 possible angles of separation; each angle appeared 8 times. No combination of lines was repeated.

The tachistoscope employed in the experiment used an episcotister, and allowed an exposure of 60°. The *O*s were instructed to read the cards from left to right, and to write the letters or lines as soon as they had been exposed. A ready signal was given from 1 to 1.5 sec. before each exposure. The experimental session consisted of an explanation of the procedure; the exposure of 4 practice-cards (each was representative of one of the 4 series of this portion of the experiment); repetition of these cards if *O* requested it; presentation of the 4 series. There were 5 *O*s, making it possible to vary the order of presentation so that each series was presented first, second, third, and fourth.

As in the previous scoring, a consonant reported correctly or a line drawn in the right position was given a score of 1; and if the position of both lines was wrong but the angle of separation was reproduced correctly a score of 1 (half credit) was given. The maximal score for any series was 20 for each position. Since two consonants or one pair of lines had been demonstrated in preliminary work to be within the span of attention, scores much lower than 20 indicate inhibition between the units.

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[3] J. P. Hylan, The distribution of attention, *Psychol. Rev.*, 10, 1903, 373-403, 498-533.

Multiplication. In one pair of series, two units of interpolated activity consisted of multiplication instead of learning. In one case 3 groups of consonants and then 2 simple multiplication problems were exposed; for the other series there were 2 multiplications and then 3 groups of consonants. There was the usual 6-sec. interval between exposing a group of consonants and designating it for report, for the *O*s with the first method of recall. No record was kept of the accuracy of the multiplications.

The instructions with this procedure were: "You are going to be shown two multiplications, such as 8 x 9, or 2 x 17, and three groups of consonants. As you see each group of consonants, try to learn it. When you see the multiplication, say the answer aloud. You will not be asked to remember the multiplications, so do not try to learn them. The purpose of this series is to have you do some kind of work other than learning for three seconds; all you are expected to do is to try to multiply. If you cannot get the answer while the problem is being shown, forget about it as soon as the drum turns; as long as you were busy thinking about it when the multiplication was in front of you, you did the right thing. Your answer will not be recorded."

After the practice trials the *O*s had little difficulty in following instructions. They gave every indication of trying to perform the multiplications, but dismissed them readily after the exposure period, with little or none of the tension that might be expected to follow an incompleted task.

Series Used in the Experiment. Six *O*s learned the first 13 series. Those series were constructed as is indicated in the following list.

Series I	2 groups of consonants
Series II	3 groups of consonants
Series III	5 groups of consonants
Series IV	10 groups of consonants
Series V	2 sets of lines
Series VI	3 sets of lines
Series VII	5 sets of lines
Series VIII	3 sets of lines followed by 2 groups of consonants
Series IX	2 groups of consonants followed by 3 sets of lines
Series X	2 sets of lines followed by 3 groups of consonants
Series XI	3 groups of consonants followed by 2 sets of lines
Series XII	2 multiplications followed by 3 groups of consonants
Series XIII	3 groups of consonants followed by 2 multiplications

Five *O*s learned the following series, which were exposed on the tachistoscope.

Series XIV	2 groups of consonants
Series XV	2 sets of lines
Series XVI	1 group of consonants followed by 1 set of lines
Series XVII	1 set of lines followed by 1 group of consonants

Results and Discussion

The data are presented in Tables I and II. When, as in Series I, two groups of consonants are presented successively to *O*, both the first and the second can be reported equally well. A series of this length seems to be within the memory span, as the term refers to the case in which *O* can almost always reproduce material perfectly.

When a series of three groups of consonants is presented, however, the scores for all the positions are lower than in the previous case. Apparently Series II, containing nine consonants, exceeds the span of memory; only 6.7 are reported as a mean score. Such a sequence may be considered to be composed of a series of two groups, comparable to Series I preceded by a single group of consonants. From this point of view the second and third positions are equivalent to the first and second positions of Series I; the scores obtained might then be predicted to be—,2.8, 2.8. Since the actual scores are—,2.1, 2.5, it is clear that the first group exerts a proactive inhibitory effect upon the second and third. Following the same line of reasoning, the third position shows a pronounced retroactive inhibitory effect upon the first and second. But the analysis is incomplete without consideration of the series as a whole, which shows that the retroactive inhibition is stronger than the proactive, since the score of the last position is higher than that of the first two.

TABLE I

SCORES OBTAINED AFTER A SINGLE PRESENTATION OF EACH SERIES ON A MEMORY DRUM

The scores of the positions in which consonants were learned are printed in lightface; those of the lines are printed in boldface.

(Maximum score=3)

Series	Position 1	2	3	4	5	6	7	8	9	10
I	2.8	2.8								
II	2.1	2.1	2.5							
III	1.1	1.3	1.7	1.9	2.7					
IV	1.2	1.2	1.2	1.0	1.2	1.4	1.3	1.1	2.1	2.7
V	**2.1**	**2.5**								
VI	**1.7**	**1.9**	**2.3**							
VII	**1.4**	**1.7**	**1.4**	**1.4**	**2.3**					
VIII	**1.4**	**1.7**	**1.5**	2.5	3.0					
IX	2.7	2.7	**1.5**	**1.6**	**2.1**					
X	**1.6**	**1.6**	1.3	2.1	2.6					
XI	1.5	1.9	2.2	**1.6**	**2.0**					
XII	—	—	1.9	2.1	2.1					
XIII	1.6	1.6	1.9	—	—					

In Series III, fifteen consonants were presented, and the mean number reported was 8.7, although only 6.7 had been recalled when nine consonants were exposed. If the same type of analysis is made of these data as of those of Series II, it will be found that the mean of the last two positions is only slightly lower in Series III than in Series II. This indicates that the proactive inhibition rapidly approaches a maximum as the number of units is increased.

This increase brings about one interesting change: although the mean score of the last positions is relatively constant, the difference in value of the different positions becomes more pronounced. Presented graphically, this appears as an increased tilt of the curves of score *vs.* position; the last position has a greater advantage over the penultimate in a series of five units than in a series of three. The retroactive inhibition is also greater in this series than in the preceding one: the mean of the first two positions is 1.2 when they are followed by three groups of consonants, but it is 2.1 when they are followed by only one consonant group.

In Series IV, ten groups were presented successively, and a levelling off in the retroactive inhibition appears which seems directly opposed to the tilting of the proactive effect: the scores of the first eight positions vary about 1.2, and this variation is clearly within the limits of experimental error. The mean of the last two positions is somewhat higher in this series than in Series II and III, while the mean of the last three positions is only slightly lower than in Series III. Proactive inhibition, then, shows little change with increasing length, but proactive tilt is accentuated; positions 1-3 in Series II scored from 2.1 to 2.5; positions 3-5 in Series III from 1.7 to 2.7; and positions 8-10 in Series IV from 1.1 to 2.7.

The same general tendencies appear in the series of lines, if allowance is made for the fact that the lines were more difficult than the consonants. Difficulty rather than quantity of the material to be learned determines the influence of inhibitory effects, as can be seen in Series V, where two sets of lines were presented. The number of errors was relatively high—an accuracy of only 70% for the first position—so that in this case a series of two lies beyond the memory span. As occurs with the consonants when the length of series exceeds the span, there is a difference in score between the positions. The first is lower than the second, indicating again that retroactive inhibition is stronger than proactive inhibition.

In the series of three sets of lines, the scores are lower; and in Series VII, with five sets of lines, the scores for all positions except the last are lower still. This series seems to show the same pattern of position effects as appeared in Series IV, where there were ten groups of consonants. Assuming the lines to be about twice as difficult, so that one set of lines is equivalent to two groups of consonants, the parallel is marked: the first eight positions of Series IV have approximately the same score, and the ninth and tenth are higher; the first four positions of Series VII are approximately uniform, and the fifth is higher.

According to most theories of retroaction, inhibition does not take place between dissimilar materials. The next four series test this theory: each of them is composed of five units, of which either two or three are consonants, and the other three or two are lines. If the scores are like those where only two or three units were presented, no interaction has occurred; if the scores are lower or show proactive tilt, then there has been inhibition or interaction.

A glance at the data of Table I shows that the second alternative describes the results. When three sets of lines are presented alone, their scores are 1.7, 1.9, 2.3. When three sets of lines are followed by two more sets, the scores are 1.4, 1.7, 1.4. When they are followed by two groups of consonants, the scores are 1.4, 1.7, 1.5—almost identical with the preceding case.

In the same series, Series VIII, the two groups of consonants score 2.5 and 3.0 Two consonant groups presented alone (Series I) score 2.8, 2.8. Thus it seems that the three first units caused a proactive tilt of the two last units; and those two last units caused a retroactive inhibition of the three first ones. This reciprocal effect is present in Series X and XI as well as in Series VIII, although it is not apparent in Series IX, where the two first sets of consonants score almost as high as if they had been presented alone, but lower the scores of the three sets of lines that follow them. These data warrant the conclusion that both proactive and retroactive inhibition can occur between dissimilar learning materials.

Robinson has suggested that besides a similarity of content between the original and the interpolated materials,[4] there may be a similarity of forms of presentation or of the processes involved. Since this experiment is concerned with serial learning, a certain formal similarity of presentation is necessary. "Processes involved" probably refers to the set of *O;* this factor was varied in the next two series. Instead of being required to learn both the original and the interpolated activity, *O* was instructed to learn three groups of consonants, and to perform multiplications in the other two units of the series.

The data of Series XII and XIII show that multiplication inhibited learning the consonants. Three groups of consonants presented alone scored 2.1, 2.1, 2.5; three such groups presented after multiplications scored 1.9, 2.1, 2.1; when multiplication followed the three groups, the scores were depressed to 1.6, 1.6, 1.9. The proactive effect is as usual less pronounced than the retroactive; but in both cases there is evidence of inhibition.

In most experiments on retroaction, no inhibition has been found when dissimilar materials were learned. In the series just described, retroaction has been demonstrated not only with dissimilar learning material but even with different interpolated activity. Much shorter time intervals than are customary were used here; this may have been the cause of the increased susceptibility of the material to inhibition. In that case, even shorter time intervals may result in even more pronounced inhibitory effects between dissimilar materials. This possibility was tested in the next four series, with tachistoscopic presentation. The results are presented in Table II.

Two groups of consonants were shown in Series XIV, and two sets of lines in Series-XV. In both series, the first unit was reported more accurately than the second by each *O;* this shows that the material was read from left to right, according to the instructions. The scores of the consonants were consistently higher than the scores of the lines; we may infer that the lines were more difficult.

TABLE II

Scores Obtained after a Single Presentation of Each Series on a Tachistoscope

Thes cores of the positions in which consonants were learned are printed in lightface; those of the lines are printed in boldface.
(Maximum score=20)

Series	Position 1	Position 2
XIV	18.8	17.8
XV	**14.4**	**10.8**
XVI	18.4	**11.4**
XVII	**14.4**	16.4

Series XVI and XVII were mixed, composed of one consonant group and one set of lines. The consonant scores are slightly lower, for each position, than they were when only consonants were shown; the scores of the lines are the same for the first position and slightly higher for the second. This implies that with tachistoscopic exposure, inhibition is determined by the difficulty rather than the similarity of the material: lines have a stronger inhibitory effect than consonants, whether the material they inhibit be consonants or other lines.

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[4] E. S. Robinson, Some factors determining the degree of retroactive inhibition, *Psychol. Monog.,* 28, 1920, (no. 128), 1-57.

THEORY

A large group of diversified experiments deals with the relation between general body processes or affective and emotional conditions, on the one hand, and retroactive inhibition, on the other.[5] This work shows that pleasant odors and sleep after learning facilitate report; unpleasant odors, unpleasant emotional conditions and activity tend to inhibit it. Such conditions probably have on *O* a general effect, which would influence most of his activities as well as being detrimental to recall. The experiments show, therefore, that the retention of material that has been learned is not wholly independent of the rest of the body processes. On the other hand, most of the experiments which relate inhibitory effects to similarity indicate a high degree of specificity for learned material. Apparently memory of such material is affected somewhat by the general activity of *O*, but is at the same time differentiated from other memories.

The results of the experiments reported here indicate that the differentiation does not take place instantaneously at the exposure of the learning material. With a very brief interval between perception of the units, as in tachistoscopic presentation, the memory systems are not individuated and thus learning of any sort has as strong an inhibitory effect as has learning of similar material. Differentiation has begun but is not complete with the time intervals of a few seconds used in the other series; dissimilar materials or even dissimilar activities affect each other, although by this time the strongest inhibition comes from material of the same type as that which is being inhibited.

It is likely that the speed of individuation is not constant, but depends upon the difficulty of the material.[6] An easy task is probably dissociated sooner than a more difficult one from the mass of unrelated activity in the organism; after a relatively short period it would therefore be resistant to inhibition. In a more difficult task the changes would be slower, as well as weaker. According to this reasoning, with tachistoscopic exposures all learning material is highly susceptible to inhibition, even from unlike material; with time intervals of a few seconds (as in Series I to XIII) easy material is less liable to inhibition from dissimilar presentations than is difficult material (lines were more readily inhibited by consonants then were consonants by the lines); and after longer periods this susceptibility lessens or disappears, as it does in most experiments. The theory thus accounts for the major findings of this experiment.

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[5] Representative experiments are: J. D. Frank and E. J. Ludvigh, The retroactive effect of pleasant and unpleasant odors on learning, this JOURNAL, 43, 1931, 102-108; L. M. Harden, The effect of emotional reactions upon retention, *J. Gen. Psychol.,* 3, 1930, 197-221 J. G. Jenkins and K. M. Dallenbach, Obliviscence during sleep and waking, this JOURNAL, 35, 1924, 605-612; E. B. van Ormer, Retention after intervals of sleep and of waking, *Arch. Psychol.,* 21, 1932, (no. 137), 1-49.

[6] B. Ribsskog, Experimentelle Untersuchungen über die für den Unterrichtserfolg günstigste Repetitionszeit, *Zsch. f. angew. Psychol.,* 43, 1932, 301-347.

CONCLUSIONS

(1) When series of nonsense material are learned, as the series increase in difficulty there is (a) a retroactive inhibition of the earlier units which increases at first, but seems to reach a maximum, and (b) a proactive effect which may take the form of inhibition or of tilt.

(2) Proactive inhibition is weaker than retroactive inhibition.

(3) When experimental conditions are varied, parallel changes occur in the retroactive and the proactive effects.

(4) With time intervals of the order of 8 sec., dissimilar materials inhibit each other almost as much as similar ones.

(5) With time intervals of the order of 8 sec., multiplication causes both proactive and retroactive inhibition of learned material.

(6) With tachistoscopic exposure, inhibition is determined by the difficulty rather than the similarity of the material.

(7) Material that has been learned becomes individuated during retention.

(8) The speed of individuation becomes slower as the difficulty of the material increases.

EFFECT OF CROWDING OF MATERIAL ON CURVES OF FORGETTING

By EDWIN B. NEWMAN, Swarthmore College

In the effort merely to add greater precision to scientific findings, conditions are sometimes so altered that a new experimental situation is created, and to the problems originally envisaged are added new ones. In attempting to increase the statistical reliability of experiments on learning, for instance, often such large masses of items are used that interfering or disruptive forces among these items are introduced into the learning process. Facts established with single or isolated learning units are not always true for longer series of massed units. In the experiments to be reported in this paper, the effect of this massing of material on the conventional curves of forgetting has been investigated.[1] Two particular considerations motivated the study.

First, Jenkins and Dallenbach and Van Ormer have reported forgetting curves for intervals of sleep and waking.[2] In a comparison of the results obtained in these two investigations, it appears that the forgetting curves for intervals of waking have markedly different shapes. The curve in the study of Jenkins and Dallenbach shows a loss during the first hour of approximately 50% of the material originally learned, with a continued drop during the entire 8-hr. period until 90% has been lost. Van Ormer's curves, on the other hand, show a slightly greater loss, 55%, during the first hour, and then only a comparatively small drop for the remainder of the 8-hr. period to 75%. While it is possible that the inconsistency in these results is due to arbitrary differences in the technique of the experiments, it seems more probable that the larger amount of material, approximately three times as much, learned by Van Ormer's *Ss* at any one experimental session would have a bearing on the outcome of the experiment.

This view is supported by results reported by Woodworth, using lists of from 5 to 30 sets of paired words.[3] After 3 repetitions the list of 5 pairs

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* Accepted for publication February 28, 1939.

[1] The experiments reported were conducted by members of undergraduate honors seminars at Swarthmore College. Credit is due to Margaret Chase, Peggy Harding, Ann Hargreaves, Helen Crosby, and Dagny Hoff for their respective shares in the experiments.

[2] J. G. Jenkins and K. M. Dallenbach, Obliviscence during sleep and waking, this JOURNAL, 35, 1924, 605-612; E. B. van Ormer, Retention after intervals of sleep and of waking, *Arch. Psychol.*, 21, 1932, (no. 137), 1-49.

[3] R. S. Woodworth, The influence on retention of conditions favoring quickness of learning (abstract), *J. Philos.*, 12, 1915, 246. Cf. also, Woodworth, The better

showed 100% correct associations while the list of 30 pairs showed only 71% correct. Two days later the relative position of the lists was reversed; the list of 5 pairs had fallen to 4% correct, while the list of 30 pairs dropped only to 34%. Since the time devoted to each pair was equal in the two cases, Woodworth suggested increased effort on the part of the *Ss* to account for the clear-cut superiority of the longer list over the single list in the second recall. The present experiment was formulated, therefore, as an attempt to clarify this situation, that is, to test the effect of the amount of material learned on the shape of the forgetting curve.

Secondly, however, certain important theoretical considerations are involved. If forgetting is an expression of interference by events occurring in the daily experience of *S*, a masking of the original traces, then a considerable measure of such interference introduced intentionally on the occasion of the original learning should have a marked effect on the shape of the forgetting curve.[4] It might be assumed, for example, that material which has been mastered in spite of the strong intra-serial interference present in a long list would now be less subject to the relatively less important influences of subsequent chance events. Conversely, relatively isolated experiences would be under a continual detrimental influence because of the proportionately larger effect of the events of daily life. This hypothesis is doubtless too simple, as the facts reported below will suggest, but it indicates at least the general kind of relationship which must hold between these two conditions.

THE EXPERIMENT

Two groups, totaling 21 students, learned by the anticipation method single lists of 8 nonsense syllables, and, at other times, 3 such lists of 8 syllables in quick succession. After intervals of 1, 24, and 48 hr. the *Ss* relearned the same lists. Retention was measured in terms of the conventional savings, that is to say, the difference between the trials necessary for the original learning to one perfect repetition, and trials for relearning to the same criterion. Each *S* served in 6 single experiments, one under each of the variations of the experimental conditions indicated above. The influence of the relative difficulty of the 12 different lists of material used and of the fairly sizable over-all practice efforts was equalized by rotation of both the lists and of the order of experiments for the various *Ss*. Thus 7 *Ss* relearned first after 1 hr., 7 other *Ss* first after 24 hr., and the remaining 7 after 48 hr.,—the order for single lists being the reverse of the order for the triple

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retention of longer memory lessons (abstract), *Psychol. Bull.*, 13, 1916, 65; and *Experimental Psychology*, 1939, 19-20, 56-60.

[4] The theory that forgetting depends upon interference has been clearly formulated by J. A. McGeoch, Forgetting and the law of disuse, *Psychol. Rev.*, 39, 1932, 352-370. So far as the writer is aware, however, no critical review of the older facts about forgetting in terms of this theory has been published.

lists. Group I, consisting of 9 *S*s, learned all 3 single lists before starting the triple lists. Group II, containing 12 *S*s, learned the single and triple lists interspersed so as to balance practice effects in the original learning. The extent to which these controls were successful may be judged by comparing the rates of learning indicated in Table I below.

RESULTS

Original learning. The number of trials required by the two groups to learn the lists is given in Table I. In each case the results for all the lists learned before the 1-hr. interval are grouped together; results for the other two intervals are arranged in the same way. The values in the table each represent the mean for all *S*s in that group. Comparisons of these values, as well as the size of the standard deviations reported, indicate rather large variability from one *S* to the next. In addition, the rate of learning for the triple lists is significantly faster than the rate for the single lists in Group I. The procedure for this group indicates that this difference is due to an over-all practice effect. For the purposes of this paper, however, this difference is not of major importance, since we are interested in the slope of the forgetting curves; in the subsequent discussion, Group I and Group II are treated together. While the rates of learning for the various sub-groups are not as uniform as might be desirable for greatest reliability, the absence of systematic differences enables us to treat them as functionally equivalent.

TABLE I

TRIALS REQUIRED FOR ORIGINAL LEARNING

(Each value is the mean of values for all 8-syllable lists learned by *S*s in each group preceding the interval indicated.)

Group	Hours to relearning	Single lists	Triple lists
	1	7.33	4.89
	24	7.78	5.30
I	48	6.22	5.44
	Mean	7.11	5.21
	$S.D._{mean}$	.49	.20
	1	8.50	6.97
	24	6.92	6.75
II	48	6.83	6.64
	Mean	7.42	6.79
	$S.D._{mean}$	.71	.40

Forgetting. Forgetting may be calculated either by finding the percentage saving for each individual *S* and then taking the mean of these percentages, or by averaging the actual number of trials for learning and for relearning and calculating the saving from these two means. In this experiment, the two methods give very nearly identical results, but since the latter gives more concrete information it has been used in the subsequent presentation. In Table II are given the data relative to forgetting, and the final results are presented graphically in Fig. 1.

Four values are given for each of the six experimental conditions, the mean number of trials required for the original learning, the mean number of trials for relearning, the saving in terms of trials, and finally, the saving in percentage of trials for original learning. In the column headed "Difference" are given comparisons of the savings for the single list and the triple lists at each interval. The standard deviation and critical ratio for this difference has been calculated directly from the differences of the individual *S*s. In the lower part of the table are presented, correspondingly, comparisons of savings for the 1-hr., 24-hr. and 48-hr. intervals, together with their critical ratios computed again directly from the differences for each individual subject.

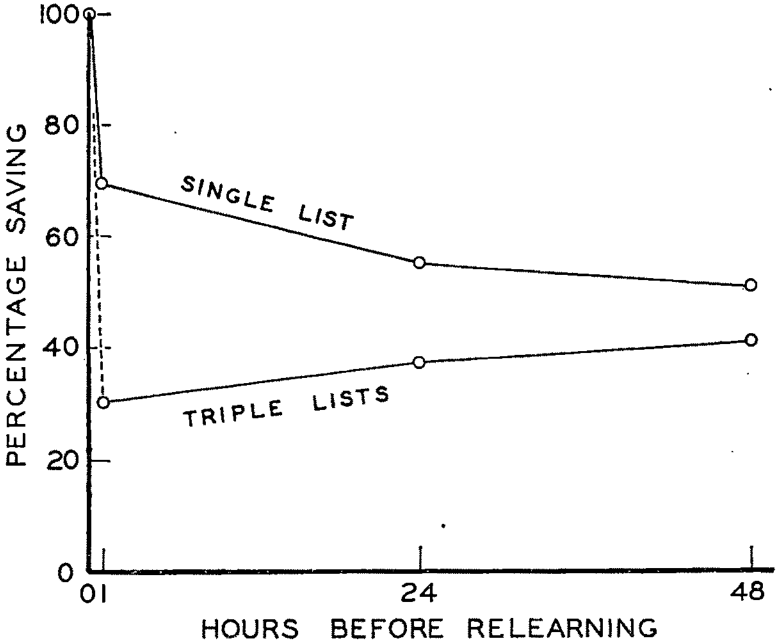

FIG. 1. CURVES OF FORGETTING, SHOWING RETENTION MEASURED BY SAVINGS UPON RELEARNING

The upper curve is for a single list of 8 syllables; the lower curve is for 3 lists of 8 syllables learned in quick succession.

At first glance it may be seen that the expectation of a marked difference in the slope of the forgetting curve for the single list and the triple lists is borne out. The triple lists show more than twice as much forgetting as the single list at the one-hour interval, only one and a half times as much as the single list at the 24-hr. interval, and but slightly more than the single list at the 48-hr. interval. The 1-hr. difference is highly reliable, the 24-hr. probably reliable, and the 48-hr. not reliable. Time obviously affects the two cases differently, working to the advantage of the triple lists and to the comparative disadvantage of the single list. This demonstrates, so far as is possible, that the difference between the forgetting curves of Jenkins and Dallenbach and of Van Ormer is a function of the amount of material each gave his *S*s to learn, and it further gives us a significant clue to the relatively less clear-cut outcome of Van Ormer's experiment.

TABLE II

TRIALS REQUIRED FOR LEARNING, RELEARNING, AND SAVINGS FOR INTERVALS OF 1, 24, AND 48 HR., FOR SINGLE LISTS AND FOR 3 LISTS OF 8 SYLLABLES LEARNED AT ONE TIME

(Critical ratios are calculated from S.D.$_{\text{diff}}$ found directly from differences for each *S*.)

Time		Single list	Triple list	Difference S–T	Critical ratio
1 hr.	Learning, trials	8.00	6.07		
	Relearning, trials	2.43	4.24		
	Savings, trials	5.57	1.83	3.74	4.37
	Savings, %	69.6	30.1	39.5	
24 hr.	Learning, trials	7.28	6.13		
	Relearning, trials	3.28	3.80		
	Savings, trials	4.00	2.33	1.67	2.46
	Savings, %	54.9	37.0	17.9	
48 hr.	Learning, trials	6.57	6.13		
	Relearning, trials	3.24	3.63		
	Savings, trials	3.33	2.50	.83	1.69
	Savings, %	50.7	40.7	10.0	
Difference (24–1), trials		−1.57	.50		
Critical ratio		2.16	1.35		
Difference (48–24), trials		− .67	.17		
Critical ratio		1.00	.41		
Difference (48–1), trials		−2.24	.67		
Critical ratio		4.77	2.31		

Much more striking was a result which appeared quite unanticipated by me. Some of the first results with the triple lists indicated a greater degree of saving at the 48-hr. interval than at the 1-hr. interval. This result was at first attributed to chance, but further experimentation only served to reenforce the original observation. A glance at Fig. 1 will show quite clearly that there is a continuous rise from the 1-hr. interval through the 24-hr. to the 48-hr. interval: more is remembered of the triple list the longer the interval before it is tested. The differences are not large on an absolute scale, but the rise is so consistent from *S* to *S* that the approximately 10% increase between the 1-hr. and the 48-hr. intervals appears statistically significant, and it is probable that the smaller differences would prove reliable were a sufficient number of *S*s employed.

Even though this experiment was not set up so as to be formally comparable to the conventional experiments on reminiscence, the parallel between the results noted above and those designated by the term *reminiscence* is so striking as to suggest that they actually represent the same phenomenon.[5] Moreover, the procedure of this experiment did not actually require that at any one moment the *S*s should have complete mastery over all three of the triple lists. Thus the 100% point plotted for a zero time-interval and the steep slope of the curve from this point down to that for 1 hr. have no real significance, and we have to consider merely the trend of the curve between 1 and 48 hr., which shows clearly the parallel to which I have referred.

DISCUSSION

It is possible to formulate certain logical consequences of these experimental findings.

In the first place, two general alternatives are available to explain the increase in performance on the triple lists with the passage of time: either the traces involved (or any equivalent facilitating agent) are strengthened, or an interference initially present is lessened. A strengthening might result from either a process of perseveration or consolidation, or from processes of growth or maturation. The increase in the retention of specific items which is present in the experiment could with difficulty be accounted for by either of these alternatives.[6] The second alternative, the lessening of an initial interfering or inhibiting process, seems at the present time far more likely. The initial interference might have something to do with factors like fatigue or loss of interest; or the inhibitory effect might have its origin within the material itself, in the form of interaction among its items.[7] The last of these explanations is the only one which appears satisfactory, and I shall tentatively adopt it.

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[5] The work on reminiscence has been adequately reviewed by G. O. McGeoch and will not be gone into here; cf. G. O. McGeoch, The conditions of reminiscence, this JOURNAL, 47, 1935, 65-89.

[6] It cannot be denied that the improvement in certain types of performance may be adequately explained by a concept of maturation. James's example of learning to swim in the winter is too well known to need elucidation. Cf. R. H. Wheeler, *The Science of Psychology*, 1929, 290-300, 319-322, 495-496. Snoddy's experiments on mirror drawing have been cited in support of this theory: G. S. Snoddy, An experimental analysis of a case of trial and error learning in the human subject, *Psychol. Monog.*, 28, 1920, (no. 124), 1-78. On the other hand, the disappearance of Snoddy's "irradiation pattern" during a rest period may be similar to the subsidence of initial interference posited in this paper.

Second, since the single lists do not show "reminiscence," while the triple lists do, crowding of material is indicated as the condition which is specifically responsible for the interference postulated above.[8] Crowding introduces the probability of strong interference in the initial learning and therefore the possibility that such interference will subside in the course of time. With less crowding, this smaller amount of initial interference allows for but little change in time. One need not assume, of course, that such interference is entirely absent in the single lists. On the contrary, it is probably present in no inconsiderable amount. One need only postulate that the intra-serial interference in the single list is not so large that the facilitating effect of its removal will offset the decrement due to the chance inhibitory forces of ordinary forgetting. According to this view, any actual forgetting curve will be the resultant of (1) interfering or inhibitory processes accumulating in time, and (2) the decreasing effectiveness of any interference with the passage of time. The latter would include both interference present at the time of original learning, and that accruing in the process of forgetting.[9]

Third, the fate of the intra-serial interference may be specified to some degree. The results of the present experiment show that this interference gradually decreases during the period from 1 to 48 hr. after learning. It is doubtful whether the three points on the curve given above furnish any clue to the complete course of this function, but it is important to emphasize the relatively long period, certainly considerably more than one hour, during which "reminiscence" is found.

The conclusions which have been reached in this discussion are not entirely novel. In several important respects they agree with predictions made by Ward and by Hovland on the basis of deductions from so-called conditioning experiments.[10] Ward and Hovland have pointed to the temporary character of inhibition in the learning process as the source of the reminiscence effect. This intra-serial inhibition depends upon "inhibition of delay" derived from remote excitatory tendencies spanning any given syllable. Implicit in their assumptions is the conclusion that larger amounts of material should introduce proportionately greater amounts of inhibition. This last factor, however, has been relatively neglected in favor of the variable used by Hovland; namely, the spacing of the learning trials.[11] Unfortunately, the effect of spacing may be interpreted in two different ways. Either spacing in time may allow some kind of an interference or inhibition to disappear during the learning process so that a subsequent pause shows no additional effect, or the spacing may effectively eliminate the initial crowding or massing of material which produces the interference or inhibition. It seems important, therefore, to lay particular stress upon the fact that interference is a direct result of a crowding, or massing of homogeneous or near-homogeneous items in a particular learning process.

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[7] The probability that fatigue plays a rôle in these experiments may be excluded by noting that the rate of learning for the triple lists is slightly lower than that for the single list in Group II when, if anything, it might be expected to be higher. This difference, although statistically unreliable, suggests a warming-up during the learning period rather than any fatigue.

[8] The fact of crowding is as old as Ebbinghaus but it has recently been given new meaning. Cf. H. von Restorff, Ueber die Wirkung von Bereichsbildung im Spurenfeld, *Psychol. Forsch.*, 18, 1933, 299-342; and C. L. Hull, The conflicting psychologies of learning—a way out, *Psychol. Rev.*, 42, 1935, 491-516. The experiments of Von Restorff are summarized in K. Koffka, *The Principles of Gestalt Psychology,* 1935, 481-493, and W. Köhler, *The Place of Value in a World of Fact,* 1938, 256-265.

[9] Retroactive and proactive inhibition should be added to chance interference as factors producing forgetting. A common basis in crowding, retroactive inhibition and forgetting is indicated. Cf. also Von Restorff, *op. cit.* 328-342.

In one relevant respect, the present results disagree with those of Ward and Hovland. These writers are inclined to point to a very short period immediately following any learning process as being the period when one might expect to find most reminiscence. Perhaps this is because the parallel with conditioning is so alluring.[12] Since the first point on our curve represents a reproduction fully one hour after the end of the learning, and since a substantial and statistically reliable rise was found between this point and that for the 48-hr. interval, it is no longer possible to argue that the locus of reminiscence is in the immediate temporal vicinity of the learning process. To this extent, the present finding is an important corroboration of the shape of curves such as those published by Ballard.[13]

An effect closely akin to reminiscence has been described recently by

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[10] L. B. Ward, Reminiscence and rote learning, *Psychol. Monog.*, 49, 1937, (no. 4), 1-64; C. I. Hovland, Experimental studies in rote learning theory: I. Reminiscence following learning by massed and by distributed practice, *J. Exper. Psychol.*, 22, 1938, 201-224; II. Reminiscence with varying speeds of syllable presentation, *ibid.*, 338-353. Both experiments were based on predictions of Hull, *loc. cit.*

[11] This difficulty is also present in Hull's discussion where the effect of length of list on the amount of inhibition is nowhere explicitly recognized.

[12] Cf. W. M. Lepley, A theory of serial learning and forgetting based upon conditioned reflex principles, *Psychol. Rev.*, 39, 1932, 279-288. This view has led Hull to lean heavily upon a decrease in "excitatory tendency" to explain forgetting, a view at variance with the interference theory advanced by McGeoch and adopted in this paper.

[13] P. B. Ballard, Oblivescence and reminiscence, *Brit. J. Psychol. Monog. Suppl.*, 1, 1913, (no. 2), 1-82.

I. Müller in her investigation of "Reproduktionstörung."[14] In experiments arranged like those on retroactive inhibition, her results indicate a greater disturbance of recall when interpolated material is brought closer to the test. Since this disturbance is temporally associated with the recall, Müller has termed it "disturbance of reproduction" rather than retroactive inhibition, from which it would seem to differ. The parallel with reminiscence is shown in certain of her further experiments, in which the percentage of correct recall was quite consistently higher if the test followed the interpolated material not immediately, but after 12 min. The "disturbance of reproduction" behaves so much like reminiscence that, as Müller points out, parsimony would seem to demand a single explanation.

Evidence from these two earlier experiments, as well as from the present study, justifies the conclusion that the intensity of interference or inhibition produced by massing of material decreases to some extent with the passage of time. It is an effect which may take place within 2 min. after learning (Hovland and Ward), over a period of 12 min. (Müller) and over a period considerably greater than one hour. There is as yet no indication of the reasons why an initial inhibition should tend gradually to subside.

SUMMARY

(1) The forgetting curve for a single list of 8 syllables was found to fall off to 65% retention in the first hour and to continue dropping to about 50% retention at the end of 48 hr. The forgetting curve found for 3 such lists learned in quick succession, on the other hand, showed a rapid drop to 30% during the first hour, followed by a continuous *rise* to 40% at the end of 48 hr.

(2) Earlier experiments dealing with forgetting have not taken adequate account of the influence on their results of the amount of material given *S* to learn.

(3) The rise in the curve for the triple lists, it is argued, is a result of the gradual subsidence of interfering forces which are produced by the massing or crowding of items in the original learning.

(4) 'Reminiscence' will appear, according to this view, when the magnitude of this recovery is greater than the loss due to the chance interference of events in daily life which otherwise produce forgetting.

(5) Interference may continue to subside for a period of well over an hour.

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[14] I. Müller, Zur Analyse der Retentionsstörung durch Häufung, *Psychol. Forsch.*, 22, 1937, 180-210. Müller points out that her results indicate an interpretation of reminiscence in terms of interference (*op. cit.*, 208-210).

'SUBJECTIVE' COLORS FROM LINE-PATTERNS VIEWED IN POLYCHROMATIC AND MONOCHROMATIC LIGHT

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Color-effects are obtained from the stimulation of adjacent retinal fields by temporal patterns of white light as in the Benham disk and its modifications. These rotating stimulus-patterns carry alternation of black and white in various proportions.[1] The explanation of the color phenomenon is that the chromatic constituents of 'white' grow and decay at different rates.[2] The hue, saturation, and chroma of the color observed on a particular concentric ring of the disk vary also with the duration and intensity of stimulation and speed and direction of rotation.[3]

This same color phenomenon can also be obtained, although not as vividly, from the observation of stationary stimulus-patterns, as a black and white tile floor or the Luckiesh-Moss line pattern consisting of parallel diagonal black and white lines.[4] In just what way hue is a function of time in a stationary pattern is not entirely clear. Dallenbach and Erb have made a thorough study of the Luckiesh-Moss pattern and have found that the 'subjective' colors observed in the pattern are to some extent dependent upon eye movement, but eye movement, though necessary, does not appear to be a sufficient or compulsory condition.[5] At least we can venture to say that since eye movements seem necessary to the production of the color-effects, line patterns are not really stationary but in some way, not entirely clear, allow for intermittent stimulation of light on adjacent retinal fields as in rotating stimulus-patterns.

The illumination under which observations of this color phenomenon

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[1] Diagrams of the various disks used in the production of "subjective colors" are given by Erb and Dallenbach, 'Subjective' colors from line-patterns, this JOURNAL, 52, 1939, 229-230.

[2] L. T. Troland, Vision: 1. Visual phenomena and their stimulus correlations, *The Foundations of Experimental Psychology,* 1929, 200.

[3] The most thorough study of the control of variables in producing color-effects from rotating disks of light and dark patterns was made by G. A. Fry. Cf. Modulation of the optic nerve-current as a basis for color-vision, this JOURNAL, 45, 1933, 488-492; Color phenomena from adjacent retinal areas for different temporal patterns of intermittent white light, *ibid.,* 714-721; Color sensations produced by intermittent white light and the three-component theory of color-vision, *ibid.,* 46, 1934, 464-469.

[4] M. Luckiesh and F. K. Moss, A demonstrational test of vision, this JOURNAL, 45, 1933, 137 ff.

[5] M. B. Erb and K. M. Dallenbach, 'Subjective' colors from line-patterns, this JOURNAL, 52, 1939, 240-241.

have been made has usually been white light or day light. A few experimenters, however, have reported color-effects from rotating black and white stimulus-patterns in polychromatic and monochromatic light. Here, of course, it is not a question of breaking down the constituents of 'white' light, since there is no 'white' light present.

The first reference to the employment of illumination other than white light was made by Benham.[6] He had objected to Liveing's physiological interpretation of the phenomenon of perceiving different colors on the Benham disk. Liveing stated that the eye perceives colors more quickly at one end of the spectrum than at the other. Benham claimed that the color-effects were "artificial sensations" as he had observed them when his disk or top was illuminated by a bright sodium flame and that they could not be explained by physiological functioning of the eye alone. Liveing repeated the experiment of Benham and reported seeing only dark brown, pale brown and gray, no red, blue and green as in white light.[7] But he also employed as stimulus, spiral black figures on a white background in sodium light. Some observers reported a play of colors; he did not. Liveing then conceded that the phenomenon was 'subjective' and varied from individual to individual.

Abney observed the Benham disk in gas light and through colored filters giving relative monochromatic light.[8] He also made white light by mixing complements. In all cases he found some color-effects on the different concentric rings of the disk, but in monochromatic illumination the variation of colors was very slight and with some lights, such as green and violet, he saw only the concentric rings as different tints of the color of the illumination.

Turner painted the Benham pattern on a glass disk mounted on a revolving stage and rotated it in a projection lantern by means of a multiplying wheel.[9] He reported a great variety of effects if colored glass was interposed in the path of the beam. For example, with blue glass in gas light and a moderate rate of rotation, the concentric rings were red, gray, green, and blue. With monochromatic red glass the lines were blue, gray, red, and dark red. He was puzzled to find a rich blue under monochromatic red illumination, but he did not attempt to explain the phenomenon.

The most thorough work done with monochromatic light was that of Piéron.[10] He employed Wratten filters through which he observed several modifications of the Benham disk. Unequal opacity of the filters prevented a comparison of the colors in equivalent conditions from the point of view of aberration (lag) of the different colors, but by varying the speed of rotation he obtained an optimal visibility for each filter. With R and O filters Piéron obtained very good colors. With B and G filters, the colors were poor. The colors varied with the speed of rotation as in white light. Piéron questions whether the mechanism is the same that produces the color-effects in white light. He suggests that most likely all the spectral regions are susceptible of acting on all fundamental chromatic processes, but with unequal intensity (power).[11]

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[6] Benham, The artificial spectrum top, *Nature,* 51, 1894, 200.

[7] Liveing, *ibid.,* 200.

[8] W. de W. Abney, The artificial spectrum top, *ibid.,* 292.

[9] Dawson Turner, The spectrum top, *ibid.,* 439.

[10] Henri Piéron, Le mécanisme d'apparition des couleurs subjectives de Fechner-Benham, *Année psychol.,* 23, 1922, 1-49.

The phenomenon of flight of colors after exposure to white light would seem to be related in some way to the phenomenon of 'subjective' colors, although the typical flight of colors including those other than the positive and negative after images appears only after longer and more intense stimulation than the color-effects from the more immediate and weaker stimulation of black and white patterns. Flight of colors has also been observed as a result of monochromatic (sodium-yellow in the arc spectrum) stimulation of the retina.[12] No explanation, however, was offered for this phenomenon.

No work has been done in observing a stationary black and white pattern in monochromatic or polychromatic illumination other than daylight. Dallenbach and Erb made a thorough study of the subjective colors from the Luckiesh-Moss line pattern under various conditions, but in artificial daylight illumination only.[13] They found, in the first place, that the pattern of narrow black lines close together on a white background gives rise, when regarded steadily for short periods of time, to the perception of pastel colors, usually of low saturation and light tint. The lines were predominantly yellow and blue, but covered the entire spectral range. These colors appeared to both naïve and sophisticated *O*s. The colors were more pronounced when fixation was not in the plane of the figure. They modified the width of the diagonal black lines and the intervening white spaces, and they varied the color of the lines and background (*i.e.* blue lines on white, etc.), but no condition was as effective as the original black-white pattern. The *O*s all reported some kind of phenomenal movement across or on or between the black lines.

Our problem was to supplement the work of Dallenbach and Erb by determining the effect of observing the original line pattern of Luckiesh and Moss in colored light, both polychromatic and monochromatic.

Procedure. Our general procedure was the same as in the experiment of Dallenbach and Erb.[14] The stimulus-figure (see Fig. 1) was a pattern of diagonal lines about 0.5 mm. wide and about 1 mm. apart, covering a rectangle 9 x 6.5 cm. and printed in dead black on egg-shell white paper. This pattern was presented to the *O*s for 1 min. by means of a simple exposure-apparatus. The following colored lights were employed:

(1) Polychromatic:
  (a) Tungsten lamp in which all colors are emitted in varying proportions, the preponderance being at the red end of the spectrum.

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[11] *Ibid.*, 36.

[12] A. Ford, Duration of stimulation and the flight of after-images from sodium-yellow in the arc spectrum. *Psychol. Bull.*, 28, 1931, 709.

[13] *Op. cit.*, 227-241.

[14] *Ibid.*, 234.

(b) Quartz mercury arc lamp that contains very little red, fairly bright Y-O line (577 and 579 mμ), bright Y-G (546 mμ), faint B-G (492 mμ), strong B (436 mμ) and violet low in visibility (405 mμ).

(2) Monochromatic:

(a) Yellow light. Filter: amber OG-3 used with illumination by quartz mercury arc lamp. Radiations almost exclusively 577 and 579 mμ and a very little red at 623 and 691 mμ.

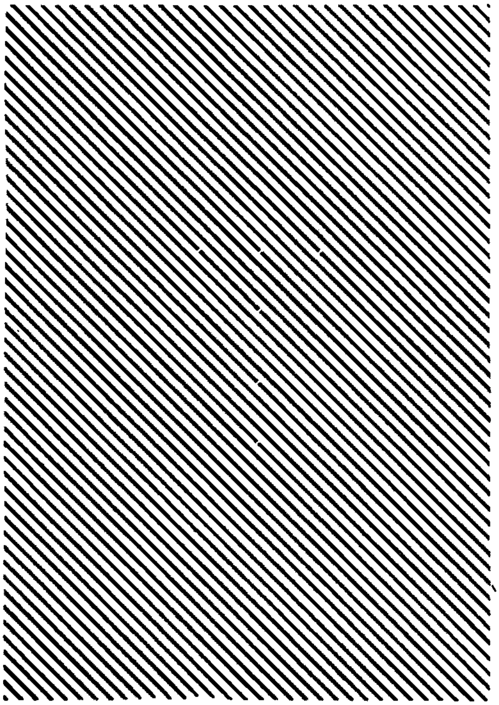

FIG. 1. THE STIMULUS-PATTERN

(b) Green light. Filter: liquid in a circular parallel sided quartz cell with illumination by quartz mercury arc lamp. Radiation at 546 mμ with a minute amount of 577 and 579 mμ.

(c) Red light. Filter: H-R traffic red with illumination by Tungsten light. Transmits light at 623 and 691 mμ.

The filters were placed in a pasteboard eyepiece so that the stimulus-card was observed monocularly with the right eye. The intensity of light reaching the eye was kept constant for all the light sources. The necessary intensity of light falling on the stimulus-card, in order to allow for this constant intensity of light reaching the eye (with or without the filters) was determined in advance.[15] Adjustments were then made by varying the distance of the course of light that was placed behind, but slightly above O's head, and by reading the intensity in terms of foot-candle power from a sight meter placed on the stimulus-card. O's eyes were always at a distance of approximately 14 in. from the center of the stimulus-pattern when exposed.

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[15] The foot-candle power falling on stimulus-card necessary to produce an equal intensity of light penetrating the eye: mercury arc plain, 4.1; amber filter (yellow),

Five *O*s served under these five experimental conditions. Two of these *O*s had observed in Dallenbach and Erb's experiment. Besides these 5 *O*'s, two others served in certain parts of the experiment—one with the Tungsten lamp and the red filter, the other with the mercury arc lamp and the yellow and green filters. Six of our *O*s were trained psychologists and one was a physicist.

Every *O* was first told to look at the stimulus-pattern which was to be exposed directly in front of him and describe what he observed. Secondly, he was told to look at the stimulus-pattern with far-fixation and do the same thing. Each *O* observed the stimulus-card once under the different conditions of illumination. The order of presentation of the different light conditions was: mercury arc plain; yellow filter; green filter; tungsten plain; red filter. The experiment was carried on during the summer of 1934 in the physical and psychological laboratories of Cornell University.

Results. As in the case of daylight illumination, both 'subjective' colors and phenomenal movement were reported by all *O*s at some time during the exposure situations. The main results can be classified in the following manner: (1) Differentiation among the black diagonal lines both of hue and of brightness (astygmatic effect). Two *O*s stated that this phenomenon occurred particularly when the eyes assumed the position for far-fixation. (2) Color in the interstices between the black lines, either filling in all the space between the lines or merely along the edges of the lines. (3) Patterned lines (figures or designs, sometimes beaded, sometimes streaming, in motion) diagonally across the parallel black lines. Sometimes these patterns were black or gray, but more often colored, pastel shades. (4) When color phenomena were reported, continual change of the color was observed, generally in the direction of an increase in saturation.

Not all the *O*s reported the phenomena above, nor did any one *O* always report all these phenomena at a given time, although this did occur.

Under all conditions, except monochromatic red, many color phenomena were reported: With the polychromatic illumination, the colors were generally more vivid and saturated than in the monochromatic light. Even in the yellow and green monochromatic lights, however, color phenomena were observed that were not limited to the color of the light source nor to its negative after-image. For instance, in the yellow light, red, green, dull orange, reddish yellow, yellowish green and blue (purple is the nega-

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12.2; liquid filter (green), 47; tungsten plain, 3.5; red traffic filter, 27.3. My thanks are due Dr. Roger Estey for providing the filters and making the photometric determinations.

tive after-image of this yellow) were observed at different times. With the green light, yellow, bluish green, red, brown and blue (violet is the negative after-image of this green) were observed.

With the red light, however, only various shades of red (one instance of orange) were observed. The streaming phenomenon across the diagonal black lines was reported, but this was either a gray pattern or reddish.

Unfortunately, all the observations in this experiment had to be carried on under conditions of low illumination because of the low intensity of light capable of penetrating the filters, particularly the red filter. Consequently, the intensity of illumination was in no way comparable to that in the experiment with the daylight illumination. It will be a matter for further investigation to find out the influence of the intensity of the illumination on the nature of the color phenomena observed.[16]

Summary. We may summarize the results of this study as follows:

(1) The same phenomena were observed in colored light sources as in the previous experiment when daylight illumination was employed.

(2) The color phenomena reported in polychromatic illumination are apparently analogous to the phenomena reported by other experimenters who have employed temporal patterns of intermittent stimulation of 'white' light upon adjacent retinal fields.[17]

(3) The color-effects reported in the present experiment under monochromatic illumination need further explanation than that of aberration of polychromatic light in a temporal sequence, since no polychromatic light is or was present. The tri-receptor theory of Young-Helmholtz states that a given receptor in the retina is not only sensitive to a given wave-length but is also sensitive to the other wave-lengths of the spectrum, but to a less degree.[18] Consequently, it is conceivable that under certain conditions not well understood of intermittent stimulation by temporal and spatial patterns, in monochromatic light, the equilibrium of the receptors (cones) is broken down in some way beyond that of the complementary balance so that they respond as though stimulated by different wave-lengths.

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[16] With illumination of greater intensity various color phenomena were reported with the red filter.

[17] The 'white,' of course, is not always observed as white but as colored by the predominant hue in the polychromatic illumination, *i.e.* red with illumination by the Tungsten lamp.

[18] S. Hecht, The retinal processes concerned with visual acuity and color vision, *Bull. No. 4, Howe Laboratory of Ophthalmology, Harvard Medical School,* 1931, 67-80.

NEW METHODS OF OBTAINING NEUROTIC BEHAVIOR IN RATS

By GEORGE HUMPHREY and FRED MARCUSE, Queen's University

In a recent monograph,[1] Maier described certain disordered activities that he obtained in the rat. We have succeeded in duplicating his results by techniques entirely different from his. Maier used Lashley's jumping-board technique, according to which the rat jumps from a platform at one of two patterns, behind one of which is food. Acutely disordered behavior ("neurotic") was produced when one pattern was removed, leaving the animal no choice but at the same time forcing it to jump by turning a jet of air upon it.

We have used a new series of stimuli, graded in severity, partly on normal rats, partly on a small group of animals in which chronically disordered behavior has been induced by a method to be described. The method apparently permits of differentiation between these two groups, and clearly contrasts what may be called a chronic and a traumatic stage of abnormal behavior.

In order to induce chronically disordered behavior, 10 rats were trained by daily runs for 25 days in a Warner-Warden multiple Y-maze, set up in the type left, right, left, right, foodbox. The foodbox had no bottom so that the rat and its food were in direct contact with the floor of the room. With 6 of the animals the foodbox was moved along the floor, after the animal was in the box and the door closed. Movement was carefully effected so as not to cause pain; the extent of movement varied from 4 to 10 ft., with no appreciable effect on immediate behavior, except that the animals did not eat until movement ceased. The relation of this movement to the animal was something like that of a revolving door, which is being pushed by someone else, to a pedestrian. The remaining 4 rats were trained in the ordinary way, with a stationary foodbox.

Results. (a) Chronically disordered behavior. The 6 rats whose foodboxes were moved were consistently above the 4 unmoved controls in their scores, both for time and error. The 4 controls satisfied the criterion for learning; namely, 5 errorless runs, within 16 days, but not one of the 6 experimental rats did so within 25 days. The curves for the experimental animals are strikingly more irregular than those of the controls, both in time and errors. At the same time there appeared in the experi-

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* Accepted for publication August 10, 1939.

[1] N. R. F. Maier, *Studies of Abnormal Behavior in the Rat,* 1939, 1-81.

mental animals the following activities which are significant of disorder. (1) "Belly-crawling"—*i.e.* crawling upon the belly through part of the maze or when finally moving into the foodbox—was observed in the behavior of all 6 experimental animals and on several occasions by each of them. (2) Loud gnashing of the teeth was frequently heard—louder than the chattering that these animals ordinarily make. (3) Spasmodic starts from one side to the other of a cul-de-sac were common; as was also (4) shivering. There was (5) frequent biting of the door of the foodbox and also at the metal mesh at the top of the culs-de-sac. The rats (6) frequently moved hesitatingly back and forth a few inches at a time when they arrived at the entrance to the foodbox. When half way into the foodbox, they would frequently (7) withdraw—sometimes completely retracing the maze (with or without errors) and then return again to the foodbox (with or without errors). All these actions were observed in all the rats during the 25 days of their training.[2] None of them was observed in any of the control animals.

Further analysis showed that while the experimental animals never learned the maze in the sense that they achieved errorless runs into the foodbox, 5 of the 6 learned to run *as far as the door* of the foodbox without error. In that they achieved the criterion of 5 errorless runs by times varying from the 12th to the 27th day. That is to say, these animals ran to the door of the foodbox, probably to within sight of the food, without a mistake—and did that on 5 successive trials. They then retraced their steps making mistakes, behaving abnormally and so on, spending in this way the rest of the time before they entered the foodbox. The sixth experimental animal did not even learn to reach the door of the foodbox without error during the 25 trials.

It is hard not to use anthropomorphic terms in describing these animals. The most natural description seems to be that the animals *knew the way to the foodbox but were prevented from entering it by some conflicting motive.* In terms lately made fashionable by Lewin, one may say that there was apparently present a cognitive structure corresponding to the maze, with conflicting valencies at the foodbox. The conflict is evidently one which is very difficult, perhaps impossible for the animal to resolve.[3] It apparently induces a condition of 'nervous strain' which results in the chronically disordered activity above described. It may be mentioned that 85 rats were handled during the experiment by the junior author, who was bitten 4 times in all, each time by one of the chronically disordered animals. Total daily errors of a separate group of 4 animals trained similarly were temporarily reduced, by 2 cc. of a 10% solution of alcohol, from 9 to 0, and the average running-time was reduced from 22 sec. to 7 sec. These effects occurred the day after the alcohol was given them—the 38th day of the experiment. A similar result was obtained after a second dose of alcohol—given 5 days later. This finding is reported as suggestive rather than as scientifically conclusive.

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[2] No attempt was made to make a quantitative record of such behavior. The records show teeth chattering, 22 observations; belly crawling, 20; spasmodic starts, 16; shivering, 15; biting at door, 14. Since more than one person was engaged in training the rats, which in the case of the abnormal animals is a very protracted business, and since no scheme of annotation was adopted until late in the experiment, these figures are suggestive rather than exact.

[3] Except by some special device, such as was adopted by one rat who made a practice of snatching out the food before the door of the foodbox could be closed.

(*b*) *Acutely disordered behavior.* Maier reports highly excitable behavior in his rats, followed by a plastic phase, in which the animal could be taken in the hand lying on its back, and its limbs 'moulded' by the experimenter. We have obtained these effects by a procedure different from his. Stimuli of three grades were used. S_1 consisted of ringing an electric bell (2 dry cells) for 3 min. on the *floor* of the carton on the upper surface of which the animal was placed. S_2 consisted of ringing the bell for 3 min. close up to the cage, and on the same plane with it. S_3 consisted in swinging the cage gently 5 or 6 times with an oscillation of 180° while the bell was ringing in the position of S_2. Thus S_1, S_2, and S_3 were of progressive degrees of 'severity.' Experiments were mainly confined to S_1 and S_3, though seizures were obtained by all three methods. Table I shows the results obtained at the first trial of the stimulus in question. S_2 was not used at a first trial.

TABLE I

NUMBER OF SEIZURES OBTAINED AT FIRST TRIAL OF THE VARIOUS STIMULUS-GRADES

Rats	Stimulus-grade	No. males	No. females	No. seizures
Normal	S_1	24	—	0
	S_1	—	26	1
	S_3	23	—	11
Chron. dis.	S_1	6	—	3

It will be seen that while S_3 induces seizures in nearly 50% of normal male rats, S_1 has no effect on 24 males and induces a seizure in only 1 of 26 females. On the other hand, S_1 induces seizures in 3 (*i.e.* 50%) of 6 chronically disordered males.

This was a rational solution of the problem; the animal obtained the food without being moved. The records show that this animal's performance improved in time and errors after he had found this solution and before means were taken to bar it.

(c) Nature of seizure. The seizure as observed by us consists essentially in very violent motor activity. The animal begins by violent jumping round the cage, from one to another of the four walls, hardly pausing between each jump. Sometimes he appears to wound his nose during this behavior. When the cage door is opened, he leaps out on to the floor, rushes madly round and round in circles, or straight across the room, bumping headlong into chair legs and walls, and at times making great leaps across the floor. There is sometimes a peculiar noise, between a grunt and a squeak. Sometimes the rat leaps up directly into the air. One stunned itself for over a minute. Functional paralysis of the front legs often developed during seizure. In all, 40 seizures were observed. Of these, 28 were obtained from 3 rats; and of these 28, 6 were produced by S_1, 3 by S_2, and the rest (19) by S_3. All three of these disordered rats became adapted to S_1 and S_2 after two or three stimulations, and thereafter S_3 was used.

(d) Plastic phase. As observed by us, the plastic phase consists in complete inactivity immediately succeeding the excited phase. The animal can be taken in the hand, laid on its back and its legs stretched out or moulded,to use Maier's term. The feet and tail are bluish, the nose red, possibly owing to wounding in the cage. The hind toes are often rigidly outstretched, in contrast to the closed toes of the front feet. There is loss of the reflex to tickling the feet and in three cases there was loss of the blink reflex to a puff of air. A sharp pinch of the tail or pulling the vibrissae has no effect. The neck is drawn up in a peculiarly stiff posture.

Summary. Acute neurotic behavior was produced on 40 occasions in 15 rats, by a graded series of stimuli. The least 'severe' stimulus, produced seizures in 3 of 6 chronically disordered male rats, had no effect on 24 normal males, and produced a seizure in one of 26 normal females. The most 'severe' stimulus produced seizures in 11 of 23 normal males. The rats were throughout in good physical condition, as far as could be judged. Experiments are now proceeding to effect a standardization of the stimuli and, since the number of the chronically disordered animals is admittedly small, to confirm the results on a larger scale.

APPARATUS

THE NEUROVOLTMETER

By Edmund Jacobson, Chicago, Illinois

One of the greatest needs today in clinical as well as in research medicine is a convenient method and instrument for the measurement of nervous and of muscular states in man. With such an instrument the worker can enter fields new in the sense that they have seen much speculation but relatively little that can be measured in the clinic or in the laboratory. What happens in an individual when he is nervous or tense is obviously of great importance to the general practitioner as well as to the specialist. The complexities are so great, however, that little more than a beginning has been made. Workers interested in problems of nervousness may be tempted to begin with a definition; but the route here preferred is to secure measured data which will lead toward a definition.

In previous articles, I have shown that it is possible to measure what is clinically called 'nervousness' in intact man. This is accomplished without the use of an anesthetic, but also without noteworthy discomfort or psychic disturbance. Measurements of action-potentials are made in muscles or in nerves as desired. The *Ss* have included normal persons, but also patients suffering from various disorders, including neurotic or so-called 'nervous' or 'hypertensive' individuals. I have described methods whereby action-potentials from one or more muscles or nerves can be photographically recorded over a considerable period of time such as an hour.[1] The results can be presented in various ways; namely, as the photographs themselves, or as a curve in which action-potentials are plotted against time. A third manner of expressing the results is to average the peak microvoltage per unit of time (1/5 sec.) for the desired period of test and to express the result as a single number. For example, an individual who, during a given hour of test, showed an average of 5 to 10 microvolts certainly was very tense or nervous throughout much or all of the period. On the other hand, if the number was about one microvolt or under, the average was not far from that secured from a group of unselected or so called 'normal' individuals.

For these purposes, either a string galvanometer or an oscillograph with a suitable amplifier assembly can be employed. I have preferred the string galvanometer. It is necessary to have the apparatus sufficiently stable so that the recording string or wire vibrates very little when a short circuit exists across the input of the amplifier. Indeed, the vibrations of the string should not at the most exceed 2 or 3 mm. in either direction. What is required, however, with this degree of stability is that the string should show a definite excursion from the zero axis of at least several millimeters per microvolt p.d. impressed upon the input terminals of the amplifier.

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* From the Laboratory of Clinical Physiology. Read before the American Association for the Advancement of Science, Section I, Psychology, June 22, 1937.

[1] Edmund Jacobson, Electrical measurements concerning muscular contraction (tonus) and the cultivation of relaxation in man; studies on arm flexors, *Amer. J. Physiol.*, 107, 1934, 230-248; Electrical measurements concerning muscular contraction (tonus) and the cultivation of relaxation in man: relaxation-times of individuals, *ibid.*, 108, 1934, 573-580; Electrical measurement of activities in nerve and muscle, in *The Problem of Mental Disorder*, ed. by M. Bentley and E. V. Cowdry, 1934, 133-145; Measurement of the action-potentials in the peripheral nerves of man without anesthetic, *Proc. Soc. Exper. Biol. & Med.*, 30, 1933, 713-715; The course of relaxation in muscles of athletes, this Journal, 48, 1936, 98-108.

With such methods of recording it is a long and arduous task to secure a graph representing the variations of the action-potentials from a nerve or muscle during the period of an hour or so. Evidently it would be useful if we could secure some instrument which would simplify this task by integrating the voltages at a particular instant. In Fig. 1, is a photograph of the instrument designed to perform this sort of integration. This has been worked out with the collaboration of the Bell Telephone Laboratories including the help of Messrs. H. A. Frederick, D. G. Blattner, and H. M. Stoller, to whom I wish to acknowledge great indebtedness.

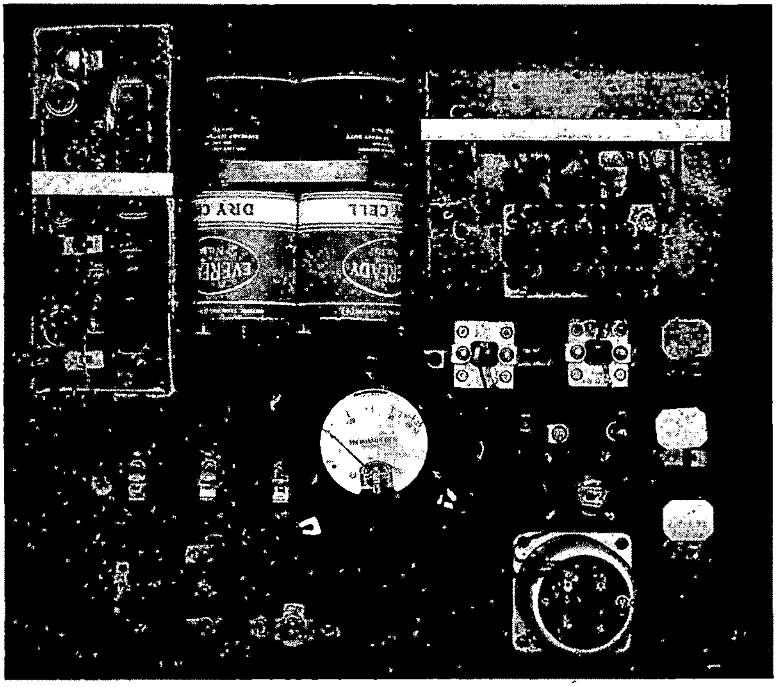


FIG. 1. THE NEUROVOLTMETER

The method for the use of this instrument is extremely simple and convenient. Fine platinum-iridium wires with very sharp points are inserted into whatever muscle tissue or nerve is selected for measurement purposes. These wires are protected with insulating tape so that there is no exposure of platinum to air. Both the wires and the skin are previously sterilized with alcohol. Because the wires are very fine as well as sharpened, they are readily inserted with no marked discomfort and the patient does not as a rule complain about their presence. Surface electrodes can be used, if preferred, thereby avoiding penetrating the skin. Leads connect the wires to the input of an amplifier. The amplifiers I employ most commonly show a fairly flat characteristic from about thirty to four thousand. The amplifier is connected to the input of the instrument which is to be seen in Fig. 1. A diagram of the circuit is shown in Fig. 2.

It is necessary to be able to vary the sensitivity of the meter and this can be done very readily by means of a variable shunt placed across the meter or across the entire circuit. If the patient is relatively calm and relaxed no shunt at all will be required; but if the patient is very excitable or tense the needle will at once shoot up to the highest point unless a shunt is used.

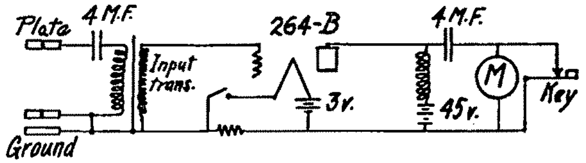


Fig. 2. Circuit Diagram of the Neurovoltmeter

The input terminals, plate and ground, are connected with the output terminals of the amplifier described previously, which delivers an open circuit voltage on the order of 7 millivolts. The input transformer has an approximately 15:1 step-up to the grid of a 246B vacuum tube. This tube operates on 0.3 ampere at 1.5 volts on the filament and 45 volts, 2 milliamperes in the plate circuit. The microammeter is connected in series with a 4 microfarad blocking condenser across the choke coil in the plate circuit. A strap key is provided which normally short-circuits this meter to protect it from transient disturbances. With 7 millivolts applied to the input terminals of the amplifier the meter reads approximately 20 microamperes. This makes it possible to cover a range of input voltages from 2 to 25 millivolts impressed upon the amplifier. By means of an adjustable shunt (1-10000 ohms) on the output of this amplifier, the necessary range for all measurements of nerve and muscle potentials can be covered.

The actual procedure in making a measurement is simply to insert the electrodes in a patient who may be lying down or sitting up as desired, press the key and read the meter. The preparations require but a few minutes and the meter affords instantaneous readings.

Meter readings secured with any particular shunt resistance can be standardized readily. An essential step is to read the meter when a short circuit exists across the input of the amplifier. For example, with a shunt of 3000 ohms, the meter reading is 2. When 1 microvolt is impressed the reading is 6, and when 10 microvolts are impressed the reading is 68. There is a little discrepancy here, showing that a little more work will need to be done to correct this point. We find, however, that the needle deflection is approximately 7 scale-divisions on the micro-ammeter per microvolt.

Seven scale-divisions is a large deflection for only one millionth of a volt. To effect this deflection, as you will have noted from the circuit diagram, the potentials from the electrodes in the nerve or muscle tissue are rectified. In other words we are securing a power reading and this should prove very useful for many research purposes as well as for divers needs in medical practice.

Fig. 3 illustrates how the instrument can be standardized for photography of the deflections of the needle. It shows how far, in millimeters, the needle moves for one microvolt (Fig. 3a) and for 10 microvolts (Fig. 3b) and it shows how tightly the string is set (Fig. 3c). Photographs can be made if the meter is removed from the cabinet and placed in the path of the rays from the lamp of the string galvanometer. A small projecting rod, such as a broomstraw, is fastened perpendicularly to the tip of the needle in order to record the position of the needle on the same photograph as shows the string shadow. This changes the natural period somewhat, but we can ignore this change, since it is not designed for common use but only to permit us here to illustrate some of the characteristics of this meter.

For muscular studies in man, it is convenient to insert one electrode (positive) in the right biceps muscle near the midline (but below the "nervous equator" of Hermann) and the other under the skin in the olecranon fossa. This has been done in preparation for Fig. 4, which illustrates complete relaxation in the forearm flexors as shown by the string galvanometer as well as by the position of this needle. Here, as may be observed, the needle follows a quiet course corresponding with that of the string.

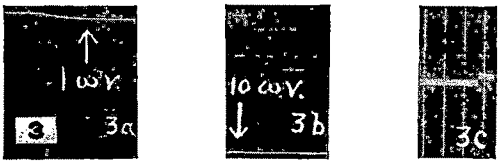


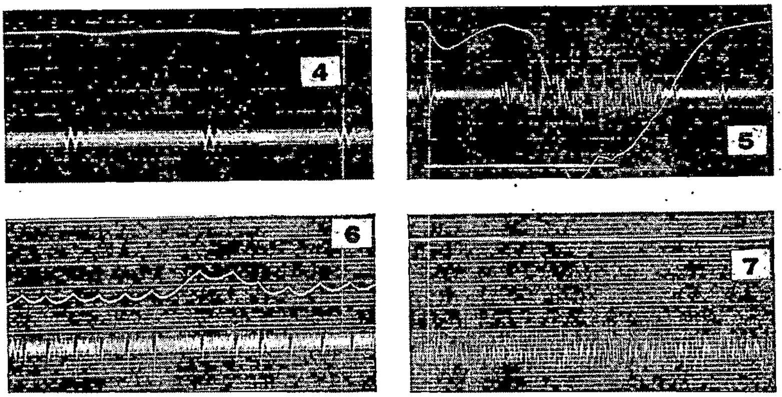
FIG. 3. RESPONSE OF NEUROVOLTMETER

(a) To 1 microvolt, 57 cycles. Position of the needle indicated by arrow corresponding with meter reading 2 microamperes. (b) Arrow indicates position of needle responding to 10 microvolts. Meter reading 68 micro-amperes. (c) Speed of photographic paper, distance between vertical time lines = 0.2 sec. String tension, approximately 3 millivolts per centimeter. These features apply to Figs. 4, 5, 6, and 7.

As will be recalled from studies previously published from this laboratory, mental activity itself can now be measured, in so far as its occurrence characteristically includes activity in peripheral nerves or muscles. Fig. 5 illustrates what commonly happens when *S* merely imagines lifting a 10 lb. weight in his right hand. As will be seen, the deflections of the string become very marked while the excursion of the needle evidently integrates the same potentials. To be sure, the period of the needle is relatively slow: there is a considerable lag before the needle returns to zero, following the recurrence of complete relaxation.

A record from the same muscles in a patient who is somewhat tense is shown in Fig. 6. Several years after a skull fracture sustained by compression from a folding bed, she continued to be in a somewhat over-emotional or 'nervous' state, although her outward demeanor appeared calm. Here, although she is trying to be as quiet as possible, the string shows marked fluctuations continuously and irregularly and the meter reading is continually up in the neighborhood of 4 microvolts much of the time. Another illustration from the same patient is shown in Fig. 7. At a later date, when this patient had become less nervous, the readings are more like those shown in Fig. 4.

As stated previously, the new instrument should have many uses for the medical practitioner as well as for the research student. Therewith he can determine pre-



Figs. 4-7. Sample Records

Fig. 4. Complete relaxation in a muscle as shown by the string galvanometer (lower tracing) as well as by the position of the needle (upper tracing). Voltage sensitivity of the string galvanometer assembly approximately 7 millimeters per microvolt, A.C. in this and in each of the figures following. Read all figures left to right. In all records appearing in this article, *S* is lying quietly, with eyelids closed.

Fig. 5. Action-potentials in muscles which flex the forearm when *S* merely imagines lifting a 10 lb. weight in his right hand. Signal line at the bottom indicates clicks of telegraph key; the first, at the beginning of the line, indicates to *S* that he is to imagine lifting a weight; the second, at the end of the line, that he is to relax any tensions present. Electrodes are inserted in the right biceps muscle region. The middle tracing, made by the string galvanometer, indicates action-potentials from the right forearm flexors (as in lifting), beginning about one second after the signal to imagine and ceasing soon after the signal to relax. The remaining tracing, which begins on the left near the top, is made with the neurovoltmeter, using a shunt resistance of 3000 ohms. As will be seen, it greatly resembles the form of a mechanical record of muscular contraction. (Over the beginning of the signal line a brief excursion appears in each tracing, which is to be ignored, since it is due to an inductive effect upon closure of the signal circuit.)

Fig. 6. Record from the right biceps-brachial muscles in a somewhat tense or 'nervous' patient. Note the action-potentials shown in the lower tracing (string galvanometer), including the recurrent firing in one group, overshooting the others in amplitude. Note also the parallelism in the middle tracing (neurovoltmeter). No shunt across the neurovoltmeter. Scale reading, 13 micro-amperes per microvolt, A.C. The short-circuit line (zero reading) lies about 6 mm. from the top of this record.

Fig. 7. Another sample from the same patient. Note the action-potentials, continuously varying around about 2-3 microvolts (lower tracing: from the string galvanometer), while a parallel recording of the same muscle tension appears above from the neurovoltmeter, which is here set at a somewhat less voltage sensitivity than in Fig. 6. The short-circuit line (zero reading) lies about 1.5 mm. above the upper portions of the neurovoltmeter tracing.

cisely how nervous or tense his patient is at a particular time when he wishes to make a diagnosis. He can test also the effects of any form of treatment which he has chosen to employ so far as they are shown in the nervous or muscular system. In these various ways the instrument may prove of service for treatment as well as for diagnosis.

SUPPLEMENTARY EQUIPMENT USEFUL IN RESEARCH ON EYE MOVEMENT

By JAMES S. KARSLAKE and JOSEPH TIFFIN, Purdue University

Three pieces of equipment, recently developed in connection with research in eye movement, that are also of interest to the general laboratory technician, are as follows: *(1)* exposure-device; *(2)* tachistoscope with remote control switch; and *(3)* writing table. These are illustrated in Fig. 1. This equipment was developed from the need for an easy and effective means of presenting advertising copy to the reader, in connection with investigations on the attention-value of advertising.

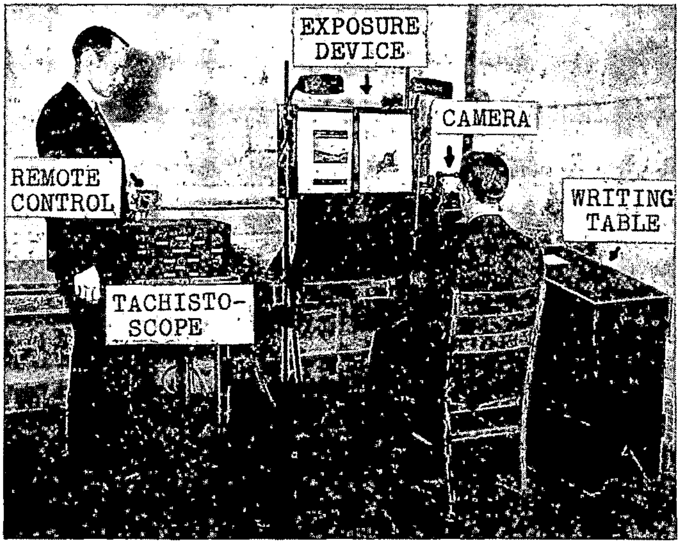


FIG. 1. SUPPLEMENTARY EQUIPMENT FOR RESEARCH IN EYE MOVEMENT

The exposure-device has been employed for the presentation of a series of advertisements, and when used in connection with a tachistoscope (time-interval control device), such factors in the study of advertising, as, what sort of material first attracts the attention, and what material attracts the most attention, may be investigated. For example, one may undertake to arrange a number of advertisements in their rank order of merit, from most attractive to least attractive. Such a study may be made by pairing each in the series of ads with every other ad in the series, and exposing each pair to the attention of the reader for a definite interval of time. Once such a series has been so constructed, it may be used as a basis for determining the attention-value of any other ad it may be of interest to investigate. To evaluate a new ad on this basis, one need simply determine which ad in the standardized series a number of observers find to be equally attractive.

To prepare advertising copy for use, the ads are mounted on a series of rectangular cardboards and stacked in the order in which they are to be presented. The cards are then hung from the top, on a pin, and will be seen in the proper order as they are fed through the exposure device. The cards are fed through by pressing a plunger on the back of the panel, causing the front card, or pair of cards, to drop into the box below, and presenting the second card, or pair of cards, to the attention of the reader. If the room is darkened between one presentation and the next, and if the reading lights are controlled by the tachistoscope, each successive pair may be exposed for a predetermined interval of time.

A photographic record of the eye movements of the reader, taken while he is observing the material as it has been presented above, is considered to be objective evidence on the attention-value of whatever copy is under investigation. On the basis of such an eye movement record, this series of ads may be arranged in their order of merit. In making any such photographic record, both lights and camera are controlled by the tachistoscope. While the master switch on the panel of the tachistoscope is used to control both reading lights and camera, the tachistoscope, in reality, controls the length of time both lights and camera are on; the operator merely determines when the interval is to begin, and does so by closing the master switch. The tachistoscope darkens the room and stops the camera after a definite interval of time has elapsed. The range of time intervals available on our tachistoscope is from 0.1 to 15 sec., and the interval to be used is a matter of choice. After any given interval has been chosen by an appropriate setting of the time-control switches, the time-interval during which lights and camera are on may be repeated time after time, wholly by means of the master switch. This master switch on the panel of the tachistoscope, is in parallel with a remote control switch, the use of which allows the experimenter considerable freedom of movement during an experiment, independently of wherever the tachistoscope may have been placed in relation to the other equipment.

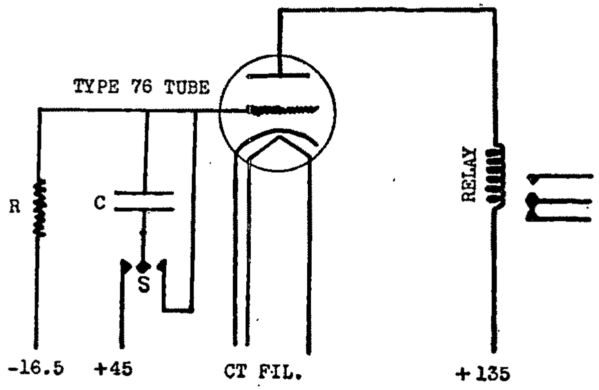


FIG. 2. DIAGRAM OF THE FUNDAMENTAL CIRCUIT OF THE TACHISTOSCOPE

The writing table is for the convenience of the reader whenever he may be required to make any sort of written response in connection with the material he is reading, as, for example, in studies on problem solving. This table has been so constructed as to change the paper at the press of a button placed at the foot of the reader, permitting him to make a change in the paper without diverting his attention from the problem at hand. Here, too, the tachistoscope has been so arranged as to control the change in paper.

For the information of those who may be interested in constructing similar equipment, a more thorough explanation follows:

(1) Exposure device. The cards, on which the advertisements are mounted that are to be presented for a study of their comparative attention-values, are hung in the device on a pin inserted through eyelets placed in the top edge of every one. Blank cards hung in front of the first pair prevent the reader from seeing any copy until the experimenter is ready to begin. After darkening the room, a plunger at the back of the device is used to release the two blank cards in front, which then drop through a chute into the box below. The first pair of ads are then in a position to be seen for an interval of time controlled by the experimenter. By means of either the master or remote control switch, the tachistoscope is used to start the camera, flash the lights on for a definite time interval, flash them off, and stop the camera. The experimenter makes ready to present the next pair of ads to the reader, while the room is dark, by dropping the front pair into the box below, and resetting the tachistoscope.

Only one card at a time is released from the front end of each pin because of the small clearance between the pin ends and a stop extending across the front top edge of the cards. As the pins are adjustable in length, this clearance is just sufficient to release the cards one at a time when the plunger at the back of the panel is pressed forward. To be effective, the cards must be of uniform thickness, smooth and flat.

(2) Tachistoscope with remote control switch. Basically the operation of the tachistoscope (see Fig. 2)[1] depends upon reducing, for a short interval of time, the negative bias on the grid of a three-electrode vacuum tube, during which time-interval, the flow of plate current actuates a relay, temporarily completing an external circuit. The length of time during which the external circuit is closed depends upon how long the grid bias is insufficient to overcome the flow of plate current through the tube. This interval of time depends upon the size of condensor (C) and leakage resistor (R) in the grid circuit. By means of a single-pole, double-throw switch (S), a positive charge (45 volts) placed on one plate of condensor (C) attracts to the other plate, a negative charge which must be withdrawn from the grid, allowing plate current to flow until a sufficient charge of electrons from the power supply can flow through the leakage resistor (R) to re-establish the negative bias on the grid. The choice of time intervals, then, is merely a matter of selecting the proper size of resistor (R) and condensor (C) to be used in the grid circuit of the vacuum tube. Tap switches for the purpose of choosing any combination of a number of resistors and condensors, then, will serve to select any timed-interval within the range of values available. To repeat any given timed-interval, switch (S) is used to reset the circuit, by short-circuiting the condensor.

Based on the above principle, a multiple tachistoscope (see circuit diagram;

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[1] Acknowledgement is made to Mr. Paul Griffith of the State University of Iowa for suggesting this fundamental circuit.

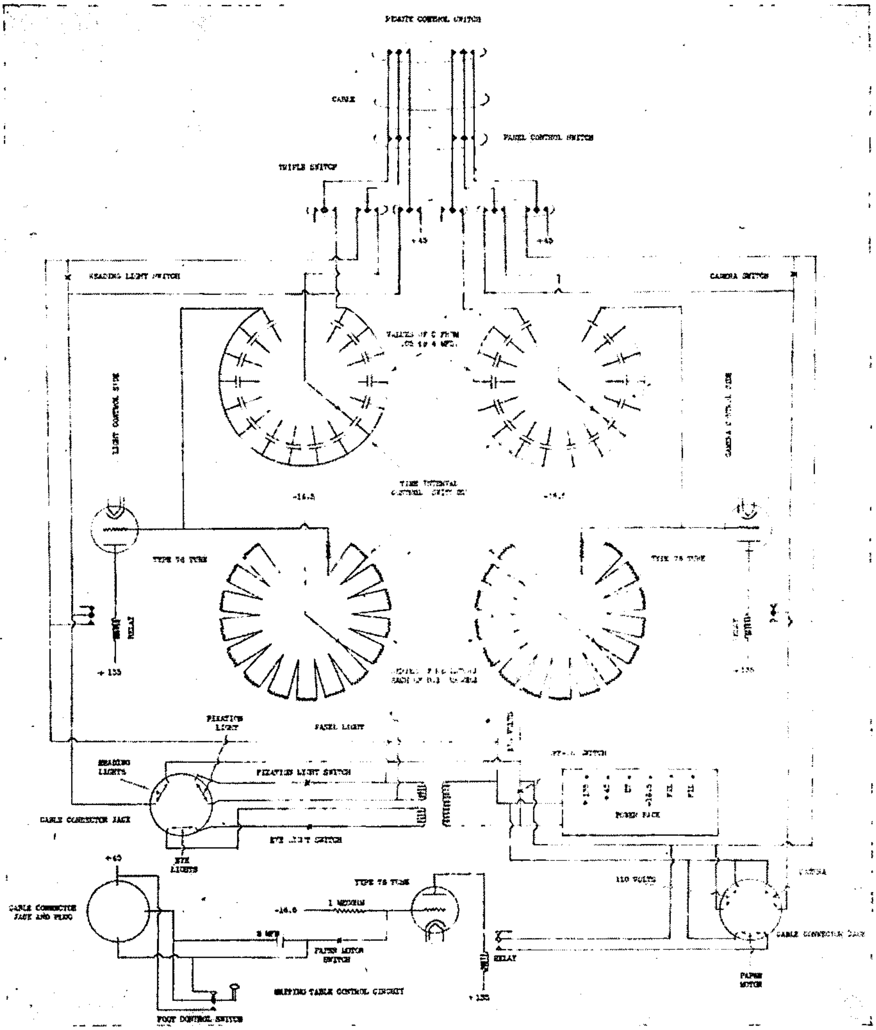

FIG. 3. CIRCUIT DIAGRAM OF THE ELECTRONIC DEVICE FOR TIMING THE OPERATION OF CAMERA, READING LIGHTS, AND WRITING TABLE

Fig. 3) was constructed for simultaneously controlling both the camera and reading lights. In this tachistoscope is a central switchboard consisting of a triple switch, actually three double-pole, double-throw switches. The combination of elements, and the way in which they are to be arranged for single-switch control, is determined by the position of these three double-pole, double-throw switches. Single-switch control is obtained with either one of the two switches marked panel control switch or remote control switch on the circuit diagram. One or the other of these master switches enables the operator to control any combination of lights and camera from anywhere within a distance of 15 feet, either tachistoscopically or non-tachistoscopically, at will, and to reset the tachistoscope for repeating the timed-interval. The range of timed-intervals with the values of R and C shown on the circuit diagram is, by steps, from 0.1 to 15 sec.

Incorporated within the same unit is a similar circuit for timing the operation of a motor used to replace the paper on the writing table.

(3) Writing table. In this device, a roll of 7-in. adding machine paper is threaded out through a set of sponge rubber rollers over a pane of glass set flush with the table top, and drawn on over through a second set of motor driven rollers. A light box beneath the pane of glass illuminates the writing surface from below. The foot control button indirectly closes the circuit through the motor drive for a period of time controlled by the tachistoscope. Releasing the foot button resets the tachistoscope for the next occasion.

As has been indicated in the foregoing discussion, this equipment is particularly well adapted to the study of consumer interest in advertising. It seems quite possible, however, that it might be found of value in various other types of research problems, such as the study of reaction times in response to various visual stimuli, as a simple substitute for the metronoscope in increasing the speed of reading, or in any type of research employing the presentation of a visual stimulus.

A NEW OBSTRUCTION BOX

By G. L. FREEMAN, Northwestern University

In the usual type of obstruction box the animal is re-introduced to the starting compartment after each successful crossing.[1] Such handling may very well interfere with goal orientation and so effect behavior measurements. Furthermore, the fact that only one type of obstruction (shock) is readily used does not commend the method for studies of frustration in which barriers of varied type are needed. A new obstruction box now in use in our laboratory seems to meet these difficulties and to present certain other advantages for the study of frustration and motivation in rats.

The apparatus (Fig. 1) consists of a continuous runway, divided into eight segments and arranged so that obstructing barriers may be introduced into each segment. The goal object (food or mate) is placed in a basket and moved around the runway somewhat after the fashion of the mechanical 'rabbit' in whippet racing. After systematic deprivation, the animal is introduced in a segment of the runway and allowed to connect with the goal object at a 'choice-point.' Then the basket is quickly withdrawn and moved across a 'barrier' by the experimenter, who stands in the center of the apparatus. In order to make further contact with the goal, the animal must now leave the choice-point and pass through the barrier. When such a reaction is made the animal is again allowed a momentary connection before the basket is moved ahead. Segments of the apparatus lying behind each new choice-points are closed off so that the animal is always presented with the alternatives of advancing against obstruction in order to achieve a goal or remaining at the choice-point. Several types of barrier are available: (1) a continuous galvanized steel trough running under the choice-points is filled with *water* of constant tempera-

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[1] F. A. Moss, A study of animal drives, *J. Exper. Psychol.*, 7, 1924, 165-185; C. J. Warden, The relative strength of the primary drives in the white rat, *Ped. Sem.*, 41, 1932, 16-33.

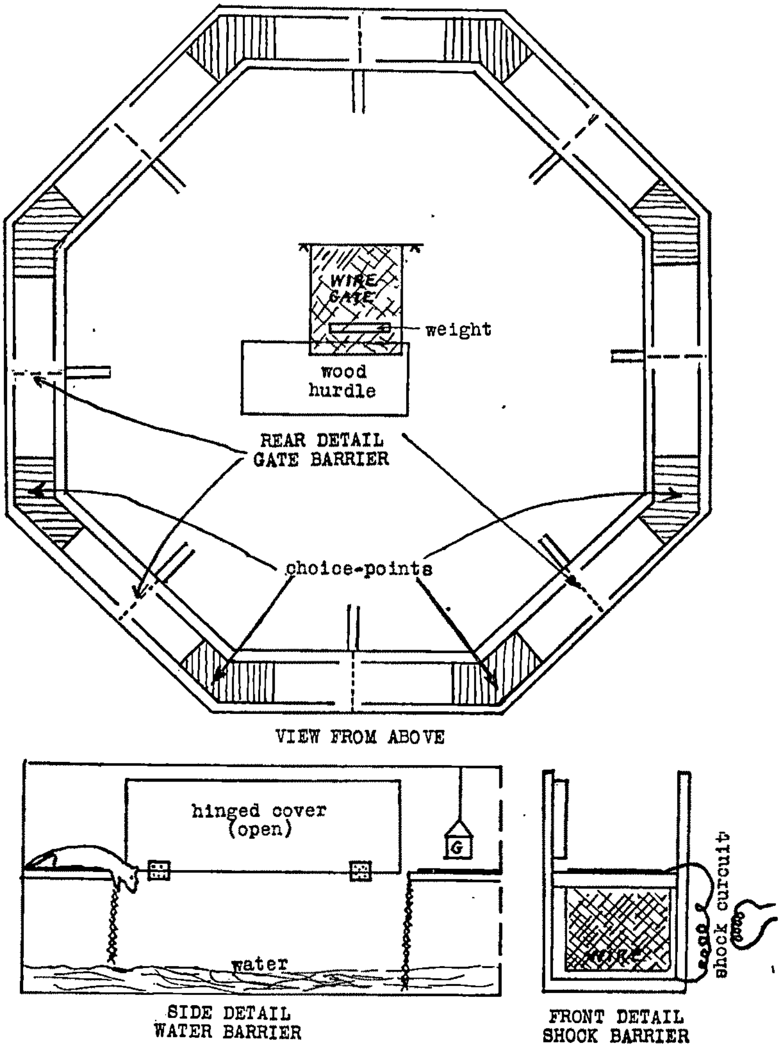

FIG. 1. A NEW OBSTRUCTION BOX

ture; (2) the trough may be drained and an *electric shock* delivered to the animal as he starts from a choice-point down into the now empty trough; (3) hinged covers may be dropped over the exposed part of the trough and weighted *wire gate* barriers placed between choice-points. Barrier as well as goal object is not changed in a given test period.

In actual practice if an animal stays at a choice-point or refuses to cross the barrier within an arbitrary time limit of 2 min., the goal object is again momentarily presented and withdrawn. This makes it possible to put the animal, technically at least, through a fixed number of units each day. Since the total number of units crossed has been found to correlate highly with average time spent per unit, we have usually chosen to end a day's run at the eighth unit and to permit full connection with the goal therein; animals who do not actually cross the entire eight units are not so rewarded before removal to the home cage. A few preliminary runs against a barrier at minimal potency are necessary to make the animal goal-oriented. When systematic experimentation begins the barrier is advanced to maximal potency.

By use of this apparatus it is possible to obtain several quantitative measures of behavior. These include: (1) time spent in obstruction unit (average and m.v.); (2) number of physical contacts with the barrier; (3) amount of vacillatory activity at a choice-point (turnings, tentative approaches); (4) number of regressions (refusal to advance upon the barrier or to maintain goal-orientation); and (5) eliminative behavior. It is also possible to obtain at choice-points measures of galvanic skin resistance by an adaptation of the potentiometer technique described by Muenzinger and Mize.[2]

APPARATUS NOTES

A MODIFICATION OF THE DARROW BEHAVIOR RESEARCH PHOTOPOLYGRAPH

The Darrow Behavior Research Photopolygraph[1] is designed to record various physiological phenomena on a roll of photographic paper 175 ft. long and 6 cm. wide. The paper recommended by the manufacturers is P.M.C. normal white glossy bromide paper no. 1,[2] wound on core no. 93. At the current price of $3.15 per roll, this paper costs 1.8¢ per ft. A substantial reduction in the cost of operating the photopolygraph can be effected by slightly modifying the camera so as to permit the use of Insurance Bromide paper, Grade A,[3] which at the current price of $2.65 per 250-ft. roll, costs only 1.06¢ per ft. Insurance Bromide is 2⅜ in. wide, wound on core no. 1.

In order to convert the photopolygraph for the use of the slightly wider Insurance Bromide paper, it is necessary to widen the driving mechanism. This may be done by inserting a no. 8 brass washer at the end of each of the spacers of the driving mechanism. The bearing-plate which is thus moved from its former position is then soldered to the two pieces which form the slit of the camera. To remove the driving mechanism from the camera, detach the upper gear from its shaft and remove the two screws which are directly above the gear train. The driving mechanism can then be slid out from the other side of the box.

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[2] K. F. Muenzinger and R. H. Mize, The sensitivity of the white rat to electric shock: threshold and skin resistance, *J. Comp. Psychol.*, 15, 1933, 139-148.

[1] Darrow, C. W., The behavior research polygraph, *J. Gen. Psychol.*, 7, 1932, 215-219.

[2] C. H. Stoelting Co., Chicago, Illinois.

[3] Manufactured by Eastman Kodak Co., Rochester, New York.

Since the Insurance Bromide is thinner than the P.M.C. Bromide, the large removable roller should be covered with a piece of latex pneumograph tube in order to prevent the small driving roller from slipping on the paper. A strip of felt glued into the groove in the inner side of the lid of the metal paper magazine serves to provide room for the wider roll of paper and also maintains a light-proof joint.

The Insurance Bromide is a more sensitive paper than the P.M.C. Bromide. It is thus necessary to replace the red glass in the side of the photopolygraph with a piece of opaque material. Furthermore, a 15-c.p. single contact lamp should be substituted for the 32-c.p. lamp with which the apparatus is equipped.

The following developer has been used satisfactorily with the P.M.C. Bromide paper exposed by a 32-c.p. lamp:

Potassium bromide	0.5 gm.
Elon	1.5 gm.
Hydroquinone	5.8 gm.
Sodium sulphite	19.4 gm.
Sodium carbonate	38.9 gm.
Water	40.0 oz.

In using the Insurance Bromide at a speed of 4 mm. per sec. with the 15-c.p. lamp, this developer should be mixed with an equal amount of water. At a speed of 2 mm. per sec., one part of developer to 2 parts of water is satisfactory.

University of Missouri FREDERICK A. COURTS

A MODIFIED GALTON BAR

Judgments made on the Galton bar ordinarily found in the laboratory are frequently affected by visual cues on the face of the apparatus, *e.g.* flecks of dust, scratches, dents, and paint-blemishes. The involuntary or deliberate use of those cues introduce a source of error into the *Os*' judgments which may render the measurements invalid. We have, therefore, adopted in our laboratory a modification of the apparatus—first proposed and successfully used by Hamilton in 1929[1]—which eliminates all of those cues. Because Hamilton's modification is not generally known, we present here the essential features of our modification.

The usual bar-apparatus is placed in a large light-proof box into which *O* looks through an eye-piece. The standard and variable bars are adjustable slits mounted in the rear of the box. The slits are backed with a large piece of frosted glass, which diffuses the illumination—a bank of lamps, a Lumiline, or a north window. On looking through the eyepiece *O* sees two bars of light against a black background—no details of which are visable. Judgment of relative extent is, consequently, based solely on the apparent length of the two bars.

The instrument used by us consists of a box of 60 x 60 x 25 cm., painted black on the inside. *O*'s eyepiece is taken from a stereoscope. The two slits are 1.5 x 30 cm. and are covered by black metal shutters sliding in grooves fastened to the outside of the box. The slit on the left is located 2.5 cm. below that on the right, but there is no vertical overlapping until both slits are open to an extent exceeding 25 cm. The shutters are adjusted by means of a strong string attached to them at a point above or below the slit (so as not to appear in the illuminated bar) and extending it in a loop between two large pulleys at the sides of the box. Tension on the string is maintained by a small spring. The pulley to *O*'s right is attached by means of a flexible coupling to a shaft carrying the control knob. The apparatus is so arranged that either shutter may be used as the variable and the standard by a simple alteration of the string attachments. This permits a change of space order. Pointers, attached to the shutters, move along millimeter scales. Variable electrical contacts may be provided on the scales and pointers for producing sound or light signals, if desired, to indicate a given error of judgment.

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[1] H. C. Hamilton, The effect of incentives on accuracy of discrimination measured on the Galton bar, *Arch. Psychol.,* 16, 1929, (no. 103), 1-73.

College of William and Mary R. H. HENNEMAN and J. C. TALLEY

NOTES AND DISCUSSIONS

CORRELATIONS BETWEEN THE UNIVERSITY-GRADES OF PARENTS AND CHILDREN AND OF SIBLINGS

This is a study of the correlation between the standing in the University of Michigan between parents and children and between siblings, for the light it may throw upon inheritance of intelligence.[1] Three previous statistical approaches to the problem have been made. The first was Pearson's determination of the coefficient of correlation between siblings based on the estimates of teachers.[2] His frequently quoted conclusion was that intelligence and other mental characteristics showed a correlation coefficient of about 0.50. This is approximately the same as the correlation for physical measurements.

A second study was made by Shuster and Elderton of the relative standing over a period of 92 years of fathers and sons in the Oxford University honors examination.[3] The honors examination is a test extending over four days and is reputedly one of the most severe academic tests applied anywhere. The results use only five ranks: first, second, and third class honors and passed and no degrees. The grades are sharply marked off at the top but group the great mass into only two categories. Slightly more than a third obtain honors against nearly two thirds in the pass degree and no degree group. The coefficient of correlation between fathers and sons was 0.312, and between brother and brother about 0.40, from 0.39 to 0.42 by different methods of calculation.

A somewhat similar study of the boys in Harrow and Charterhouse indicates correlation of about the same amount for brother-brother relationship. Here the ranking in the class for one term was taken as the basis of comparison. The average coefficient of correlation for all groups was 0.398, a very close agreement with the Oxford result. No values were obtained for the father-son relationship as the records did not give the names of the fathers. In an appendix to the article,[4] Karl Pearson states that all such correlations from academic groups are too low because the individuals who attend colleges are selected from a small section of the population. He developed a formula which made possible the calculation of the true correlation from the rough one on the assumption that the variability is reduced through self selection by a definite percentage. Applying this formula to the Oxford and Charterhouse studies he finds that the correlation should be 0.44, if the distribution is reduced 15% and 0.54, if it is reduced 25%. Whether the reduction is one or the other of the percentage can hardly be regarded as proven, but there is certainly some selection so the coefficient should probably be greater than it appears on the face of the returns. This brings the correlations to the neighborhood of 0.50 which Pearson's students have regarded as normal since his first paper.

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[1] Thanks are due to the National Youth Administration, and to the Rackham Fund of the University of Michigan for funds to carry on the research. I desire to thank Professor George Meyer for asking his class in statistics to compute the correlations for the siblings, and Mr. Forth, Mr. Bussell, and Mr. Bond of the National Youth Administration for collecting the data and for much help in computations. I desire especially to thank Miss Ann Sylvester for checking the computations of the others and for many computations of her own.

[2] Karl Pearson, Inheritance of mental and normal characteristics in man, *Biometrika*, 3, 1904, 131-190.

[3] Edgar Shuster and E. M. Elderton, The inheritance of ability, *Eugenics Laboratory Memoirs*, 1, 1907, 3-42.

[4] Karl Pearson, Influence of academic selection on correlation coefficients, *Eugenics Laboratory Memoirs*, 1, 1907, 41-42.

We need not summarize the results of the studies of twins and ordinary siblings. The method was different in that the measurements were by tests, the subjects were of all ages, and entirely unselected from the community. It will be recalled that, according to Newman and his collaborators,[5] identical twins show correlations of 0.90 or more, fraternal twins from 60-80 and ordinary siblings of 0.50-54.

All of the studies agree in showing considerable similarity between father and child and brothers and sisters. Our investigation takes a form more like the study of Shuster and Elderton. We also dealt with school grades and calculated the correlations both between parents and children and between siblings of the same family. The fathers and mothers of students in college at the time of the investigation were traced from answers to a question on the blank filled in by entering students that asked where and when a parent attended college. These were confirmed by the records. We made three different studies of correlations between parent and child. In the first two cases the parents had attended the university before the present system of using letters for designating standing had been introduced.

Down to 1912 the only grades recorded were passed, conditioned, and failed. The conditioned students were given a second examination. This mark indicates the mediocre students in the particular course. We used a system suggested by Mr. Bussell, one of our N.Y.A. workers, to assign numerical values to these rough designations. A record of 9 was assigned arbitrarily to all students who took just the required number of courses and passed each of them. The others had a point subtracted for each course failed and a half-point for each course in which they were conditioned. Students who took more than the required number of courses were given added credits. There were few of these. In our first study of 294 pairs, the grades grouped themselves rather narrowly. Forty-nine had a grade of 9 and only 12 fell below 5.

The children all attended while the usual letter grading was in vogue. For these we assigned a value of 4 to A; 3 to a B; 2 to a C; 1 to a D, and 0 to an E. The sum of these points is called the honor point record, and what is known as the honor point ratio is the quotient of the sum of the honor points divided by the total number of hours taken. Our scheme for changing the older passed, conditioned, and failed marks into numerical values lumps all the best individuals into one group. This included approximately a sixth of our first group. The Oxford studies on the other hand distinguished the best fairly sharply, into at least three groups, while it lumped the great mass into only two categories. The letter grades made possible very close indications of standing for the best and the worst.

Two studies were made at different times comparing parents graded on the old system with children graded on the new. The first correlated 294 pairs of parents and children and gave the very low correlation of 0.05 ± .038 A second group of 112 different pairs gave the higher correlation of 0.17 ± .062. No satisfactory explanation can be given as to the large difference. The span in years between attendance of the parents in the two groups was a little greater for the first group. That, however, should not be an important factor. There were rather more required courses in the period when some of the parents attended the university. That might have made a difference but should have worked to the disadvantage of all parents alike, and so should not have affected the correlations.

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[5] H. H. Newman, F. N. Freeman, and K. J. Holzinger, *Twins: A Study of Heredity and Environment,* 1937, 55-93.

The wide divergence in the two coefficients and the question as to the accuracy with which our arbitrary transfer of passed, conditioned, and failed into numerical values represented actual differences in ability led us to undertake a third study. The individuals in this study were all made up of pairs in which the parents attended after the new marking system was put into effect. Both parents and children were rated by honor points, with its greater fineness of discrimination at all levels of excellence. In this third group, as in the earlier, where two children of the same family were found the average honor point ratio for the two was used in the computation. This method of averaging the standard seems preferable to using each child separately as did Shuster and Elderton or to omit one child as did Dr. Gordon. To omit one might lead to yielding to the temptation of selecting the one that would favor a desired result, if any result was desired.

The third group consisted of 123 pairs. The coefficient of correlation was 0.23 ± .057. This result is obviously more reliable than either of the other two. The measures of standing are more accurate and more consistent for parent and child. The time interval between parent and child was less than in the other two studies, at the greatest a difference of twenty-six years. None of the parents were in college at the time the required course system was effective. There would also be less difference in the organization of courses and in other imponderables of attitude of students and of the personnel of the instructing force. If we should average the three correlations without considering the number of cases, the result is a coefficient of 0.15. A better idea of the result would be given by saying that it ranged in three groups from 0.05 to 0.23. It would probably give a truer picture if we considered only the last determination, obtained under the best conditions. The latter value shows a significant if slight relationship, the former none at all. It may be worth while to indicate that all are positive.

A study of the correlation in standing between siblings of the same family did not meet with the difficulties encountered by those of parents and children. All siblings studied were in attendance within comparatively few years. The methods of grading were the same and the social and educational conditions did not vary appreciably. First a coefficient of correlation was obtained for all pairs without reference to sex. This was 0.25 ± .05 for the 175 pairs used. Then the data were broken down with reference to sex and the three possible relationships: brother-brother, sister-sister, and brother-sister were calculated separately. Wide divergences were found for the different combinations. For 85 cases of brother-brother combinations, the correlation was 0.149. The 26 cases of sister-sister pairings gave a coefficient of 0.565, and 63 cases of brother-sister groupings showed a coefficient of 0.464. One might ask why the values were so different between the groupings. It is easier to ask than to answer. There are no obvious reasons why sisters should be more alike than brothers, or why a boy should be more like a sister than a brother. In the latter case one would expect the relations to be reversed. Probably the differences must be charged to chance, due to an insufficient number of cases, when they were divided into the smaller categories.

It might be mentioned that an earlier investigator, Elderton,[6] found similar discrepancies between different pairings based on sex, but the differences did not in all cases run in the same direction as ours. The results were derived from studies of brothers and sisters in two English elementary schools. Miss Elderton does not say how many were studied, but is apologetic for the small numbers. Two methods of evaluating intelligence were used: the Stanford-Binet and estimates by teachers. Corrections were made for differences in age which were believed necessary because the brighter children in the upper grades had been promoted. The correlation between brother and brother was 0.489 when the I.Q. was used as a measure and 0.486 when the teachers' estimates were used. The sister-sister correlations were 0.384 and 0.540 for the two methods of measurement. The brother-sister correlations were 0.532 and 0.499 respectively. The differences are much less than those we found and differ in direction. For her the brother-sister correlations were highest while in our results they were intermediate to the very high sister-sister correlation and the very low brother-brother. From the small number of cases available in either study, one can hardly draw conclusions that would hold as to the general hereditary resemblances between the sexes, even were the results consistent in the two studies.

As compared with the Galton Laboratory work our correlations are all considerably lower. The father-son relationship for Oxford honor students gave a coefficient of 0.312, our parent and child correlation was 0.23 for the most favorable conditions. The sibling coefficient (brother-brother) at Oxford and for the public schools was 0.39 to 0.42 and 0.398 respectively, ours was 0.25. We could argue on Pearson's assumption of restricted selection from the population of students that the true correlation would be ten points or more higher. Since any estimate as to how great the selection is, is purely arbitrary, all calculations would be of little value. We can rest content with the statement that they could be made higher if selection is taken into account. An explanation of the difference may lie in the more comprehensive material involved in the grades in the two cases. The Oxford honors examination is a comprehensive test covering a relatively limited class of material that is approximately the same for all examinees. The grades given in an American university cover very diverse material, with somewhat changing standards of marking for the same course at different times and for different courses. The smaller correlation may well be due to the diversity of the material and of the scoring. The lower correlations obtained when tests are used as a measure rather than college standings of all sorts would also be due to the restricted material of the test and to a smaller degree of selection among the individuals tested. Moreover, intelligence is probably only one of several factors in determining college standing. In addition, persistence, confidence in ability and many other emotional and volitional qualities contribute to determine the accomplishment and the grade that will be given. The test requires ability to work intensively for a brief period, while success in college demands continued effort and often without immediate incentive. These other factors are also probably inherited, but not necessarily in the same degree.

If one considers only the higher correlations and makes an allowance for selection, the correlations appear fairly significant. The correlation between brothers and sisters is not so much lower than between the results of mental tests and standing in college or than between standing in lower schools and standing in college. If one chose students on the basis of the standing of a brother or sister one would not do so much worse than to choose by tests or by high school grades. Siblings give a closer correlation because both have the same parents, while the correlations between parent and child consider only one parent.

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[6] E. M. Elderton, Present position in regard to the inheritance of intelligence, *Biometrika*, 14, 1923, 398 ff.

University of Michigan W. B. PILLSBURY

A NOTE ON THE USE OF "OMNIBUS" TRAINING TO VALIDATE SEASHORE'S "CAPACITY" HYPOTHESIS

Many investigators have concerned themselves with the validity of the Seashore tests, using as their criteria of validity grades in applied music and theory courses. These investigations have in most instances indicated relatively low coefficients of correlation.[1] Against this procedure Professor Seashore has vehemently protested, maintaining that the tests "should not be validated in terms of their showing on an omnibus theory or blanket rating against all musical behavior, including such diverse and largely unrelated situations as composition, directing, voice, piano, violin, saxophone, theory, administration or drums; because there are hundreds of other factors which help to determine the job analysis in each of such fields."[2] Seashore further states that "I have been bombarded all these years by the omnibusists for this type of validation, but I have persistently refused action on the ground that it had little or no significance."[3] The tests, he says, "represent the theory of specific measurements insofar as they conform to the two universal scientific sanctions, on the basis of which they were designed; namely, that (1) the factor under consideration must be isolated in order that we may know exactly what it is that we are measuring, and that (2) the conclusion must be limited to the factors under control."[4]

A correlative assumption in Seashore's psychological theory is that training has no significant effect upon scores in the Measures of Musical Talent. This is one of the fundamental postulates of the "capacity" hypothesis.[5] In this connection, Stanton's studies on the effects of musical training upon talent scores have been approved and sponsored by Seashore and have been accepted generally.[6] In his preface to one of Stanton's studies, Professor Seashore writes:

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[1] P. R. Farnsworth, An historical, critical, and experimental study of the Seashore-Kwalwasser test battery, *Genet. Psychol. Monog.*, 9, 1931, 291-391; J. L. Mursell, What about music tests?, *Mus. Ed. J.*, 24, 1937 (Oct.), 16. The issues of the test discussion, *ibid.*, 24, 1938 (May), 22.

[2] C. E. Seashore, Two types of attitude toward the evaluation of musical talent, *ibid.*, 24, 1937 (Dec.), 25.

[3] *Op. cit.*, 26.

[4] *Op. cit.*, 25.

[5] A paper on "The present status of the 'capacity' hypothesis in Measures of Musical Talent" is now in preparation. This paper will attempt to classify, summarize and evaluate the literature dealing with the question of the non-improvability of scores in the Seashore Measures through training.

[6] H. M. Stanton, and W. Koerth, Musical capacity measures of adults repeated after musical education, *Univ. Ia. Stud., Aims & Prog. Research*, 31, 1930, 1-18; Musical capacity measures of children repeated after musical education, *ibid.*, 42, 1933, 1-48; H. M. Stanton, Measurement of musical talent, *Univ. Ia. Stud. Psychol. of Music*, 2, 1935, 1-140.

> The Measures of Musical Talent here discussed were built on the assumption that they should measure these specific capacities before musical education was begun and that the capacities would not be greatly modified by training. Attempts to validate this assumption have been made by various indirect methods, but the final critical proof has awaited the accumulation of successive measurement upon the same individuals before, during, and after musical training. This has been accomplished through the accumulation of a remarkable series of measurements by Professor Stanton.[7]

Seashore apparently relies upon such data as Stanton's to substantiate his claim that training does not materially improve test scores, and that the tests are, therefore, stable 'capacity' measures, on the basis of which predictions of future scores may safely be made.

The correctness of these views must in large measure depend upon the appropriateness of the training employed. It is, therefore, pertinent to inquire into the type of training used by Stanton. This consisted of (1) individual lessons in voice and instrument—piano, violin, 'cello, harp, organ, flute, tympani, (2) group training in orchestra and chorus, and (3) theoretical subjects such as harmony, counterpoint, appreciation, theory and conducting—the typical 'omnibus' situations, 'diverse and unrelated,' which were rejected by Seashore for *validation* of the tests!

If the tests cannot be validated against 'omnibus' criteria, how can the failure of such training to produce significant increases in scores be said to prove that scores in the tests are not improved by training of any kind? If the only criteria for validation are "specific" and "internal," *e.g.* correlation of the Sense of Pitch test with "objective records of musical performance in pitch intonation or ability to hear artistic pitch deviation in the musical situation," then may not also "specific" training, closely related to the content of the tests, be requisite for proof of the assumption that the test scores are unaffected by training? Obviously, it is logically possible that certain "omnibus" training procedures may transfer in some degree to a "specific" test, but the real question is whether anything short of intensive training which is purposefully remedial and directed toward improving a "specific" capacity, can be said to demonstrate conclusively that training is immaterial. That it is possible to develop such specific training methods seems indicated by Wolner's study of the effects of training upon pitch-deficient children.[8]

Assuming that the Seashore tests are valid, and accepting Seashore's criticism of the use of "omnibus" criteria to challenge the validity of his tests, it seems proper to question the use of the 'omnibus' type of training to demonstrate the non-improvability of 'capacity' scores.

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[7] Stanton and Koerth, *op. cit.*, 42, 1933, 3. Stanton's method was to give the tests to students entering the Eastman School of Music and to retest after a 3-yr. interim of formal musical education. The absence of significant improvement in scores upon retest is the "final critical proof" referred to above.

[8] Wolner gave intensive individual training in vocal reproduction of intervals and in pitch discrimination to 7 children who were so markedly pitch deficient that they were not only unable to discriminate tuning fork differences of 30 ~ (at a standard of 435 ~), but were incapable of distinguishing piano intervals as large as an octave. Each child was given 20 min. of training daily, 5 days a week for a period of 81 days. Results in part: 4 out of 7 of the children reduced their thresholds in pitch discrimination to 0.5 ~; the remaining 3 had thresholds of 2, 3, and 5 ~ respectively. (Cf. M. Wolner, An experiment in individual training of pitch-deficient children, Unpublished Master's dissertation, Colleges of the City of Detroit, 1932; M. Wolner, and W. H. Pyle, An experiment in individual training of pitch-deficient children, *J. Educ. Psychol.*, 24, 1933, 602-608.

Northwestern University RUTH WYATT

A SIMPLE MEANS OF PRESENTING AMBIGUOUS VISUAL MATERIAL POSSESSING COLOR, DEPTH, AND MOVEMENT

The success of the Rorschach test has called attention to the valuable diagnostic material that becomes available when the individual is told to organize and interpret relatively ambiguous sensory materials. Stern's[1] development of cloud pictures and Skinner's[2] use of the verbal summator are other recent examples of the same general type. This note describes a simple means of presenting ambiguous visual material which possesses color, depth, and movement, and which may be interpreted in meaningful fashion by an observer.

The principle involves the passage of polarized light through cellophane. If white light, which vibrates in heterogeneous directions, is passed through a polarizer it is then given a uniform vibration direction. Upon its further passage through a layer of cellophane, which acts as a double refracting medium, the light becomes elliptically polarized, *i.e.,* the vibration directions fall in a continuous ellipse. The constants of the ellipses are different for each wave-length composing the white light. If this elliptically polarized light is then passed through a second polarizing medium, there is a differential absorption which is determined by the constants of the ellipses and hence is different for each wave-length. The original white light will then become colored since the rays of different wave-lengths are being differentially absorbed.

These are interference colors, and a subtractive spectrum is available. The spectrum is a first-order interference spectrum of gray, white, yellow, orange, red, purple, blue, and green. Since the absorption is determined by the ellipticity for a ray of any given wave-length, and since this ellipticity is a function of the thickness of the double refracting medium, the original color will be determined by the thickness of the cellophane.

If the second polarizer, or analyzer, is rotated through 90°, the rays fully transmitted at first will now be fully absorbed, and those previously absorbed will be transmitted. There will be a corresponding reversal from transmission to absorption for the rays of those wave-lengths partly transmitted. Therefore, as the analyzer is rotated, the original color will pass through a stage of desaturation and then change to its complementary. The color thus is a function first of the thickness of the double refracting medium and secondly of the angle of rotation of the second polarizer.

These principles may be applied specifically as follows: White light from a concentrated light source (electric light bulb) is passed through a polarizing screen consisting of either a celluloid film (Polaroid type 2) or of a celluloid film laminated between two layers of glass (Polaroid type 1). These are supplied by the Polaroid Corporation. This polarized light is then passed through cellophane. One must be careful to use cellophane and not kodapak or protectoid, as these last do not have the requisite molecular orientation. To hold the cellophane securely it may be fitted into the interior of a section of cardboard packing tube. If the fit is a snug one, the sides of the tube will hold the cellophane in place. If one now places a second polarizing screen against the end of the tube and looks through this lens at the cellophane within the tube, one sees a varicolored, patterned field not unlike the field of a kaleidoscope, except that it has depth.

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[1] W. Stern, Cloud pictures: A new method for testing imagination, *Character & Pers.,* 6, 1937, 132-146.

[2] B. F. Skinner, The verbal summator and a method for the study of latent speech, *J. Psychol.,* 2, 1936, 71-107.

The colors are determined both by the thickness of the individual sheets of cellophane and by the rotation of the second polarizer. The thickness of the cellophane may be varied by superposing individual layers on each other. These may be stuck together with ordinary rubber cement, or merely placed at different depths in the tube. Patterns may be worked out by varying the number of layers of cellophane in different parts of the field. One may get an excellent random effect by simply crumpling up the cellophane into a ball and placing it in the tube. By wetting the cellophane and stretching it in spots, distortion of the molecular orientation is caused in local areas with resulting color changes. Many color effects are possible.

Depth is present since one is looking at a three dimensional field, with the different layers of cellophane at different positions in the tube.

The semblance of movement may be obtained by rotating the second polarizing lens. This causes the colors in each part of the field to desaturate, pass into their complementaries, and finally reach their original values again. The resulting color effects resemble movements with the colors continually disappearing and reappearing in various portions of the field.

In one form of the apparatus, as set up by the writer, the parts of the apparatus were mounted in line in a wooden trough. The second polarizing screen was rotated by peripheral friction drive through a gear wheel connected to the shaft of a variable speed motor. The use of a rheostat enables one to control the speed of the color changes or 'movement.' Interchangeable lengths of cardboard tubing were used, so that many patterns would be available.

Preliminary investigation shows that the extra dimensions of color, movement, and depth may act as limiting factors, and that the interpretation is sometimes not as easy nor as varied as with the standard Rorschach ink blots. On the other hand, when interpretation does come it is often more 'vivid.' Many individual peculiarities of response are also noticeable. The method suggests itself as an interesting addition to present interpretative tests.

Wheaton College WILLIAM A. HUNT

BOOK REVIEWS

The Psychology of Art. By ROBERT MORRIS OGDEN. New York, Charles Scribner's Sons, 1938. Pp. vii, 291.

Professor Ogden may have had some difficulty in choosing a title for his book. The first part is called "Aesthetics"; the other four parts deal with the separate arts: music, poetry, visual arts, and eurythmics. By naming only the first part "Aesthetics" he has forestalled the possible criticism that much of what he has written is not esthetics. Whether the book as a whole is psychological is a more debatable question. Certainly the psychological treatment, a combination of response and Gestalt psychology, is in evidence throughout the book, as are also esthetic principles; but Ogden is particularly interested in "good" forms objectively given, and the emphasis—or "ground-rhythm" as he might say—is primarily upon the formal elements. A knowledge of these elements is undoubtedly of much value for the artist, but some readers, unconvinced by the argument that combinations of sound, line, etc., are good or bad in their own right, might wish that the author had devoted more space to explaining why the forms that are found in art happen to be acceptable. This belief in objective form has at any rate given the author the courage to pass dogmatic judgment upon the esthetic values of specific works of art, although he shows a commendable tolerance when discussing the modern trend.

The part on esthetics treats principally of the esthetic attitude, which the author believes should in the narrower sense be called "the artistic attitude," and of the classification of the arts. His description of the attitude is clearly stated in behavioral terms stripped of the traditional and vague philosophical implications which so often confuse the issue.

For Ogden there are two main types of behavior, active behavior which is either positive or negative—or as Holt would say, adient or abient—and passive, which may either be a state of quiescence due to inability through conflict to take an active attitude or passive contemplation and appreciation. All of these types are esthetic "in their primitive or instinctive modes of expression." This is obviously a very broad statement, which, however, he makes more specific when describing the esthetic attitude. This attitude is distinguished from the practical "give and take" attitude by the fact that we do something *with* rather than *toward* an object. The author states that the technical name for the attitude is empathy which, according to the reviewer, seems too general an application of that useful term.

Ogden is very much at home in the field of music and handles the subject as an expert. For the average readers who are interested more in general problems of esthetics than in special fields, he is perhaps too much at home and in consequence for them much of this chapter may be difficult of comprehension—as for example, his description of the melodic principle of the tonic trend: "All melodies composed in a scale of just intonation will indicate effects of finality on those notes which, in relation to preceding notes, reduce to the ratio-number 2 or a power of 2." As Ogden agrees that "music alone can give a sheer pattern," he devotes most of these chapters to the musical scale, harmony, counter-point, cadence and other items of musical composition. He also describes atonic music, folk music, and primitive music.

After explaining Schönberg's composition on the principle of equal semi-tone intervals and their multiples, he states that "Although untutored listeners cannot immediately 'hear' this logic, there appears to be no reason why they should not learn to do so." One wonders how necessary it is to know this logic in order to appreciate Schönberg and whether, if such knowledge is obtained, the appreciation does not become fundamentally intellectual rather than esthetic. The same thing might be said of a knowledge of dynamic symmetry which will be mentioned later under the visual arts. In this connection Ogden admits, quite rightly, that the formal relations are usually apprehended naïvely, as pleasing or otherwise, without direct knowledge of the underlying geometrical forms. Even such simple mathematical relations as those in the musical scale may not be known by a person of keen musical appreciation. The reviewer remembers the day which an outstanding composer spent with him in order to learn about these tonal relations. Surprisingly his first question was: "What is meant by the octave being a relation of 1 to 2?"

In the chapter on poetry the author shows in a most interesting way how close is the relationship between form and meaning. As he correctly remarks, "behavioral impulses determine the meaning of words and phrases, and these find their expressive utterance in verbal forms, more or less suited to the intent of the mover." Most editors would, however, probably think that he is too optimistic when he states: "Any meaning which affects our being will demand a concise and simple way of saying it." These chapters contain much detail about poetic form with excellent illustrations, and there is also an interesting and important discussion of the relation of poetic thought to prose.

The part on visual art, which covers the broadest field, takes up almost half the book. The keynote is contained in the statement, "Starting with a canvas, a block of stone, or the area of a projection screen, the artist's problem is to design this given space with an enrichment of partial patterns which will improve and make more evident its unique and original wholeness." The first of these chapters, therefore, deals with the perception and representation of space. In his explanation of the third dimension the author places too much importance upon the act of convergence, which he uses even to explain the effect produced by the stereoscope. The chapter on color contains much that is found in an introductory psychology, including surface and film color, and color music.

The chapter on geometric design is concerned primarily with naïve geometry and dynamic symmetry which are familiar to us principally through the writings of Hambidge. The author soon has the reader thinking in terms of root-two rectangles, golden sectors, and whirling squares. That buildings like the Parthenon may be analyzed into geometrical designs cannot well be denied, and for those who are unacquainted with the intricate form of symmetry to be found in art, this chapter is valuable. But one often wonders in following the analysis of pictures, statues, and architecture in terms of dynamic and static symmetry how much is merely wish-thinking upon the part of the enthusiastic author and how much the actual intent of the artist.

In the chapter on painting and sculpture "certain influences of materials and techniques on drawing, painting and sculpture" are discussed. The important difference in the effect of free carving and modeling, and of drawing and painting, are clearly outlined. The development of modern art is also sympathetically presented.

The chapter on the techtonic arts, in addition to buildings, includes house furnishing, costumes, gardening and landscaping, staging and screen-presentation. The principles underlying the treatment of masses and the harmony of parts are rather fully explained. The range of illustration is from the Temple of Neptune at Paestum, through Notre Dame of Paris and the Empire State Building, to modern streamlined dwellings and factories.

In the part on eurythmics the author takes occasion to impress upon the reader the importance of well-coördinated rhythmical movement, and lauds the efforts of Dalcroze to encourage the attainment of intricate, rhythmical responses. In the final paragraph of the book, in discussing the reciprocal adjustment between the organism and the environment, he makes the important statement that "The greater the degree of orderliness, the greater will be the efficiency of this adjustment. Less lost motion, and more enjoyment: this is the simple and practical precept of an esthetic method of education in all arts and all the skills."

The Psychology of Art is a valuable book. It contains much evidence of the erudition, sensitive esthetic appreciation and versatility of the author. Owing to the originality of style and the nature of the subject matter, the book is not easy reading and therefore probably not well-suited for the beginning student of esthetics or art. It will, however, undoubtedly prove helpful to the specialists in the various fields which are included in the book.

Princeton University H. S. LANGFELD

Psychology of Music. By CARL E. SEASHORE. New York, McGraw-Hill Book Co., Inc., 1938. Pp. xix, 408.

The motto of this book is said by Professor Seashore to be *"Not psychology but to psychologize."* That is, the book is intended to be a series of incomplete skirmishes upon various problems of music, designed to "stimulate and guide the student of music in scientific observation and reasoning about his art."

The problems discussed are both theoretical and applied. Under the first rubric may be included those chapters (A Musical Ornament, The Vibrato; Pitch; Loudness; Duration; Timbre; Tone Quality; Sonance; Consonance; Volume; Rhythm; Timbre of Band and Orchestral Instruments; Violin; Piano; Voice) dealing with the physical characteristics of renditions by voice and various instruments as recorded by oscillograph, piano-camera, and harmonic analyzer, and as displayed in the numerous 'performance scores,' tone spectra, and tables. These physical characteristics are related to the perceived tone; and the point is emphasized again and again that in musical art "all is illusion," *i.e.* what is heard is rarely, if ever, an exact duplication of the physical event. Other topics of psychological significance are considered, such as pitch discrimination, subjective tones, audible frequency ranges, audiometry, beats, and resonance, although at no time is a complete summary of facts attempted, and many times key definitions are completely omitted. Indeed, an outstanding quality of the book is its skeletal and selective structure.

Throughout, there is observable Seashore's deep concern with problems of application, which becomes explicit in his chapters on Learning in Music, Principles of Guidance in Music, Measures of Musical Talent, Analysis of Talent in a Music School, Analysis of Talent in the Public School, and The Development of Musical Skills. His program of application involves not merely the psychologist with his "Measures of Talent" test-scores, but also critical appraisal of many more data, such as motivation, family tradition, and willingness of the subject to work.

While these chapters on application are highly valuable, on the whole, the reader will stand aghast at the recommendation to "trust the first impression . . . instead of repeating the impression, repeat the recall or memory," and at the blanket rule to learn by parts. The treatment of Thinking in Music, and Imagining in Music are unsatisfactory. "Thinking" resolves itself into a matter of I.Q., and involves a discussion of the questions "Are musicians, as a class, intelligent?" The chapter on imagining in music simply quotes various composers on the rôle of imagery in their own work, and does not attempt to go much further, except to state that imagery may operate in the hearing, recall, and creation of music. Seashore states that although imagery "is a phenomenon which does not lend itself to psychophysical measurements," he is inclined "to give it a central place and to expect the most immediate and helpful leads from an informal inventory of the use of mental imagery." But surely the student of remembering and imagining will have more to offer, even if only by way of a program of research, than a mere "informal inventory." Indeed, a general criticism is that Seashore seems to stop short with his quantitative data and simple report, never suggesting the possibility of laboratory investigation into the 'higher mental processes.' Again the reader may quarrel with Seashore's definition of rhythm as an "instinctive disposition," or his derivation of rhythmic measure from the dubious "attention wave."

The book has as a main theme the contention that the psychologist in music has two chief tasks: (a) the measurement of four physical variables (frequency, intensity, duration, and composition of the sound wave) and the determination of their psychological correlates under various conditions; and (b) the integration of facts from anatomy, physiology, physics, anthropology, and acoustics. The first of these problems is the main concern of the book; the second is resolved mainly by proclamation of the problem.

There are, in addition to the already mentioned topics, brief chapters on The Inheritance of Musical Talent, Primitive Music, and Musical Esthetics, and three preliminary chapters on The Musical Mind, The Musical Medium, and The Science of Music.

Under the hands of a competent and informed instructor, the book may be used to great profit with students having some slight acquaintance with the first principles of psychology. Its extended dissection of performance scores, alone, should yield the student some exciting knowledge and understanding of the possibilities of scientific investigations into the realm of music.

Cornell University JOHN IRVING LACEY

Grundlinien der Kunstpsychologie. By OTHMAR H. STERZINGER. Leipzig, Leykam-Verlag, 1938. Pp. xiv, 279.

The main purpose of Sterzinger's book is to establish some differentiation between experimental esthetics and experimental psychology in the analysis of art. On the hypothesis that the former is strictly normative, and the latter strictly factual, he has constructed a text of very systematic exploration of all the elementary sensory functions, perceptual processes, and subjective factors which contribute to any form of esthetic experience. Accordingly, in addition to vision and hearing, there are discussions of tactual, olfactory, gustatory, kinesthetic, and organic (including sex) esthetic response.

It is clear that no single volume could possibly contain more than an outline of the scientific aspects of these categories, and the reader who expects a Helmholtzian treatment is doomed to swift disappointment. On the other hand, the book is provocatively rich in suggestions and problems for further experimental study. Among these may be mentioned the relation of the feeling-tone of colors to specific materials, and to specific and relative brightness values and the "illusions" of transparency, illumination, perspective, etc., as related to physiological and perceptual behavior. For example, there are three types of space involved in painting: geometric, produced through knowledge of Euclidian geometry; atmospheric, which must reproduce the colors and density and "distance" of the air; and finally, illuminative, which must distinguish "glow," lustrousness, and lighting, as very distince aspects of perception and therefore of painting technique. These in turn are related to the point-by-point relative reflection and absorption of the various parts of picture. As examples of these principles, the author presents illustrations from the various schools of painting.

In the auditory field, an effort is made to establish analagous categories. Thus there are really three comparable acoustic "perspectives" which follow the initial localization of a sound in space. Similarly, analagous acoustic 'glow,' lustre, and lighting are produced "quite obviously" by different musical instruments. Not all of the author's examples in this respect would pass muster by ensemble players, since the fusion of such timbres is quite another thing, perceptually, than are the individual tonal qualities. On safer experimental ground are his very good discussions of interval "quality;" tonality—fortunately including non-European systems; melodic principles; perception of acoustic rhythms, and harmonic 'laws.' In general, the discussion of music is the most satisfying part of the book, since it is rather more restricted to experimental data, and provides excellent illustrations.

In the later chapters, such general esthetic concepts as proportion, contrast, number, symmetry, repetition, economy, and "esthetic geometry" of various lines, patterns, and parts, are further discussed and illustrated from the various fields of art. Finally, the unifying principles which produce the basic "Gesamteindruck" (Gestalten of a higher order) receive both scientific and some speculative treatment.

In terms of critical estimate, the book has the following defects: a forced imbalance between the enormous subject matter and its technical analysis; a tendency toward provincial selection of examples—not always first-class—in the visual arts; and a tendency toward the same introspective dogmatism of which he accuses the philosophers, especially where scientific data are scarce or lacking. On the other hand, the book must be regarded as an important synthesis of the material of the practical arts, the problems of analytical esthetics, and the contributions of physiological, classical, and configurational psychology. It is especially valuable in bringing together the various experimental studies of the Gestalt school and relating them directly to perception of esthetic form. The book is entertainingly written, and at least to those with sufficient scientific and artistic background to fill in the gaps, it is provocatively stimulating.

University of Michigan MARTHA GUERNSEY COLBY

Die Formenwelt des Tastsinnes. By G. RÉVÉSZ. Haag, Martinus Nijhoff, 1938, Vol. I, pp. xii, 291; Vol. II, pp. x, 293.

The theory of haptics embraces the principles of perception and knowledge as well as those determining the creation of forms in the world of tactual impressions —the world of forms as it appears in the spatial consciousness of the blind. Many isolated studies of the psychology of the blind exist, but up to the present time these remained unsystematized and no clear distinction was made between the results of speculation and the findings of empirical research. The consequence was a certain indefiniteness and the presence of many contradictions in the theory of haptics or the science of tactual phenomena, in marked contrast with the consistency and definiteness achieved in the fields of optics and acoustics.

Dr. Geza Révész, formerly professor of psychology at the University of Budapest, and now at the University of Amsterdam, is well known in America for his *Psychology of a Musical Prodigy,* and other studies in the psychology of music. He has devoted many years to psycho-physical researches in vision, audition, and tactual sensation. This has led to a preoccupation with the general problems of space perception and its various modes in man and animals. The present volume on the world of forms experienced by the blind, is a natural outcome of studies of this character and although the author is modest in his claims it will doubtless prove a revelation to most readers. Révész employs a variety of approaches, philosophical, mathematical, biological, and experimental, and discusses the significance of haptic experiences for varied aspects of life, but particularly in art.

Volume I treats of the basic principles of tactual perception and begins with a general discussion of the sciences of space. The author is a nativist and assumes the a priori nature of the intuition of space. An analysis of the relation of optics to haptics and of the latter to geometry introduces us to the theoretical principles of haptics, *i.e.* the tactual perception of form. The chief empirical problem in this field is that of the concrete nature of tactual space and its objects. It appears that there is a complete contrast in the structure of tactual and visual perception. The former is characterized by an analytical emphasis of parts in a whole, while the latter obeys a configurational principle achieving a unity of spatially articulated parts.

Haptic and optic space are the basic forms of space perception and space as perceived by the other senses is always a derivative. Visual, tactual and kinesthetic space, though equally complete in their development are of unequal importance, because they do not yield equal amounts of knowledge concerning the external world.

Haptics may be general or special and in the section on the special haptics of form, the author shows that the perception of tactual forms depends upon ten general principles and tendencies, viz. the steoreoplastic principle, the principle of successive procedure, the kinematic principle, the metric principle, intentional attitude, the tendency toward schematization, the tendency toward transformation, the principle of structural analysis, the principle of constructive synthesis and the principle of autonomous configuration. All of these principles and tendencies are deduced and demonstrated from experiments with normal and blind subjects, the latter again including subjects congenitally blind as well as subjects acquiring blindness at various ages. Experiments and observations on cases of operation for the removal of blindness are included. The second chapter on special haptics of form discusses the knowledge and total experience of proportion and symmetry while the third chapter, deals with varieties of tactual form perception, as determined by the predominance of either the optical or the haptic experience. Révész characterizes such types of tactual form perception as optomorphic or haptomorphic according to the data, dominating their construction. Volume I closes with a brief section on the perceptual world of the blind, in which several approaches to a practical psychology of the blind are suggested and the basic problems for investigation are formulated.

In the second volume of his work Révész develops the esthetic problem of the tactual world. He asserts that this is a virgin field and he offers the first scientific report on the creative achievements of blind sculptors and modelers. This volume which is in some sense an independent work, is richly illustrated and must prove of great interest to all who are concerned with the welfare and education of the blind.

The first part is devoted to the general principles of a tactual esthetic, the difficulties attending esthetic perception on the part of the blind, and the esthetic evaluation of plastic art by means of the tactual and kinesthetic senses. The esthetics of the blind are found to depart from accepted standards, and their plastic creations tend to symbolize emotional urges rather than represent reality as normal individuals perceive it. The second section dealing with the creative activities of the blind, presents comparative experimental studies of the work of the congenitally blind, persons who have developed blindness at various ages, and normal individuals, working with bandaged eyes. There is an interesting account of the creative activities of blind sculptors of the past such as Kleinhans, Vidal, and Masuelli. The volume closes with a chapter on the basic problem of the blind in achieving artistic form, and the significance of sensory limitations in determining types and styles of plastic art. The author declares that a complete and adequate discussion of haptics is beyond the powers of a single individual but expresses the hope that he has laid the foundations and formulated the basal concepts of a genuine psychology of the blind and an explanation of the nature and significance of haptics in the elaboration of our perceptual world.

This hope seems justified, since the volumes certainly represent the first complete organization of the subject and, though occasionally burdened with theory, suggests many new approaches and problems and presents a rich fund of specialized information and excellent bibliographies.

University of Cincinnati CHARLES M. DISERENS

Taste and Temperament. By JOAN EVANS. New York, 1939. Pp. 128, 48 illustrations.

This is a curious little book in which the author applies Jung's well known types *extravert* and *introvert* to the perception of art by layman and artist. Not satisfied with this simple bifurcation, the author proceeds to "subdivide these categories according to the pace of their reaction, and so arrive[s] at a classical quaternity of temperaments" which she calls "slow" and "quick." Useful as Jung's types have proved themselves to be, they are a classification of opposites with a wide ranging middle ground. When to these two types are added the qualifications "slow" and "quick," one is at a loss to know just what is meant. Though the author is at pains to elaborate her characterizations, they remain indefinite because they so obviously overlap.

For instance, the slow extravert is deliberate, persevering, dogmatic, methodical, academic. He is a collector "who in seeking for Rarity falls in love with Rarity for Rareness-sake." He is mathematical, geometrical, favors straight lines and Aristotelian classifications. He is apt to confuse beauty with use. Though courageous, he is haunted by fear and tends toward megalomania and paranoia. He is dynamic, shuns decoration; a moulding or a cornice repels him. His interest in the association of ideas might make it hard for him to believe "that none of this associational process enters into a purely aesthetic sensation." Yet "the existence of 'surrealist art' which is composed of an expression of such associated ideas" complicates the question. "Fortunately," concludes the author, "an art which declares itself without aesthetic intention lies outside the scope of this book." Baroque is an outstanding type of slow extravert art, and the Escorial is a monumental example. Typical artists of this persuasion are Michelangelo, Titian, Holbein, Rembrandt, Delacroix, and Goya.

The author finds somewhat less to say in characterizing the other three types. The quick extravert is feminine, understands the feelings of others, and is exemplified in the actor, salesman, courtier, and the great mystics. All extraverts think of their surroundings as backgrounds, the slow type as autobiography and the quick as *mise en scène*. Color interests them more than form. Dreams and illusions, the supernatural and sentimentality engage them. They are dissatisfied with the world, view and paint it in peculiar perspectives, and their figures levitate. Botticelli, Raphael, Tintoretto, Correggio, El Greco, and Wiliam Blake are cited as quick extraverts. Among the quick introverts Wiliam Morris, John Ruskin, and Sir Joshua Reynolds are cited as exemplars. These theoretical persons are yet said to be genuinely capable of an esthetic emotion, both disinterested and profound. This type favors decoration, landscape, and still life. The compositions are static, simplified, and make much use of receding planes of distance. In contrast to the slow extravert, the slow introvert favors curved rather than straight lines, and in contrast to the quick extravert his figures show weight rather than levitation. The slow introvert is harder to find and to recognize. He is more discrete, more driven to understand what he is doing. His philosophy is Platonic rather than Aristotelian. He is defeatist, given to psychological portraiture and cubic, box-like composition. Hugo van der Goes, Vermeer, Velasquez, Dürer and Cezanne are named to illustrate the type.

A sense of lightness, conveyed by whatever means, among which color would surely be one, appears to characterize all the examples selected as "quick," and a sense of heaviness all those called "slow." Thus, whereas the author's reference of these types of composition to Jung's classification of temperament and "the pace of their reaction" is odd, the esthetic sensibility that accounts for her divisions seems sure. A psychological problem is thus set which deserves further study.

Cornell University R. M. OGDEN

Psychology of Individual Differences. By A. R. GILLILAND and E. L. CLARK. New York, Prentice-Hall, 1939. Pp. xvi, 535.

There are colleges and training schools for educators, social workers, and business administrators, where the curriculum is built to specification according to the "needs" of the students. These needs have been formulated by some eminent educators, who write as if their information came from the Holy Ghost. These institutions demand certain courses, (1) which can be called psychology; (2) which look practical, being unfettered by 'theory' or by logic; (3) which prerequire nothing but semi-literacy of the students; (4) which demand little of the students except recitation of certain words in grammatical order; (5) which are 'certified' by the authorities who license teachers to teach; and (6) which also will be passed by at least 95% of the students who elect them. Such courses demand specially written text-books.

Our authors, however, have sought "to collect in one volume the more important facts and conclusions in the broad field of the psychology of individual differences" (p. v). In their chapter entitled "The problem of individual differences," they actually state no problems for solution. They do assert, repeatedly, and convincingly, that every individual differs in at least some respects, from at least some others. Moreover, these differences are recognized by intelligent individuals, who differ from each other as widely as Aristotle and waterfowl. They assert (pp. 1 f.) that wild ducks, as well as wild geese, fly in V-formation; that the foremost bird is also the leader of the flock in the sense that he has been "selected" probably for his physical prowess, and also in the sense that he sets the course for the rest of the flock. Some ducks, of course, do not fly in this manner. As for the geese, who can tell whether the foremost bird sets the course for others, they for him, or external conditions for them all? I have seen flocks of wild geese change their direction, and at the same time change formation and also "leaders." Of course, as Aesop taught us, we should try to imitate the wisdom of animals, but I am not sure that the geese have set us a perfect example.

As "causes" of individual differences, the authors mention age and sex, but they emphasize heredity and environment. Which of these latter is the more important? They cite authors who have expressed opinions, as if the question were not meaningless, like the question, which side of a rectangle has the more to do in determining its area. They mention C. B. Davenport and L. M. Terman as believing that at least some mental traits of mankind are inherited according to Mendel's law. Why did they not examine this belief? Mendel's law is limited in its scope. It presupposes two incompatible traits, unequivocally defined, and practically discernible. It presupposes the mating of two individuals from "pure stocks." It presupposes the mating of brothers with sisters in the first filial generation. It is, therefore, quite irrelevant to censuses of Kallikaks, Jukes, and Edwardses.

Why, also, did they not question the tradition about Jonathan Edwards? He was married 212 years ago, and had 12 legitimate children. By conservative rules of guessing, how many descendants may we suppose he has had by this time? How many of them, probably are old enough, or have been old enough, to have attained distinction? How many of them have been identified? Does the number who have been identified make up an abnormally large part of the probable number of adults? And are not those who are distinguished (for good or bad) more likely to be identified than the mediocre?

Again, consider a member of the 6th filial generation: he has 64 ancestors of the degree of Jonathan Edwards. Suppose he himself is eminent, and that one of his ancestors in the 6th degree is Jonathan, while the other 63 are not. Why regard Jonathan Edwards as a sufficient cause of his greatness, and ignore the other ancestors? It seems to me that somebody ought to analyze and weigh the premises in the eugenists' propaganda, instead of re-patterning them to uncritical undergraduate students.

In their chapter on the measurement of differences, the authors leave undefined certain statistical concepts, such as standard deviation and correlation, which underlie much that follows. They pay due ritualistic honor to the normal distribution, and give roughly the same misinformation about it as mental testers usually give. "Long ago," they tell us (p. 68), "it was observed that height and many other linear measurements of the body, if collected from large groups of men, would *tend to fall under* a normal curve" (italics mine). On pp. 112 f. they present 26 distributions of heights and 26 distributions of weights among 10,560 children classified by Bird T. Baldwin according to their ages. On p. 114 they say that "for the most part, the curves conform fairly closely to a normal distribution." From mere inspection, I doubt that this holds true. For amusement, Mr. Martin Kaplon and I have tested the normality of some of the other distributions which they present, by computing the normal distributions which have the same parameters as the actual, and by the X^2-criterion, finding the probability P that the misfit in each instance would occur by chance. One can indeed describe the distributions of the test-scores of 903 students (p. 64) and also the weights of Montague and Hollingworth's 1000 boys and 1000 girls at birth, within a tolerable error, by mentioning the parameters of the corresponding normal distributions. Incidentally, the tabular exhibit of these weights does not agree with the corresponding graphs on the same page (111); neither do the tables yield exactly the average weights that the authors mention. But our authors show some other distributions which one could not plausibly call normal. For the heights of Montague and Hollingworth's new-born boys (p. 110), the original data yield $P = 10^{-3}$; and for the girls, $P = 10^{-5}$. They might have mentioned that Boring (*Science,* 52, 1920, 464 ff.) reported in 1920 that the heights of 221,819 insured men misfit a normal distribution, P being about 10^{-68}. On p. 115, they show graphically Davenport and Love's distributions of the heights of "the first million" (actually 868,445) American soldiers drafted in the World War, and also of the same soldiers' weights. From the original data, we find for heights $P = (10)^{-1423}$; for weights, $P = (10)^{-20780}$, in which the exponent is uncertain by at least 5. One recalls, of course, the exhibit which Yule reproduced in many editions of his monumental work, of the heights of 8585 men from the British Isles, fitted beautifully to a normal curve. This distribution, however, is *not* homogeneous, being made up of four components, namely English, Scotch, Irish and Welsh, which, according to the X^2-criterion, cannot be plausibly regarded as having been drawn from the same parent population. Moreover, their numerical strengths are *not* proportional to the populations of the political units from which they are drawn. Thus, it is *not* generally true that the heights of large samples of men are normally distributed, or nearly so.

Earlier (p. 69) the authors suggest that a mental test should be rejected if its scores are not distributed normally, although it should not be accepted merely because they are. Some mathematicians find such assertions appalling. On p. 391, we are told that the Moore-Gilliland Aggressiveness Test has a "reliability" of 0.75, but a "validity" in estimating ratings of 0.90. According to some educational statisticians, this assertion implies that the test-scores "measure" something else better than they measure themselves.

This test and certain others are, according to the authors, indirect and imperfect measures, but nevertheless useful measures, of existing traits. But there *cannot be* such a trait as General Aggressiveness, such that its measure can be denoted by a single number for a particular individual, indicating "how much" aggressiveness he most probably will manifest in any unspecified situation. "To aggress" is a transitive verb; it is meaningless without an unequivocally defined object. A similar assertion holds true for such fictitious traits as General Intelligence, General Persistence, General Gullibility, and the like.

On pp. 88 f. the authors marvel that the so-called mental ages of children younger than 15 yr. are simply proportional to their chronological ages. The secret is, of course, that the testers "planned it that way." They selected their test questions so that the average mental age of the children in their "standard" population would be exactly equal to their chronological ages, as nearly as they could accomplish that feat; hence, the average of the intelligence-quotients of the standard children of any age has to be exactly 100. Had the authors exhibited the defining operations of mental age and intelligence-quotient, neither they nor their readers need to have been mystified.

Without searching for misprints and errors in citation, one finds a good many in this book. On p. 252, the authors speak of "tetrachloric" correlation. On p. 6, they misquote the Declaration of Independence, call E. H. Weber a psychologist, cite Fechner's *Elemente der 'Psycho-Physik,'* and specify 50% instead of 84% correct responses as the criterion of threshold-discrimination according to Fechner's method of right and wrong answers. They also cite Thurstone, L. L., and *Thelma Gwinn* (p. 18), but Thurstone, L. L., and Thurstone, T. G. on p. 371; they mention Gall and *Spurtzheim* twice on p. 120; Henry *F.* Garrett (p. 95); A. A. *Robach* (pp. 356, 396); *Lela* S. Hollingworth (p. 313); Mohr and *Grundlach* (pp. 344, 348); *J. J. B.* Watson (pp. 26, 57). They ascribe a certain monograph to May and Hartshorne on p. 314, but to Hartshorne and May on p. 396. Perhaps one should not cavil at details, but if they are numerous, one wonders if they may not be symptomatic.

My chief interest lies in the principles according to which the factual material is organized. From what I have said it should be clear that they are at least debatable.

For such courses as I described at the beginning of this note, this book may gain many "adoptions" immediately. If it has other uses, I do not now identify them.

Tulane University H. M. JOHNSON

Logical Aspects of Educational Measurement. By B. OTHANEL SMITH. New York, Columbia University Press, 1938. Pp. vii, 182.

This book should be required reading for every student of psychophysics, psychometrics, and educational measurements. It is disturbing, and it is therefore important. The reviewer disagrees with practically every important conclusion which the author draws, but on trying to find specific flaws in the arguments advanced, and in trying to establish his own views on a rigorous logical basis, he finds extraordinary difficulty. Dr. Smith has raised important fundamental questions. His answers are unsatisfactory. So are the answers attempted by the present reviewer! The questions, therefore, remain, and they are important and fundamental.

The author's argument, briefly, is as follows. Measurement implies the application—and the applicability—of certain elementary mathematical operations to data. The data must be shown experimentally to lie on a qualitative continuum and to exhibit properties corresponding to the postulates of the mathematics before measurement is proved possible. The postulates in question are those of *order, equality,* and *addition.* Fundamental measurement is possible if all three sets of postulates are met. Derived measurement is possible if the postulates of order and equality, but not those of addition, are met. Educational tests attempt to measure learning outcomes. These do not lie on a qualitative continuum. Learning integrations involve more than the sum of the elementary experiences out of which they are built. Two students who achieve the same score by answering different items on a test are not thereby shown to possess equal learnings, even after allowing for errors of measurement in the usual statistical sense. The nature of the interactions among experiences and learning outcomes renders extremely dubious the process of assigning to an individual a total score consisting merely of the *sum* of his item-scores. The normal distribution has never been adequately demonstrated to apply to mental traits, and normal curve scaling has not been shown to produce equal-unit scales. Progress in measurement is to be looked for in two directions: (1) through factor analysis methods, which *may* succeed in isolating simple, abstract, linear aspects of the complex of learning outcomes; (2) through a more careful study of "real learning outcomes," leading to the possible development of complex measuring instruments, and scoring schemes which actually parallel in some sense the organization of these "real learning outcomes."

It is impossible in the space of a short review to criticize the book adequately, and, as noted above, the reviewer is not yet satisfied with his own criticisms. Only a few general comments will, therefore, be made.

In the first place, it is doubtful if test builders have as strong a belief that they are measuring "real learning outcomes" as the author implies. They assume, more nearly, that they are measuring *one aspect* of these outcomes, which is correlated positively but imperfectly with the other important aspects. Test experts are among the first to insist that the validities of their instruments are lower than the corresponding reliabilities, but that does not mean that these validities are so low as to render the instruments valueless. On the contrary, the universally positive correlation between test-scores and competent subjective estimates of "real learning outcomes" provides some demonstration of the validities of tests.

In his discussion of learning, the author makes much of the fact that "real learnings," which affect the self, are to be distinguished from superficial learnings and conditioned reflexes, which do not. He mentions but does not stress the fact that these latter are forgotten much more rapidly, and implies that many tests tap primarily these superficial and rote learnings. This would be true only if each test were given immediately following instruction, and if the items called primarily for reproduction of the formal elements. By using items which require discrimination and reasoned understanding of the facts, and by collecting these into comprehensive examinations which are given some time after the formal instruction is over, a great part of the criticism is met. This is precisely what the test experts have been advocating—and to some extent practicing—for a decade or more.

In his treatment of the foundations of educational measurement in general, and of the normal distribution in particular, the author exhibits unfamiliarity with certain fundamental statistical principles not found in the elementary texts. He seems, in particular, not to know the Taylor-series argument for the first-approximation validity of linear combinations of test items, nor the fact that the normal distribution has been derived without the assumption of equal, all-or-none, additively-combining elements. A great many of his criticisms lose some of their force in the light of these mathematical demonstrations.

In spite of shortcomings in the analysis and solution of the problems raised, however, this book should be read, because it is the first, so far as the reviewer knows, which states these problems adequately, or even fully recognizes their existence.

American Council on Education EDWARD E. CURETON

A History of Science, Technology, and Philosophy in the Eighteenth Century. By A. WOLF. New York, Macmillan Co., 1939. Pp. 814; 345 figs. and plates.

The present volume—which after four years follows its excellent predecessor for the sixteenth and seventeenth centuries (reviewed in this JOURNAL, 48, 1936, 695 f.)

—maintains the standard, style and arrangement of the first volume. Science is the chief subject-matter; technology is important but secondary; philosophy is included as a courtesy to a relative of science. Thus we have in this book: Science—mathematics, mechanics, astronomy, physics, meteorology, chemistry, geology, geography, botany, zoology, medicine; Technology—agriculture, textiles, building, transport, power plants, machinery, steam-engines, mining and metallurgy, industrial chemistry, lenses and specula, mechanical calculators, telegraphy; Philosophy—psychology, social science, economics, philosophy proper.

This volume, like the first, holds to a golden mean between that brevity which lists names, dates and events and those long expositions which are thorough at the expense of something else. The treatment is not so brief but that it provides insight into the development of discovery and thought. Thus for the most part it becomes unreasonable for the reader to complain about omissions, since any addition would necessitate either the deletion of some other item or the writing of a longer book—and surely it is a reasonable undertaking for Professor Wolf to try to write a book on eighteenth century science and thought in not more than 772 pages.

As in the first book, so here there is a short chapter toward the end called *Psychology.* It is twenty-seven pages long and deals with Berkeley, Hume and Hartley for several pages each, then briefly with Diderot, Condillac, Bonnet and Cabanis, then still more briefly with Tetens, Wolff, Kant, and Moses Mendelssohn. It spends a page on Pereire's teaching of deaf-mutes. The chapter is clear and for the most part too elementary for psychologists.

Is there any psychology to be found under *Physiology?* No, there are only four pages on physiology: Haller, Réaumur, Spallanzani, and Hales. The beginnings of reflex action with Robert Whytt (1751) are not mentioned, nor his anticipation by Astruc (1736). That was a trivial affair in eighteenth century science, but what is Professor Wolf going to do about a background for reflex action when he comes to the nineteenth century?

Under the physics of light we get accounts of the conflict of the corpuscular theory with the wave theory, of Euler's contribution to that problem, of Bouguer and Lambert on photometry, of Robert Smith's *Compleat System of Opticks* (1738). The psychologist might have been interested in being told further that Bouguer in his photometry anticipated Weber's Law, that Lambert invented a color pyramid as well as a photometer, that the first certain case of color blindness was described by Huddart in 1777 and that Dalton's famous color blindness, which gave rise later to the term *daltonism,* was described in 1798. There is also the large literature on after-images and contrast for which Plateau cited in 1878 more than a hundred papers dated before 1800. The more important names here are Buffon, Benjamin Franklin and Robert W. Darwin. These are omissions that the psychologist would have liked to have supplied, but I can not tell Professor Wolf what else he should have omitted in order to let these in.

The physics of sound is very briefly treated: Sauveur on beats and consequently on the calculation of frequencies; Euler on the frequency formula for strings; Sorge, Romieu and Tartini on combination tones; Chladni on his vibration figures and the velocity of sound; Priestley on the effects of different media on sound. Sauveur's limits of audibility are given as 12½ and 6400, Euler's as 20 and 4000. I do not think that there are important omissions, although the invention of the tuning fork by the bandmaster, Shore, is interesting (1711).

As a matter of fact there really was very little scientific 'psychology' during the eighteenth century, in spite of Professor Wolf's saying: "Eighteenth century psychology showed a marked tendency toward independence as an empirical science. A number of works were written on various psychological subjects without any admixture of philosophical or theological discussions" (p. 668). He is speaking here of Berkeley's *New Theory of Vision* and of Hume's *Treatise,* and certainly these authors were more empirical than many of their philosophical colleagues. Nevertheless their contributions lack the faith in experiment and observation that characterized Newton's discovery of the complexity of white light, or, for that matter, Benjamin Franklin's account of how negative after-images ("spectra") give place to positive when the eyes are closed. All of which is not to say that Professor Wolf has not used his great skill and wisdom in the completion of another masterpiece of erudition.

Harvard University E. G. BORING

Gemeinschaft und Herrschaft als Staats- und Kulturtypen. Ein kulturanthropologischer Versuch. By PETER VON WERDER. Stuttgart, Ferdinand Enke, 1938. Pp. viii, 150.

It can be argued that the rich anthropological materials have not been sufficiently utilized for sociological theory, and as far as the author purports to use them for sociological typology one would welcome his attempt, though he tends to identify too readily the social conditions among the contemporary "primitives" with the "early" form of human societies (pp. 3 ff.). It is more than doubtful, however, whether the two concepts of socio-political organization thus derived—"gerontocratic community" and "patriarchal domination"—can be considered as sufficient for the analysis and interpretation even of those "early" associational formations. The inadequacy of the conceptual equipment of the author becomes still more apparent when more complete and infinitely variable historical and contemporary forms of dominational associations are approached (pp. 41 ff.).

The author, however, credits his two fundamental types of political domination with the ability to serve the interpretation not only of the legal forms, administrative systems, and military organization of any social group, but even of the whole range of social activities, art, science, and so forth (pp. 98 ff.).

Quite arbitrarily the two basic types of political organization (gerontocratic community and patriarchal domination) are thus reduced to two "types of family" and, in the last resort, both are related to racial types as expressive of their "natural" tendencies (pp. 11 ff.), though the author himself seems to waver somewhat in taking this unsubstantiated position.

In every concrete society the author sees either gerontocratic community or patriarchal domination or a mixture of the two. Their "harmonious mixture" is the "ideal" political form, only rarely actualized, and it goes without saying that the author finds it, among a few other cases such as the Athens of Perikles and the *Preussischer Ordensstaat,* in contemporary Germany.

The two types are not clear, precise, and unambiguous in themselves, though this is the first requirement of an 'ideal-type.' The curious thing, however, is that the negatively evaluated form of "patriarchal domination" seems to be approximated by the reality of the German situation much more closely than "gerontocracy" or an "ideal mixture" of both.

No doubt, the author is a gifted young man. He has intuition, constructive imagination, and happy *apercus* which could serve as useful hypotheses. He makes some good points, for instance in his sociology of art and knowledge (pp. 88 ff.), but good ideas remain without elaboration or substantiation and are presented as dogmatic assertions.

The book is a classic example of the disastrous blinding effects that a political ideology produces on scientific theory and research. Nothing could be recommended to the author more urgently, in his own interest as a young scholar, than to free himself from these ideological bonds, to look around him for sober empirical facts, and for more useful theoretical typologies in the field of political sociology. In the work of Franz Oppenheimer, Max Weber, Georg Simmel, Tönnies, Pareto, Mosca, MacIver, and others, there is available nowadays a considerable wealth of historically well substantiated and serviceable constructions.

Columbia University A. VON SCHELTING

Psychological Factors in Marital Happiness. By LEWIS M. TERMAN. New York, McGraw-Hill Book Co., 1938. Pp. xiv, 474.

The problems of marital incompatibility, domestic felicity, and conjugal adjustment are obviously psychological but they are not problems which have been much studied nor are they problems which are easy to attack. Because psychologists are so often called on for advice (even teaching) in these matters any study, especially one by a person so competent, is peculiarly welcome. The present volume reports the results of an effort to break into this difficult field. It is, however, essentially a pioneer research monograph. The marriage counsellor and the person involved in marital difficulties will not find much in the book to assist him.

Obviously the first thing to do was to measure degrees of marital happiness. For this a device was developed which is a combination of the rating scale and the questionary. With this in hand it was possible to correlate degrees of marital happiness with scores on personality measures and to compare in some of the various possible ways groups of happily married with the unhappily married. Both of these procedures were used, but in the main study on 792 married pairs, measures of personality traits were not obtained. The technique was to select from the Bernreuter and the Strong tests only certain questions and to add to these many others which the investigators thought desirable. However effective this method, it unfortunately ruled out the possibility of discovering the significances of the various possible Bernreuter and Strong scores. Perhaps this was done because a preliminary study did not reveal much significance in such results. The author and his associates argue that the sex relation is actually but a small part of married life; nevertheless their list of questions includes many items concerning sex practices. Much of the book involves the presentation of how the groups differ in terms of answers to specific questions, although one may properly question the reliability of information obtained by a single question on any subject.

The results are on the whole not especially startling. The happily married group were found to have had happier childhoods and happier parents, to have been subject to less severe discipline, to have had less conflict in adolescence with their own parents, in temperament they were more conformative and in reference to family size to have been a little less often 'only' children. It was just a little surprising to learn that comparisons of the happily married with the unhappily married did not show differences in the amount of education as a matter of much significance. Nor were differences in age a matter of much importance. In general it might be said in summary of many items that the happily married were of the more conservative variety of people.

Data from so large a number of adults would be valuable under almost any circumstances, but data so carefully obtained as these are genuinely worthy of very serious attention. A very clever plan was devised whereby husband and wife wrote their answers without possibility of influencing each other and then by another device which continued to maintain anonymity the returns of husband and wife were kept together without anyone knowing the identity of the respondents.

Doubtless the author and his associates would be the first to agree that the problems of marital happiness have not been solved by this study. Certainly no one could do much counselling in terms of what has been found here for the obvious reasons that these results are presented in averages. It may be quite true that a difference in amount of education may not be on the average a significant matter in the prediction of marriage success but it is quite possible that in an individual instance the degrees of education might be very influential. The same may of course be said of many other items included. The very important "will-to-coöperate" was not measured and is probably of prime importance. Perhaps the recollections of childhood reported are not valid. Such recollections are notoriously unreliable and here they may have been colored by the present degree of marital happiness or unhappiness. Any reader may feel that the measure of marital happiness used does not measure what the reader thinks of as marital happiness but it is a carefully prepared device and readers have only their own feelings to go by.

It is further possible that some of the background items stressed are important because of the traitis of personality produced by them and that these traits of personality should have been studied. It seems to the reviewer quite unfortunate that the methods prevented the determination of Bernreuter and Strong scores in the major study. But such things are easy to think of afterward. That the subjects for the study were groups of persons living in California and therefore perhaps not typical, the author readily admits. Obviously what is needed now is a follow-up study which shall sample populations far and wide over the country and by still better methods than those used for this. And, if possible, still other traits should be measured with testing begun long before the initiation of the marriage venture.

Indiana University EDMUND S. CONKLIN

The Happy Family. By JOHN LEVY and RUTH MUNROE. New York, Alfred A. Knopf, 1938. Pp. 319.

This is an application of common-sense psychiatry to making happy families. It is not an over-simplified or diluted psychiatry, but one which sees the intricacies of family relationships, yet has large trust in wholesome living and common sense as over against the application of elaborate psychiatric theories.

Dr. Levy, who seems chiefly responsible for the writing, was an associate professor of psychiatry in the College of Physicians and Surgeons, Columbia University, and died just before the book was published; Dr. Munroe, his wife, was formerly professor of psychology in Sarah Lawrence College. The book is delightfully written in a conversational style, with a fine dash of humor, and woven around composite case studies. Some of the ethics might seem dubious to the more conservative, but they are entirely wholesome.

There is probably nothing very new in the book for the psychologist or psychiatrist familiar with the literature of the family, but there is a profound wisdom in the book which will be challenging to the best professionals in this field.

The authors stress the "unconscious motivation" involved in the relations between husband and wife and parents and children, and the need for their recognition. The character of the person rather than the immediate circumstances are of most importance in controlling his behavior. Marriage problems have two facets: the real difficulty and the psychological meaning of that difficulty for the person involved. Without minimizing the importance of sex adjustment, they feel that it has been overemphasized, and that it is not so important a cause of marital maladjustment as many have claimed. They consider the need for love and self-fulfilment as the chief drives in marital relationships, but point out that the desire for dominance and protection alternate in every personality and are not uniform in time.

The common-sense point of view of the book is shown in the following summary statement with regard to infidelity: "Married people will do better, I think, to follow the course of procedure that seems to them individually natural and right rather than to base their decision on elaborate theorizing" (p. 100).

This book is a good antidote for over-sophisticated or over-conscientious parents who have been unable to digest their reading of current literature; is good reading for every student of the family; and is recommended for psychiatrists who take themselves too seriously.

Cornell University DWIGHT SANDERSON

The Sex Criminal. By BERTRAM POLLENS. New York, Macaulay Co., 1938. Pp. 211.

Bertram Pollens is the senior psychologist of the Penitentiary of the City of New York at Rikers Island, where a "Sex Clinic" has been established to study intensively the 700 cases which are classified as sex offenders out of the total of 18,000 commitments each year. According to Richard A. McGee, Warden at the Penitentiary, "This book is the first well organized, authoritative, and readable effort to bring the subject up to date for the general reader."

Pollens emphasizes throughout the book two points: (1) That the traditional legalistic attitudes, which disregard the psychological antecedents, should be discarded; and (2) that the sex criminal is characterized generally by his emotional immaturity. On the first issue, society is taken to task for its punitive rather than therapeutic attitude toward the sex criminal. On the second, Freud's convenient psychic compartments (Id, Ego, and Super-Ego) are uncritically accepted and introduced as explanatory concepts.

Some of the topics discussed in the book are: "Infantile Sexuality," "Homosexuality," "Is the Sex Deviate Born or Made?," "Is the Sex Criminal Insane?" The concluding chapters deal with the disposition of the sex criminal and advice to parents. Some interesting conclusions are put forth in the chapter entitled "Sex Criminals as a Group," for example, that most sex criminals are recidivists, that there is no real increase in sex crimes, that in a group of sex criminals studied in New York 83% of them were over 30 yr. of age, and that although sex criminals are not as a group classifiable as feebleminded they are usually of below-average intelligence. Pollens suggests that the only proper method of dealing with the sex criminal at present is to segregate him from the community, to place him in an institution which is neither penal nor correctional, so that he may be supervised, studied, guided, and, if possible, treated. The institution should be in the nature of a mental hospital rather than a jail and psychiatrists and psychologists should have the responsibility of supervision and treatment. The book is replete with case studies, many of them of recent tabloid notoriety. Only the male offender is considered.

The book will probably prove of interest to some laymen and may also have value to parole officers, social workers, and institution authorities. Its point of departure is sociological rather than psychological. The psychological substance of the book is on an elementary level, as might be expected from the fact that it is intended for popular consumption. The emphasis is expository rather than critical. It is distinctly not a contribution to science. It is filled with localisms and slang, sometimes in poor taste. The author is unguarded in proposing generalizations as truth in the absence of experimental substantiation, as witness the following statement: "Scientists, who specialize in microscopic work have substantiated their peeping instinct, while physicians who derive pleasure from the practice of surgery have degenitalized their sadistic trends."

Although the book contains information of value to some readers, its acceptance by psychologists is limited by the lack of verbal precision. Careless proof-reading and lack of adequate organization are added handicaps to its usefulness. A greater degree of scientific objectivity and impartiality, even in a popular treatise, might be expected from a psychologist. Further revisions prior to publication were in order.

Rochester, New York ANTHONY J. MITRANO

The Behavior of Organisms: An Experimental Analysis. By B. F. SKINNER. New York, D. Appleton-Century Co., 1938. Pp. ix, 458.

In this book Dr. Skinner attempts the construction of a positivistic, descriptive system of behavior. There are no hypotheses, the various terms used in the exposition being non-explanatory, and merely descriptive of carefully defined modes of behavior. New terms are devised, not through any desire for the esoteric, but simply because common terms imply a *conceptual* system, and are thus more than purely descriptive. It is interesting to note here the departure from those writers who use old terms, but with new definitions.

In the study of behavior, 'running accounts' are not desired, since they are difficult of quantification. Rather, the basic behavioral unit is the "reflex," which is defined as a given environmental circumstance and a reliable response. Thus, there is a "lawful connection" between stimulus and response, but there is no attempt at the prediction and control of behavior, since this implies "botanizing," or the collection of reflexes. Rather, the attempt is to work out the general characteristics of all behavior. A further interesting point is the author's dichotomy between "respondant" behavior, which is directly elicited by an environmental situation, and "operant" behavior, which seems spontaneously emitted by the organism. He points out that the attempt to force behavior into a stimulus-response explanatory system has delayed its adequate scientific treatment.

After a list and discussion of "dynamic" and "static" laws of the reflex, the bulk of the book is given over to the presentation of the results of several years' experimental work by the author upon behavior in the rat. A standard situation, in which the animal by pushing upon a lever could obtain a pellet of food, was used throughout. This is not the place to summarize Skinner's results, so suffice it to say that an extreme degree of careful control was used, and the results present a remarkable degree of consistency. There is no attempt to be exhaustive—that is, not *all* drives, for example, were considered. Instead, the author attempted to arrive at generalizations adequate for drives in general, and for organisms in general.

In the penultimate chapter, it is pointed out that neurology is not appealed to. This is done for four reasons. (1) Neurology is not actually more orderly than behavior. (2) Neurological data do not 'explain away' behavior. (3) More actual data concerning the nervous system are needed. (4) A precise behavioral description is needed before neurological correlates can be demonstrated. Furthermore, Skinner points out that 'CNS' should be read 'conceptual nervous system' in such a system as Pavlov's, since hypothetical centers and connections are used as explanatory media.

In the final chapter, the systematic approach is summarized. The system is mechanistic and analytical, each of which terms the author stoutly defends. The emphasis is upon quantitative data. Further, operant behavior is pointed out as being most important, since this makes up the bulk of adult behavior. Finally, there is no attempted extension to human behavior, although Skinner sees no differences, except in complexity and verbalization.

The reader of Skinner's book is left with the impression that, whether he agrees or not, the author has done his job well. The exposition is very clear and, in the reviewer's opinion, unambiguous. By anyone who feels that the future of comparative psychology, especially, lies in the greater objectification of its descriptive categories, this work should receive a ready welcome. The emphasis upon operant rather than respondant behavior should be hailed by all who object to the forcing of behavior into falsely simplified explanatory systems. Perhaps the specific laws will be modified with the extension of research, but surely the basic position should always deserve respectful consideration.

University of Alabama F. NOWELL JONES

Mental Therapy: Studies in Fifty Cases. By LOUIS S. LONDON. Vols. I and II. New York, Covici-Friede, 1937. Pp. viii, 774.

The author makes excellent use of the case method to show the relation between the neuroses and psychoses and to illustrate the technique of psychoanalysis in psychiatry. The neuroses, or psychoneuroses, including the paraphilias (perversions), are classed as minor psychoses "to differentiate them from the psychoses proper, often referred to as major psychoses." The 50 cases are drawn from the author's own practice and include a wide range in the field of psychopathology. The narrative form of these case reports makes possible for the reader a sense of progress in each analysis. He sees not only the patient and his symptoms, but also the analyst at work.

The author's interest is primarily that of the psychiatrist. He follows Stekel, with whom he worked in Vienna in 1932, in asking, "What can psychoanalysis and my experience do for the patient?," rather than asking with Freud, "What does the case bring for science?" In general, the author agrees with Stekel in both theory and practice. In Chapter II he gives a brief survey of trends in psychoanalysis. He concludes that "the two schools, the orthodox Freudian school and the Stekelian, which had been diverging for the past two decades, are now approximating."

Part V reports cases of schizophrenia and paranoia; Part VI, cases of manic-depressive, or cyclic, psychoses. In Case 39, the author reports a favorable outcome in an advanced case of schizophrenia after four years of psychoanalysis. "Not only," he concludes, "has he (the patient) made a satisfactory adjustment to heterosexuality, but he has resumed his studies and will eventually study commercial chemistry. Nevertheless, there is still a question as to the practicability of analyzing cases of dementia praecox." He is equally cautious about exaggerating claims for psychoanalysis in the case of manic depressives, but urges further exploratory analysis in both of these major psychoses as likely to throw light on psychogenesis and etiology.

Psychologists will find these case studies valuable source material in abnormal psychology, and particularly in the psychopathology of sex. Part III includes 17 cases of paraphiliac neurosis, or sex perversion, and there are examples of paraphiliac trends or sexual maladjustments in a majority of the other cases reported.

The implications of these case studies for a program of mental hygiene which includes prevention as well as therapy are not discussed by the author, but should be pondered by any reader who is interested in education, in the applied fields of psychology, in court or welfare work, or in public health, as well as by the psychiatrist. One is impressed by the geometric ratios involved in social contacts. Each of these cases is reported as an individual. Yet each of these individuals had significant contact with many others. If Case 10 indulged as a child in sex play with three, four, or five other children, these three, four, or five presumably each had similar experiences with others. In the same way there is a widening circle in the case of the perversions. It is seldom that they are passed on by one individual merely to a single partner.

There is also in these considerations an implication for the psychiatrist himself in the matter of social responsibility. There is the analyst-patient relationship, as in the treatment of disease by the physician, but there is also a public mental hygiene responsibility. The physician recognizes a public health responsibility when he reports a communicable disease and quarantines his patient. How can the psychiatrist show himself equally responsible?

Dr. London closes his second volume with a glossary. It is easy to criticize a definition, but often hard to write one. Psychologists will be likely to criticize the following unfavorably: "*Conscience:* That part of our mental faculty which deals with the moral sense of right and wrong." "*Hypnotism:* A form of therapeutics where a person is forced into an unconscious state by command of the therapist."

Ohio State University FRANCIS N. MAXFIELD

Hearing: Its Psychology and Physiology. By S. S. STEVENS and H. DAVIS. New York, Wiley, 1938. Pp. xv, 489.

The authors attempt to present an inventory of the recent discoveries in the psychophysiology of hearing, and to determine to what extent auditory research is able to yield a consistent point of view. Of the 330 titles listed in the bibliography, 280 have dates of publication more recent than 1927. No attempt is made to orient the respective topics historically. Other deliberate omissions are the fields of applied acoustics, speech, phonetics, noise and music. The book is divided logically into two sections: the first nine chapters deal with the psychology and the last nine sections with the neurophysiology of hearing.

The first part of the book begins with an analysis of the nature of the auditory stimulus and is followed by a treatment of pitch, loudness, volume, density, localization, harmonics, fatigue, vibrato and beats. The second part is concerned with an elementary presentation of the anatomy of the ear, following which is a short chapter on bone conduction and deafness. Preceded by a section on neurophysiological principles, the next two important chapters deal with aural microphonics ("cochlear effect") and the rôle of the basilar membrane. In the authors' opinion, the best explanation at the present time is some form of a "place" theory. The concluding three chapters deal with the nerve impulses in the peripheral and central acoustical pathways. Much significant material concerning the relationships between the cochlear effect and true auditory potentials is presented and constitutes one of the more important sections of the book.

To some extent, criticism of the choice of references, amount of discussion allocated to the various topics, etc., depends upon the predilections of the particular reviewer. In general, the studies surveyed are good, although one could argue for the inclusion of a number of recent important studies which have been omitted. From a student's point of view, reference is made to the work of some 12 investigators for which no bibliographical sources are given. There are also about 21 citations in the list of References (of which 17 were reported since 1927) which are never mentioned. Since a source-book was not the objective, these references should have either been omitted or else discussed in the text. In several instances, experimental studies are improperly cited.

What of the book as a whole, the avowed purpose of which was an inventory of recent discoveries? If the volume is accepted as an expression of the work and viewpoint of the Harvard Laboratories, it does accomplish its objective. This does not mean that only studies from this laboratory are discussed, but that the Harvard data have been emphasized. As such, a contribution has been made which no serious student of the field can overlook.

Often the discussion consists simply of an abstract of a particular experiment, the value of which would have been materially enhanced if the treatment had been more critical. For example, the analysis of auditory localization seems to be quite weak. While comparisons are perhaps invidious, the same topic treated by Woodworth, (*Experimental Psychology,* 1938, 518-534) is a more scholarly and organized presentation. While sufficient time has elapsed since the first studies of cochlear microphonics in 1930, the results of many investigations do not seem to have clarified several vexing problems. Speculation is offered in the interpretation of specific phenomena, but this did not go far enough. Since the purpose was to "determine to what extent auditory research is able to yield a consistent point of view" one can only conclude that either the authors missed their goal badly, or else a coördinated account is not possible at the present time. They state that "theories flourish on a certain sparseness of facts and wither in the face of abundance" (p. x). One can take issue with this remark or not, but one good hypothesis is worth a dozen uncoördinated facts.

The decade initiated by the report of Wever and Bray in 1930 has brought about a remarkable development of our knowledge of the electrical phenomena of the peripheral acoustical mechanisms. Our understanding of the central acoustical system has not progressed at the same pace. Anatomically there is the work of Nó, Poliak and Walker; studies of the electrical responses of the brain stem by Kemp et al.;

functional studies of the medial geniculate bodies by Ades, and that of the component tracts by Culler and his associates. Beside the studies of Kornmuller in 1937 and Pfeifer in 1921 on cortical electrical functions, this about covers the pertinent experimental data on the central acoustical system. It is possible that further advance in our knowledge awaits the acquisition of more data on the cortical auditory pathways and their rôle in hearing.

From a pedagogical point of view, this book is neither an introductory text nor a handbook. In the former capacity, several sections are far in advance of anything now available. The treatment of the properties of the auditory stimulus, the electrical techniques and instrumentation should be of considerable value to the student. A clearcut distinction between the properties of the stimulus and the psychological attributes is very ably presented. The relationship between intensity and frequency in determining both pitch and loudness is likewise handled very well. The student will also find a good statement of the neurophysiological principles and their significance for hearing. Many sections are far beyond the introductory student. It also follows, that some sections are too simple for the scholar. That the book is not a handbook is not intended as a criticism, since this objective was clearly not the authors' goal. However, as stated in the introductory *Perspective* by one of the dedicatées, "Certainly we are now ready for a new *Lehre von den Tonemfindungen* to orient us" (p. vii).

Brooklyn College EDWARD GIRDEN

Elements of Psychology: An Introduction. By ALBERT CLAYTON REID. New York, Prentice-Hall, 1938. Pp. xix, 409.

To write a textbook of psychology from a frankly elementaristic point of view requires nowadays considerable courage. For this reason, if for no other, Professor Reid is to be complimented. He has adhered loyally and openly to the tradition of the late Professor Titchener. In the arrangement of chapters, even in the arrangement of topics within chapters, he has deviated only slightly from the plan of Titchener's *Textbook of Psychology;* and his style is characterized by a directness and lucidity which shows the influence of the Titchenerian model. The student should find the book clear, interesting and not too difficult.

The reaction of the professional psychologist to the book will probably be one of disappointment. It is like an echo from the past. One gains the impression, not that the psychology of the past twenty years is insignificant, but that the past twenty years have never really existed. Much of the book might, in fact, have been written in the days of Helmholtz or Ebbinghaus. One cannot quarrel with a writer for adhering to a conservative point of view. One even has a certain lurking respect for an experimentalist who finds little dignity in chapters on motivation, personality and the testing of intelligence. One must insist, however, that even an introductory textbook pay at least some attention to the current research in the field which it purports to represent. A psychology of elements still constitutes, in theory at least, a defensible point of view. It has been vigorously challenged both on theoretical and on experimental grounds. It is a pity that Professor Reid, as one of the few remaining Titchenerians, has not attempted to meet that challenge.

Swarthmore College R. B. MACLEOD

You Can Sleep Well. By EDMUND JACOBSON. New York, McGraw-Hill Book Co., 1938. Pp. xix, 269.

Those who are acquainted with Jacobson's earlier publications on progressive relaxation will not be surprised to find that the present volume contains a popularized extension of his program to the induction of sleep. The author has gained in literary fluency with the passage of time; his device of making "The Doctor" the protagonist who conducts conversations with the reader definitely aids in maintaining interest in his exposition. On the systematic side, one senses a definite change on only one point; namely, that Jacobson is now apparently convinced that relaxation can be learned in somewhat less intensive fashion than he had previously maintained.

The lay reader will probably most enjoy the earlier chapters with their fluent description of the varied means by which Everyman courts sleep. Psychologists will read with greater interest the two chapters which review attempts that have been made to measure sleep and the theories which have sprung from these attempts. Those who are not concerned with the psychology of sleep may find reason for reading the volume in Jacobson's treatment of the relationship between muscle tension and various types of imaginal activity. Certainly all psychologists will recognize that Jacobson has not failed to use his literary device to take certain covert digs at some of his contemporaries. Even the dullest reader will not fail to mark the author's persistent effort to disclaim any connection between the effects of progressive relaxation and those of suggestion.

University of Maryland JOHN G. JENKINS

New Trends in Group-Work. Edited by J. LIEBERMAN. New York, Association Press, 1938. Pp. xii, 229.

Of more interest to educators and social workers than psychologists, this book is a collection of papers from varied sources. The two avowed objectives of the book are to encourage "self-critical thinking and experimentation" in the field of group-work. The latter objective apparently is considered important by the editorial board for they say, when seeking appropriate contributions, "we were astonished at the almost complete absence of material describing experimental group-work efforts, and we found very little material on group-work processes in activity and group-relationships." The implication would seem to be that all of the studies on the social behavior of children lie outside of the field of group-work. The social psychologist, however, may well feel that this is a rationalization for the "inspirational" approach to problems in the field rather than one of controlled observation.

If the contents of this book are a sample of the efforts of group-workers, "self-critical thinking" among them is certainly a necessity. This may be exemplified by a low point in the book where one author, after what is apparently intended to be a description of Pavlov's experiments on the comatose state induced in dogs during trace conditioning, asks the rhetorical question "Are we not justified, then, in concluding that all frustration (*sic*) tends to create corresponding inertia and that normal functioning is thus interrupted?" The outstanding article is one by G. B. Watson entitled "Virtues versus Virtue," a truly critical analysis of procedures in character education. In the reviewer's opinion this article should be read by every one working in the field of character education.

University of California EDWIN E. GHISELLI

Some Factors Affecting Resumption of Interrupted Activities: By Preschool Children. By EVELYN KATZ. Minneapolis, University of Minnesota Press, 1938. Pp. ix, 52.

This monograph reports the results of the author's experiments on the resumption of interrupted activities by children. The experiments are notable for the extreme care exercised by Dr. Katz in controlling the conditions surrounding the original tasks and the presentation of the test situations. The study is mainly concerned with the effect on resumption of the stage of the original task at the time of interruption, and on the presence or absence of alternative tasks. The monograph is well written, and the comments are cogent. The student interested in this topic will find the introductory remarks concerning past work, and the conclusions especially interesting.

University of Alabama F. NOWELL JONES

La function de l'imagination. By RENÉ LACROZE. Paris, Boivin et Cie., 1938. Pp. 159.

This is the sequel and companion-piece of the author's longer treatise, *L'angoisse et l'émotion,* in which was advanced the not un-Spencerian thesis that discomfort, disquietude, or uneasiness is the primordial root of all emotion. Disclaiming any intent to revive faculty psychology, and assuming the functional approach to be complementary to the structural, Lacroze defines function as the orientation of the organism toward a definite end, through some more or less stable psychological synthesis. The problem as to whether imagination is purely a gratuitous operation, subserves explanatory or practical needs (as in myth or magic), or has merely an autistic function, is then attacked.

In a long argument, running through eight chapters, and illumined by citations ranging from Comte, Lang, and Lévy-Bruhl through Janet, Ribot, and Bergson to Claparède, Dumas, Sartre, Dalbiez, and the less extravagant disciples of Freud, Lacroze supports (rather than arrives at) the conclusion that the function of imagination in dreams, in religion, in metaphysics and in art is purely subjective and personal, the fortifying of the inner self against the sense of human impotence. Art and religion, in this view, afford temporary respite from anticipated and inevitable failure, by way of mass emotion, or brief illusion of the vulnerability of material forces to human need, through prayer or magic.

The exaggerations of imaginative function and the disorders to which this may lead are touched upon, though a contribution to normal rather than abnormal psychology is obviously the aim. In the chapter on Symbolism there is reference both to Freudian theories, and to the French symbolist poets; to the relation, also, of imagery to thought.

Much of the argument is on the a priori or arm-chair level, revealing in the opinion of the reviewer little intimate knowledge of primitive man or the child mind. The social function of imagination, its interpretative aspects, its rôle in invention, its daily service in the enrichment of life, are minimized or overlooked. The task of imagination is defined in terms of the thesis of the earlier volume, the fundamental disquietude of existence, for which it serves as anodyne. According to Lacroze, the primary service of imagination is the fictitious realization of desire, offering salutary outlet to the many impulsions, spontaneous expression of which is incongruous with a complex civilization. The complete story of the imaginative functions is obviously yet to write.

These twin volumes serve, however, a secondary purpose. Not only does their prevailing pessimistic tone illume the 'introvert' temperament of the author and certain of his countrymen, notably Pascal, Renan, Maurice de Guerin; it reflects also the disturbing implications for the life and aspirations of the individual of the present European scene.

Though this second monograph is a contribution to theoretical rather than empirical psychology, the breadth and sweep of the discussion, in fluent and melodious prose, offers refreshing contrast to much of the merely expedient and mediocre product of the psychological press. The typography of the volume is excellent. A bibliography of 142 titles and a helpful index of citations is appended.

Cornell University ELSIE MURRAY

Psychology of Child Behavior. By ARNE S. JENSEN. New York, Prentice-Hall, 1938. Pp. xxi, 664.

A recent survey of the teaching of psychology in state teachers colleges indicates an interest in a greater emphasis on the growth and development of children, adaptive and adjustive behavior, and techniques for the control and modification of behavior. Jensen's book on child psychology is an out growth of class experience and reflects this interest and trend in the table of contents. After an historical introduction in Part I he devotes Part II to an examination of the child as an organism with chapters concerned with physical, motor, and mental growth, heredity, native equipment, learned responses, and individual differences. In Part III he treats of the child as a social being in the expressive arts, play, intellectual life, the emotional life, behavior problems, and group life. Part IV reflects principles of child guidance, including a discussion of mental hygiene, personality development, and the hygiene of behavior. A final division (V) gives an orientation of the child in relationship to scientific fields and to the future. The method of organization involves some duplication of theme in Parts III and IV.

Planned primarily as a textbook, the treatment is elementary in style and content as contrasted to recent books in the field. For example, Chapter IV on Physical, Motor, and Mental Growth is presented without a statistical table, graph, or reference to a recent scientific journal. Suggested reading and references are to other books rather than to current periodical literature. The elementary nature of the book is reflected in its list of general references which emphasizes material from popular publications, such as *American Mercury, Ladies' Home Journal, Atlantic Monthly, Science News Letter, Good Housekeeping,* and *Time.* Jensen does not utilize directly current material from such organizations as the Society for Research in Child Development or place much reliance on technical psychological, medical, or biological journals. Journal writings are sometimes quoted by secondary reference to other textbooks.

The book will probably find some use in institutions offering courses in child psychology without prerequisites for students who will not be expected to develop an interest in current research.

University of Michigan WILLARD C. OLSON

Prolegomena in Psychologiam. By GIACOMO VAN DER VELDT. Rome, Libreria del Collegio di S. Antonio, 1938. Pp. 308.

Father van der Veldt, Professor in the *Ateneo Pontificale di Propaganda Fide* at Rome, belongs to the Minorites, an Order noted for an interest in the problems of psychology which extends back to the Middle Ages—to Robert of Lincoln, Roger Bacon, St. Bonaventure, Duns Scotus. His book offers, in Latin, a Preface to psychology; since Latin is still an international language in schools of the Roman Catholic clergy, the book may well enjoy considerable popularity. The choice of Latin, however, has brought the author difficulties; on occasion, as he says, he has had to sacrifice Latinity for clearness, and to borrow words from the modern tongues. Thus we are prepared to meet neologisms like 'behaviorismus,' 'functionalismus,' and 'configurationismus' (*Gestalt*).

The volume, intended as a manual or a book of reference for students, is divided into four chapters. The first discusses in some detail the aim and scope of psychology, and in an Appendix treats, from both the doctrinal and historical points of view, the relation of the natural sciences to psychology. The second presents an historical survey of psychological thought from the Greeks to the present time, but not neglecting primitive and Oriental, psychologies. The treatment here is perforce brief. Plato receives only seven lines, Patristic and Mediaeval Psychology five pages, Experimental Psychology in America six pages. The inclusion of a section on history in a textbook on psychology the present reviewer counts as a virtue.

Chapter III describes the manifold divisions of contemporary psychology, distinguishing the systems according to aim, scope, and method, and in addition considers the special fields of psychology. In order to illustrate the author's method of subjoining brief appraisals to his descriptions and expositions I translate two short sections on psychoanalysis: "*Merits:* It has contributed much to our knowledge of the processes lying hid in the psychic depths. Not only does it inquire formally into desires, longings, and instincts, but it also reveals their real, individual aspect. Rightly and deservedly does it extol psychic dynamism, and lay bare the flow of instincts into the phenomena of consciousness. For psychiatric studies it proves exceedingly fertile. *Contra:* Though it may truly make numerous observations of a most subtle kind, yet many of its claims are gratuitous, and even more of these are greatly exaggerated. Its sexual theory concerning the importance of the instincts in the psychic life is surely exaggerated, and in a number of points has by no means been proved. The applications it makes in the fields of religion, theology, pedagogy, and art, resting as it does on the doctrine of sublimation, are sometimes impious and perilous, and often ridiculous." The final chapter studies methods, general and special, including measurements.

The work has an orderly arrangement. The interpretations are clear, and the criticisms, despite the dangers involved in simplification, are generally fair. A selective bibliography is affixed to individual sections. In the General Bibliography I miss Bentley's *Field of Psychology*; and in the list of periodicals the *American Journal of Psychology* is listed under its former editorship.

Cornell University HARRY CAPLAN

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By F. NOWELL JONES, JR., University of Alabama

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